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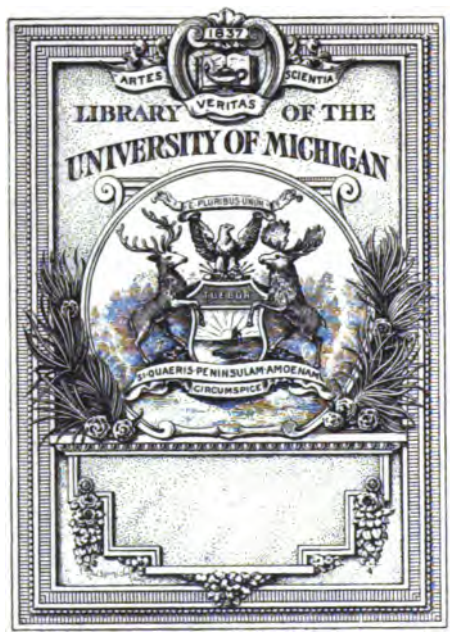
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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

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PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON FEVER.

By Dr. WM. STOKES, Regius Professor of Physic in the University of Dublin.

[IN our last volume will be found some very valuable remarks on this subject by this eminent physician. In those remarks we could not perceive much that was different from the opinions of other eminent practitioners both in London and elsewhere; but on one subject Dr. Stokes, and also the late Dr. Graves, seem to differ materially from English physicians, viz., on the influence which malarious poison has in generating fever. Dr. Stokes very plausibly alleges, that if filth and all its concomitants are such fertile causes of fever, it ought to be constantly, and not periodically, present in most districts of Ireland, as all are alike afflicted with these physical causes; whereas fever is only present periodically, and will appear at the same time in Dublin, Cork, Galway, Limerick, and Belfast. This shows that some mysterious atmospheric cause is at work: but that filth, in the form of sewers, stagnant pools, privies, &c., &c., is not the originating cause. The mysterious atmospheric cause no doubt finds a good assistant in malarious poisons, and it becomes very questionable whether the atmospheric cause *alone* would generate fever. Dr. Stokes says]

With respect to those causes which may be held to be capable of exciting fever, irrespective of contagion, I have only to say that as yet we know but little about them; and we have the highest authority for believing that the origin of epidemics is *one* of the most obscure and difficult subjects in the whole range of physical inquiry. But I cannot help expressing my belief that too much stress has been laid upon the effects of miasmata resulting from imperfect drainage or the want of ventilation and of public cleanliness in general. No one will for a moment suppose that I wish to teach that these influences do not act in deteriorating the physical health and moral condition of a people, and in thus increasing the mortality of any existing epidemic, and that their removal is not an imperative duty of every government and community; but the question before us is, are these influences in this country the sole or the chief causes of typhus fever? I do not believe that they are, because in Ireland, not only in the isolated dwellings of the poor which are scattered over the face of the country, but in the towns also, all those causes which result from the imperfect drainage of dwellings, from the accumulation of decomposing organic matters in their vicinity, and from imperfect ventilation, are, I regret to say, but too constant and too general; and yet the production of fever, whether sporadically or epidemically, is inconsistent and irregular in the highest degree. Why should these causes produce fever at one time and not at another? Why should districts remain for years free, or comparatively free, from fever, while the supposed exciting causes remain in full force? or, again, why, if the cause be constant, should the epidemic character of the fever vary? We may say, excluding the consideration of isolated cases, that each epidemic has a special or predominant character; thus, the great epidemic of 1827 and 1828 was very different from the epidemic which preceded it in 1818, and those which followed it in 1836 and in 1847. It was in the epidemic of 1827-8 that we observed the almost universal prevalence of the

secondary disease of the intestine; perforations of the intestinal tube were common; and yet, since that period, such an accident is rarely met with, either in the fever itself, or during the period of convalescence. It was at that time, too, that those singular and fatal cases of yellow fever to which we have before alluded occurred intercurrently. In this fever, too, termination by well-marked crisis was commonly observed, a circumstance which is comparatively rare in the epidemics of maculated typhus in this country.

I do not put forward these views as in any way original, for Dr. Graves long held the opinion, and taught it in this theatre, that something more than the effect of local causes, as the term is commonly understood, was necessary to explain the occurrence of epidemics in Ireland. Let me read you a quotation from his seventh lecture, one of those devoted to the consideration of fever:—"That fever, in Ireland at least, depends on some general atmospheric change which affects the whole island simultaneously, independent of situation, aspect, height above the level of the sea, dryness or moisture of the soil, or any other circumstance connected with mere locality, is proved by the fact, that, when typhus begins to increase notably in the Dublin hospitals, we may always rest assured that a nearly simultaneous increase of fever will be observed at Cork, Galway, Limerick, and Belfast. For a considerable period there was a great tendency among the physicians to refer the origin of typhus, and almost every variety of fever, to malaria, or unwholesome emanations from the soil, produced by the decomposition of vegetable matter. In Ireland, facts do not bear out this hypothesis, for, as already stated, when an epidemic of fever has become established, it breaks out simultaneously in situations the most different, and in some where no such emanations can be supposed to exist; thus I have seen a whole family affected in the telegraph situated at the summit of Killiney, a mountain formed of hard granite; and, indeed, the granite districts beyond Rathfarnham, Tallaght, and Killikee, supply the Meath hospital with its worst cases of typhus." Further on he observes:—Although ready to allow the general improvement of the health of the public from improved drainage, improved habits of cleanliness, and increased comforts, yet I cannot admit that in Ireland we are to expect any notable diminution of fever from the operation of these causes. In making this statement, you are aware that I am opposing the usually prevalent opinion. The grounds for my dissent have been partly explained to you already, for, according to my observation, the increase or diminution of fever in Ireland arises from some unknown general atmospheric or, if you will, climatic influence, quite independent of locality, and consequently, the most improved and thoroughly drained towns and country districts are quite as liable to epidemics of typhus as are the most neglected and marshy parts of our island. The causes which occasion these epidemics are, on the other hand, in no way connected with the notable variations of the seasons, for, with us, the ravages of typhus are observed sometimes in dry, sometimes in rainy seasons, and its epidemics appear quite uninfluenced by the cold of winter, or the heat of summer."

What I wish you to believe, is, as I have already stated, that our fever is both epidemic, as proceeding from general but unknown causes, and also contagious; and no one can deny that causes, which would act in depressing the health and moral energy of a people, by rendering them less able to resist the effects of disease, would increase the general mortality. The influence of bad ventilation and over-crowding I need not here dwell on; nor, on the other hand, need I occupy your time with more arguments to establish the truth of the doctrine of contagion. You will find in the writings of Dr. Christison and Dr. Graves, convincing evidences on these points; and let me again refer to the great argument drawn from the liability to contract fever which has so long been observed among the medical practitioners of Ireland.

My opinion, as I have already stated to you, is, that in Ireland, local influences have not that great importance either as generators or promoters of fever which some believe them to have, and, consequently, the deductions to be made in favour of these causes would not greatly weaken the general results. We must believe, that the causes of fever, independent of mere contagion, are various in the extreme, that they are probably numerous and complicated, acting in combination rather than singly, and varying in their effects, not only in consequence of their own properties and combinations, but also as regards the condition of the individual in whom fever is developed. To this point I have already alluded in a former lecture, in speaking

of the influence of the same contagion on individuals of different countries. And Dr. Paget, in observing that our pathology of fever is not so perfect as to assure us that there are no predisposing causes besides those which are commonly allowed, notices the comparative immunity of infants and persons above forty years of age from the typhoid fever with rose spots, and affections of Peyer's glands, &c., indicating that the constitution of the individual is an element in the question.

Let us now return to the consideration of the local diseases of fever. We have already seen that the existence of local symptoms in fever does not necessarily imply the existence of any known anatomical change. A man may have pain and delirium without cerebritis, cough and dyspnœa without pneumonia, vomiting, purging, and meteorism without enteritis. To learn this point is to advance a great way in our knowledge of fever and its treatment. On the other hand, observation during life, and *post-mortem* inspection, prove that local diseases involving alterations of structures, such as thickening, softening, vascularity, effusions, ulcerations, and gangrene, may be produced in various organs during the course of a fever.

Now, the great point in practice relating to these changes is, that they are symptomatic of the fever, secondary to the fever, and, in all probability, under the same law of periodic action as the general malady itself. They do not appear to begin with the fever, but they arise after it has existed for a certain time. There is the greatest variety in the period of their occurrence, and in their combinations; they may, as I have just now said, spontaneously subside with the fever; they may interfere with its critical termination; and, in certain cases where this does not occur, and when the general character of typhus disappears, progressive disorganization may go on in the structures which have been already affected.

In what way are we to consider the nature of these secondary affections in typhus fever? It appears now pretty well established that, in the follicular intestinal disease of the continental fevers, there is a special typhus deposit, which, at least, in the cases of recovery, is under the law, first of a progressive, and then of a retrogressive influence; just as in variola we observe the progressive maturation and absorption of the pustule. Recent pathologists have applied this principle to many other of the local diseases in typhus; and although the existence of a special deposit is not yet so well or so fully established in the case of thoracic and cerebral diseases, or at least in the case of parenchymatous as compared with membranous structures, still we may, with a good deal of safety, believe that an action more or less analogous to that which occurs in the glands of Peyer and Brunner, though perhaps not so perfectly developed, may be assumed to occur in all the secondary diseases of typhus which are not purely neurotic.

In speaking of a specific deposit in the secondary diseases of typhus, I do not wish it to be understood as intending to convey to you the impression that this deposit presents any definite characters whereby it can be distinguished from other morbid products; for, even with the aid of the microscope, we cannot discover any special distinctive characters in the deposit found in the follicular disease of the intestine, or in other forms of the secondary affections of typhus. It was at one time believed that the so-called "massa typhosa" did present special, distinctive, histological elements; but more recent research has shown that these views are erroneous. Still it is possible that, although we cannot determine the existence in it of any physical specific character, it yet possesses a vital specific character. Let us illustrate this by referring to two specimens of pus,—one taken from the pustule of variola, the other from an ordinary abscess; histologically they will appear similar, but that they have separate vital characters no one can entertain a doubt. But other conditions are found associated with this typhus deposit, or occurring when we cannot demonstrate its existence. Under this head we class many of those appearances which are described as inflammatory, such as redness, softening, tumefaction, &c. Some have supposed that these states indicate a reactive inflammation, and that such does often occur, especially in the intestine, is abundantly proved by the researches of Rokitansky and others. I think it, however, extremely probable, that, in certain structures or organs, as, for example, the lung, these appearances of inflammation may precede or accompany the typhus deposit, or that they may occur even without such a deposit at all; and I think it likely that the liability to actual typhus deposit is much less in the lung than in the intestinal glands, and greatly less in the brain and nervous system generally than in either the thoracic or the digestive systems. Were it

otherwise, typhus and typhoid fever would be much more fatal diseases than they are, death being produced in the one case by coma, and in the other by asphyxia.

But, even though he may not find, upon a close dissection, anything beyond the recognised appearances of inflammatory action spread over the surface of the mucous membrane, we are to believe that this condition, just like the typhus deposit, is secondary to the fever, and partakes of its specific character. You are not to believe that it is a simple idiopathic inflammation, nor is its existence to lead you into the deplorable errors of the antiphlogistic school, that Sangradoism of the nineteenth century which numbers more victims than the sword. This condition, however, although specific and secondary, still has some characters of irritation, or inflammation if you will; and this fact points to the adoption, in certain cases, of a practice which, to the mere theorist, appears inconsistent, namely, the use of local depletion to relieve the suffering organ, while, at the same time, we employ a general tonic and stimulating medication.

Let me, in conclusion, indicate to you in a general way the *principal pathological conditions* with which, in the treatment of a case of typhus fever, the practical physician has to deal.

1. We have the neurotic conditions, that is to say, conditions of functional derangement occurring without any known anatomical change. They may occur in relation to any of the organs; but they are, perhaps, more important, and certainly more frequent in the case of the nervous, than in those of the respiratory or digestive systems.

2. We meet with intercurrent and secondary irritations or inflammations affecting principally the mucous membranes, less frequently the parenchymatous structures, and still less the serous surfaces. These irritations, although not associated, so far at least as we can discover, with any special deposit, are, nevertheless, of a specific character, and are under the influence of the laws which govern the general disease.

3. We have the condition now specified associated with, though probably for a short time preceding, the typhus deposit, just as we see in variola the vascularity of the skin to precede and accompany the vesicular eruption.

4. We may observe the typhus deposit occurring independently of any other alteration, and apparently not associated with any irritative process.

5. We observe the re-active inflammation supervening on the typhus deposit, which has been so well described by Rokitansky, and which may produce progressive ulcerations or even mortifications of the affected parts.

Lastly, there is a condition of softening of organs, upon the nature of which we have, as yet, no very definite ideas. In some instances, it seems to be produced by an interstitial deposit of typhoid matter; while in others it is difficult or impossible to prove the existence of such a deposit. This state of softening, as we shall presently see, is one of great practical importance. Its influence upon the heart, as you all know, is most remarkable. Its existence in that organ demands special attention, and there is good reason to believe that the same state, when affecting others of the involuntary muscles, such as those of the air tubes, and perhaps also those of the intestinal canal, has an important influence on the symptoms and progress of the disease.—*Med. Times and Gazette, June* 24, 1854, *p.* 639.

[In Dr. Stokes's lectures the reader will find an ample store of valuable practical remarks, but none more so than his directions to *feed* a fever. The following opinions about the actions of the heart, and their guide in the administration of stimulants, are most valuable. He says]

What we have established as to the state of the heart in connexion with the effect of stimulants, is simply this,—we have ascertained that the efficacy of stimulants is often directly as the debility of the heart. It has been also ascertained that the power of bearing stimulants, their effect upon the nervous system, their good effects on the general condition, are directly as the weakness of the heart. We may lay down as a rule, that there are three conditions of the heart to be looked at by the practical man in the treatment of fever. In one, we have an excited heart—a violently excited heart all through the case; and this heart may be excited and violent, although the symptoms be those of extreme adynamia, although the surface be cold, the breath cold, and the pulse so feeble that it cannot be discovered. Nay, the heart may act with great force for several days, and yet there be no pulse at the wrist. This is one case. In the next case, we find exactly an opposite condition

in which the systolic force of the heart is diminished. This is shown by loss of impulse of the heart, by diminution of the first sound, and, in certain cases, by extinction of the first sound of the heart, while the second remains. This is a case which calls for wine, and in which you should give it: it is a case in which, in the vast majority of instances, wine will agree with the patient. There is a third set of cases in which the heart does not seem to be implicated at all in the course of the disease, in which, notwithstanding the existence of the most extraordinary group of symptoms affecting various organs, the heart, in the middle of the storm, seems to be in a state of calm and quiet. If we compare these three sets of cases with a view to prognosis, we may arrange them in this way. The case of excited heart all through, with feeble pulse and with adynamia, is unquestionably the worst case. There is no worse symptom in fever than an excited heart. It is especially a bad symptom when, with that excitement, we find a feeble pulse. The next will be the case of sinking of the heart; and the most favourable case is that in which, as I said before, the heart seems to escape disease. But you are not to suppose, that because you have an excited heart you are not to give wine if the symptoms of the patient require it: and you are not to suppose that, because the heart is not affected at all, you are to withhold wine if the general symptoms of the patient require it. You are not to found your exhibition of wine or stimulants upon any one thing; you are to take the general state of the patient into consideration. What we have done is to discover an intelligible practical rule which will guide you in the use of wine in certain, I think in many cases; but you are not to suppose that because this man has a clear first sound at his heart, therefore you are not to give wine. You are not to suppose that because the heart is safe you can do without wine. Now, in the case of Hardcastle, recollect that although his heart seemed to escape, or was at most only feeble through the course of the disease, what frightful adynamia existed; how for day after day the patient's face was Hippocratic, or almost so; how the general character of the disease was that of the most terrible putrescent fever; yet his heart escaped. And here is the result. We have given that man upwards of twenty bottles of wine and twenty-four ounces of brandy, and now, on the twenty-eighth or thirtieth day of the disease, we have the satisfaction of feeling that his case may be set down as among the triumphs of medicine. I wish also strongly to impress on you the great importance of the use of other forms of nourishment in this disease; for we must not only keep up the nervous energy of the system by wine, but we must support nature by food. There is no mistake more fatal in fever than the withholding of food. I was early taught the importance of the use of careful nourishment in fever by my friend and colleague, Dr. Graves. I remember once, Dr. Graves, when speaking of the necessity of the use of nourishment in fever, made use of these words,—"if you are at a loss for an epitaph to be placed on my tomb, here is one for you,—'*He fed fevers.*'" In addition to the prejudices with which the inflammatory doctrine imbued so many minds, with respect to the use of food in fever, there was a new set of arguments raised against it, in consequence of the experiments of an American physician. I allude to the case observed by Dr. Beaumont, and so often quoted since. In this remarkable case, various medical substances and articles of food were introduced through an external fistula into the stomach, their effects being noted, as also the conditions of temperature, vascularity, &c. A set of results were subsequently published in connexion with the action of the stomach upon food. One of the results stated to have been thus obtained was, that the existence of the state of fever altogether suspended the process of digestion. Here was a statement which had the appearance of being the result of strict observation. It influenced a number of young men; but did it influence those who had once been in charge of a fever hospital? Not at all; because those men knew very well that, no matter what Beaumont might say about the stomach not digesting when the patient had fever, in thousands of cases patients in fever digested remarkably well, required food, and derived benefit from it. In a large number of cases of typhus fever, the stomach has an excellent power of digestion; and, I believe, if we were bold enough, we should find that many articles of food usually forbidden to fever patients might be given with safety. A curious incident was related to me which shows that the stomach in fever is capable of digesting even a rather coarse article of food. A lady who had been recently married was attacked with extremely severe petechial fever; she was covered with dark

coloured maculæ, and the disease had run to about the twelfth or thirteenth day. She was attended by several eminent physicians. Her case was an extremely bad one, and her life was all but despaired of. She was violently delirious. Her husband had occasion to leave the house on some business. At the period of the dinner-hour of the family, the servants were cooking a rump of beef and cabbage, and the odour of it filled the house. In her delirium she called for some of the beef and cabbage; she was then, you must understand, in severe fever, and covered with maculæ. Her sister, who was attending her, believing she was dying, thought it only right to indulge her, from the feeling that it was right to indulge the request of a dying person. She proceeded to the kitchen, and, as soon as the beef was boiled, cut a very large mess of beef and cabbage; and this was brought up smoking hot to the lady's bedside, when she devoured it with great avidity. Shortly afterwards her husband came in, and was told what had happened. He became terrified, and sent for physicians in every direction. Four or five assembled; time was pressing, and every one agreed that something should be done. At length the late Dr. Harvey, a practical physician of the very first class, arrived. He was laid hold of by the agonized husband, forced up stairs, and his opinion earnestly requested. At that time the stomach-pump was not in fashion, but every one agreed that something decisive should be done, that an emetic should be given, or some extraordinary effort made to get this mess of beef and cabbage out of the lady's stomach. When Dr. Harvey went to the bedside, he found the patient in a tranquil sleep. He turned round, and when anxiously appealed to what should be done, he said—"You had better wait till she wakens; let her sleep it out." She slept for four or five hours; awoke wonderfully better, and on the following day was out of danger. I do not give you this case to induce you to feed your patient with salt beef and cabbage in fever; but it is very important, as showing that in typhus fever, with maculæ, the stomach is capable not only of digesting such a coarse article of food as salt beef, but that even such food may have a good effect.—*Med. Times and Gazette, May* 20, 1854, *p.* 508.

[Dr. Stokes makes some valuable remarks in these lectures on fever respecting some of the *local* or *secondary* diseases occurring. He observes that these vary in different countries, as to their seat and probably as to their nature. In France, for example, these secondary diseases attack chiefly the intestines. In Ireland, and probably in England, the pulmonary organs are more liable to these local diseases than the intestines, except perhaps in the autumnal months, when the alimentary canal is certainly very liable to attacks. In hospital practice, we find that pulmonary are more frequent than cerebral or nervous symptoms; and, moreover, Dr. Stokes finds that in hospital patients attacked by pulmonary affections in fever, wine and stimulants are far more efficacious than in cerebral or nervous attacks among the wealthy.]

In the typhus fever of the upper classes in Ireland the nervous symptoms are generally much more aggravated and developed at an earlier period; and it may be that this preponderance of the nervous symptoms, this tendency to affections of the brain in one class, even though these affections be principally neurotic, is a cause of the comparative exemption of such cases from the secondary bronchial disease. You will find as you advance in the study of general pathology, plenty of examples in which diseases of structure or of deposition are suspended or replaced by purely nervous affections. Explain it as you will, the general proposition appears true, that the nervous symptoms, comparatively speaking, are but slightly developed in the fever of the lower classes, while those indicative of bronchial disease are much more prominent; and conversely, that in the upper ranks symptoms indicative of irritation of the mucous surfaces are less developed, while the nervous symptoms are severe; and this, perhaps, may throw some light upon the doctrine which has been long held by many, that the mortality of fever is greater in proportion as we ascend in the scale of society.

I have already told you that, as regards prognosis, the preponderance of the nervous symptoms in fever should cause us more apprehension than that of any other class of symptoms, and especially when this preponderance is observed at an early period of the disease. This greater susceptibility of the nervous system in the higher classes may itself be a cause why we cannot use wine with as great liberality in this class as with ordinary hospital patients. And this, again, may explain the

greater fatality in private practice, inasmuch as that, in consequence of the excitement of the nervous system and the incapability of the patients to bear stimulants largely, we are debarred from the use of that which is the great medicine for fever.

I think we may lay it down as a principle, that the more the secondary affections of fever are anatomical, the greater will be the utility of stimulants; and conversely the more they are neurotic, and especially in cases where these neurotic symptoms are more closely related to the cerebro-spinal centres, the less will be the efficacy of wine. It might be supposed, from *à priori* considerations, that the man in the higher classes of life who has been accustomed to the use of wine, would require it more and derive greater benefit from it in the course of a fever, which it must be remembered is a disease of debility and prostration, than the peasant who has not been habituated to the regular use of stimuli; but I believe that the very reverse is the fact, and that the greatest triumphs of the stimulant treatment in fever, are to be found when it affects the agricultural peasant, whose nervous system has not been excited either by the use of stimulants or by intellectual exertion. I know that some will object to this on the ground that our Irish peasantry are habitually intemperate; but this charge of intemperance is only one of the many calumnies that have been put forth with reference to our countrymen.

The Irish peasant is not habitually intemperate; neither do his means permit nor his inclinations direct him to be so. When he does indulge, it is on special occasions; but you seldom meet in hospital a case of injury to the general health produced by a long course of intemperance in the agricultural peasant. The case is no doubt different in the class of artisans who are the inhabitants of our large cities and towns. In this class we find, that when fever attacks an individual we can neither use stimulants with the same boldness, nor reckon on their success with the same confidence as in the more sober portions of the population. In this respect the Irish are not peculiar.

To a person who is not in the habit of examining patients very closely, the frequency and importance of the bronchial disease would not appear very evident or striking as he walked round the wards of an Irish fever hospital. We find the patient lying in bed; he is, perhaps, in a state of stupor, but from this he is easily roused. He is covered with petechial spots, red or livid, as the case may be. There is nothing remarkable about his breathing; he is not reported to have any cough; his pulse is probably 120, or between 120 and 130, and his heart is weak. The patient, with but little hurry in breathing, not complaining of any distress, and without cough, may be, and frequently is, at that very time, almost in a very advanced stage of the bronchial disease. When you apply the stethoscope, you are surprised at the great amount of bronchial disease it reveals, although the symptoms of this important affection are absent. One would expect, that in such a case there would at all events be lividity of the countenance from the non-arterialization of the blood; but here is one of the curious things connected with fever which it is extremely difficult to explain,—that you will often see but little lividity in such cases. At all events, it is not sufficient to draw the attention of the uninformed practitioner to the condition of the patient's lung. In most patients labouring under bad typhus there is a peculiar dusky hue of the face; and this he would have whether the bronchial disease were present or not. But I have over and over again seen the most extensive bronchial disease, where every tube seemed to be half-filled, and yet little of the kind of lividity which we see in a case of asthma or of bad suffocative catarrh existed. Consider this fact for a moment, for it is full of matter for reflection. What does it tell you? It announces to you this highly-important principle, that in their formation, and in their progress, the secondary diseases of fever are, as it were, silent. They occur without the usual symptoms which are observed in idiopathic inflammations. They are not idiopathic inflammations, and therefore they have not the symptoms of them. Let us omit no opportunity of impressing this on our minds, that these diseases are not inflammations. They form silently—they advance silently. Consider for a moment the formation of a pustule in an ordinary case of small-pox. The pustule gives no notice of its formation. You turn down the clothes, and you find the arm covered with vesicles; the patient has not complained, but you find there is the disease. So it is with respect to the bronchial disease in typhus; so it is with respect to the intestinal disease in typhus.

We see the bronchial disease forming in this singular and latent manner, without excitement or suffering of the patient. We see the same in the intestine; we see the same in the heart. The softening of the heart, as you will have abundant opportunities of seeing this season, is, in its commencement and all through, a silent process. The process of change up to its maximum alteration, and the process of retrocession up to the period of recovery, considered simply, when they are not interfered with by accidental irritative inflammation, go on silently. The disease may proceed to its maximum without symptoms, and retrocede without symptoms. Now, under these circumstances, I have told you, that, when you apply the stethoscope, you find singularly extensive râles. These râles may be sonorous, mucous, or crepitating; the amount of each of these characteristics varying in every case, and in different portions of the lung, in the same individual. When you use percussion, you find that there is no remarkable dulness anywhere. I have not observed, at least in the bronchial affections of typhus fever, the curious result of percussion which we see in other cases of bronchial disease. We have not observed any increase of sonoriety of the chest. This is a very remarkable fact. In a large number of cases of acute inflammatory bronchitis, you observe, that, so far from the chest being dull, it is actually clearer than natural; and this is explained by the circumstance, that there is a great accumulation of air in the air-cells of the lungs,—that, in fact, every little air-cell of the lung is in a condition of extreme distension; it is full of air, and this air cannot find a ready egress during expiration, in consequence of the state of the bronchial tubes; and hence we have the increase of sonoriety. I have not observed this in cases of typhus fever where there was no consolidation of the lung; but, at the same time, it is to be noted, that we have not specially studied this point in fever cases. When the patient draws a deep breath, we frequently find that the râles go on increasing up to the very end of the inspiratory act; and in certain cases the following curious circumstance is found,—and it is one of those which illustrate what I have termed the silent action of the disease—that on applying the stethoscope you will find perhaps but little râle, or a loose rattle in the larger tubes, or perhaps in some of the secondary bronchial tubes,—that is, during ordinary breathing. If you contented yourself with such an examination, you might come to the conclusion that there was very little the matter with the patient's lung—slight mucous disease in the lower part of the trachea, or in the very large tubes. But you may be altogether wrong; for, under these circumstances, it frequently happens, that when a patient draws a deep breath, you are startled with the extent and intensity of the râles running to the very end of the respiratory act. It is in this condition that the patient's life is seriously endangered; for if this bronchial disease does not retrocede, but continues to advance, it may happen that the patient will suddenly fall into a condition of asphyxia, tracheal rattle comes on, and he dies a mechanical death—in fact he dies suffocated. You may leave your patient apparently going on very well, and be summoned to him in a few hours, and find him *in articulo mortis*. This is a common thing, as I mentioned to you before, in patients in whom the bronchial disease has been overlooked and neglected.—*Med. Times and Gazette, July* 22, 1854, *p.* 77.

As a general rule, in prognosis, the occurrence of any vesicular eruption whatsoever in fever is bad. We have in fever several forms of vesicular eruptions. But, suppose a patient in the eighth or tenth day of fever, when he is otherwise going on well, you take his hand to feel his pulse, and you are surprised at seeing a vesicle upon his arm. Do not neglect that—do not overlook it. The mere circumstance of that solitary vesicle forming so silently, without any pain, without any notice, points out that there is mischief before you—that the case is likely to go wrong. It is a very important prognostic. Here we have on this patient an eruption of vesicles running rapidly to deep gangrenous destruction of the part, for some of the cavities formed by them are singularly deep, though circumscribed. Now, suppose that in place of that disease attacking the skin, upon the fourteenth day, on the tenth day, or whatever day you please, in any case, a similar condition should attack the intestine. Suppose that the typhoid matter suddenly infiltrated a gland in the intestine, and that that should fall as suddenly to gangrene, and that there was a solution of continuity; you will at once see the progress of the worst form of the typhoid secondary disease of the intestine. You may look on this man as at this moment turned inside out, and that on the surface of his skin you are able to learn the his-

tory of the worst form of the typhoid ulceration of the intestine. This extraordinary condition of his surface no one will say is inflammation of his skin. Apply the same view, and will you say, that the disease of the intestine is inflammation of the intestine? Certainly not, Whether this man is to live or to die, I believe none of us can venture to say. Of course, the chances are enormously against him; but in the treatment of fever, you are never to despair so long as the patient can swallow. So long as he is able to take nourishment, so long as he is able to swallow wine, no matter how dreadful or apparently hopeless the symptoms may be, you are not to desert him, but—to use the phrase of our glorious sailors—you are to fight the ship whilst she swims. In a disease under the mysterious law of periodicity, every hour of compelled life is a clear gain. And, over and over again, you will find that your efforts will be crowned with success. You will see a patient lying with his back icy cold; you will see him pulseless,—his lungs filled up with secretion,—his belly tympanitic,—with dreadful diarrhœa,—the lower extremities gangrenous,—himself in a state of insensibility,—and yet, even under those circumstances, a recovery is possible. But that recovery can only be effected by the steadfast determination of the physician not to desert his post until the vital spark has actually fled; and, if you commit an error in holding on,—in hoping against hope,—at all events it is an error on the right side.—*Med. Times and Gazette, April* 29, 1854, *p.* 424.

2.—ON THE QUININE TREATMENT OF YELLOW FEVER.

By Dr. Wm. J. Cummins, formerly Surgeon Royal Mail Company.

[The treatment of yellow fever in the West Indies by large doses of quinine has been lately very successful, when done boldly and with promptitude, but the reverse when delayed too long.]

Yellow fever, as well as all other fevers, presents a striking distinction between the phenomena both of its second and third stages in different cases; for example, in one patient we may find, on the second day of the disease, a feverish condition, and in another, at the same period, the very reverse: in one, symptoms of inflammation of the gastro-intestinal mucous membrane; in another, a perfect freedom from indication of any such affection: in one, suppression of urine; in another, this secretion is copious, although of low specific gravity; in one, a dry skin, in another a moist; in one a yellow skin, in another the natural colour; in one hemorrhages of various kinds, in another none, &c.

Thus we find that the phenomena of this fever are by no means constant, and are frequently opposite conditions. M. Louis has proved that the same remarks are applicable to the anatomical changes discovered post-mortem. How, then, can any specific for such a disease exist, unless it possesses opposite powers?

The answer to this question can alone be:—there are two principles of treatment for fevers; the one founded on our knowledge that these diseases tend to a spontaneous cure, or in other words, that the fever poison will wear itself out of the system, and terminate the disease by what we call crisis at a certain period, and consisting of remedies applicable to the effects of the poison, or the indirect symptoms produced by it, so as to retain life, and prevent irremediable local change, until crisis has arrived, and the fever has terminated of its own accord: the other is the specific principle of treatment, which assumes that all the inconstant and opposite phenomena are effects of a general cause, which is itself the first effect of the poison applied from without by contagion, or atmospheric contamination, and on the removal of which, secondary and tertiary effects frequently cease of their own accord.

Of these two principles of treatment, the former has been found very efficacious in the fevers of temperate climates (with, perhaps, the exception of congestive typhus and scarlatina), because, as these are not rapidly destructive of life or tissue, time is granted for their full use; but such is not the case in tropical fevers, and especially that known as yellow fever, where effect follows cause, and in its turn becomes a secondary cause, with such alarming rapidity.

For this reason, although I endeavour to treat both cause and effect in the latter disease, I have most confidence in the remedies for the cause. We shall see in the sequel how quinine treats the cause.

During the months of October, November, and December, 1852, I continued to prescribe quinine empirically, and my reliance on it as a specific mode of cure increased daily, so much so, that during a lull of the epidemic, I determined to take the medicine myself, in order to test its effect in large doses, frequently repeated on the healthy subject.

The first few doses produced slight giddiness, or rather, lightness of head, and a complete state of nervous irritability, so much so, that the least unusual motion of the ship, or even the sudden entrance of any person to my cabin, or touching me, caused an involuntary start; indeed the entire surface of my body presented a morbid sensibility, though without pain. My pulse slightly increased in force and frequency soon after a dose, fell below the natural standard in fourteen or fifteen hours, and a dose taken at bed-time caused restlessness and insomnia for several hours, followed by disturbed and unrefreshing sleep. I had no deafness, but palpitation and dyspnœa occasionally attacked me for a few moments at a time during the whole time I was cinchonised, and were always brought on by smoking, which I had to give up entirely. There was no irritability of the bowels produced in my own case, nor do I remember to have noticed that quinine caused it in any of my patients.

Thus, all the sensations I experienced when under the influence of quinine, induced me to suppose that in large doses it causes a direct increase of the vis nervosa, or, in other words, stimulates the vital energy of the nervous system. If such is its real action, how does it cure yellow fever? The answer to this question must be found out by studying the disease, as follows:—

1st. All agencies which cause a depression of *the vital energy* of the system, such as intoxication, fear, anxiety, improper food, deficient ventilation, insufficiently-drained residence, sudden removal to a hot climate, great heat, &c., &c., dispose the system to the reception of the poison which is the exciting cause of yellow fever.

2nd. Agencies which cause a continued stimulus of the system are prophylactic of the disease.

[Dr. Annesley, alluding to the general opinion that stimulants guard the system from the injurious action of malaria, writes:—"But the excitement thus occasioned requires to be kept up *moderately and equably*, not to reach above a certain pitch, or fall below it." He does not, however, think the action of ordinary stimulants could be so regulated.—*Tropical Diseases*, *p.* 416.

Dr. Rush (although considering intoxication a predisposing cause of yellow fever) instances several persons who were *constantly* under the influence of strong drink, and yet escaped the disease during the entire time of its prevalence, and he explains their immunity, by stating that the stimulus of ardent spirits excited an artificial fever, which defended the system from the epidemic.—*Yellow Fever of Philadelphia*, *p.* 100.

It has been remarked that the cholera poison, which resembles that of yellow fever in many of its peculiarities, though taking effect most readily on those enfeebled by disease, or other cause, rarely attacks those afflicted with a disease *which keeps up a continued febrile action.—Medico-Chirurgical Review*, *Jan.* 1851, *p.* 51.

Quinine is a most excellent prophylactic of yellow fever, and of other diseases which cause a depression of the vital energy of the nervous system; and there can be little doubt that it acts by producing a tonic stimulus of that system, and thus antagonizing the primary operation of certain poisons.]

3rd. Thus, from considerations external to the disease itself, we may draw a conclusion that the *exciting cause* of yellow fever is a depressing agent; and if so, the first stage of the disease ought to be one of depression or minus vitality.

[This opinion is supported by various eminent writers, as Stokes, Adolphus, Nash, Stephens, &c.]

In addition to these authorities, I may state, that in all the cases of yellow fever I have myself watched from the commencement, I have noticed that a stage of depression of the vital powers is the first indication of disease having commenced; that this stage varies in its duration in different epidemics, in portions of epidemics, and in individual cases occurring at the same period of an epidemic; but is always longer and better marked in this than in the fevers of temperate climates I have witnessed; that in some cases it is continued throughout the entire course of the disease, so that death appears to result from a concentrated poison having directly ab-

stracted the vitality of the whole organism. Such cases as these are described by M. Louis (Louis on Yellow Fever of Gibraltar, of 1828, translated by Dr. Shattuck, Jun.), at least he referred the death, in the third part of his cases, directly "*to a cause unequal in its operations*," as he was unable to account for it by the effects produced.

It may be well to mention here that this great pathologist, after having visited Gibraltar in 1828, for the sole purpose of making yellow fever his study, and having made many observations, and many post-mortems, came to the conclusion, that because an unknown cause of the disease is the direct cause of death in many cases, "*the rejection of empirical treatment*" for yellow fever, "*no matter how absurd, is not warranted.*"—Ibid, page 333.

Now to recapitulate a little. From a consideration of the disease itself, of its known and unknown causes, of its prophylactic remedies, &c., it may fairly be inferred that the old doctrine of Cullen is applicable to yellow fever, viz. :—that a stage of diminished vital energy is the first link in the chain of its phenomena; from a study of the action of quinine on the healthy subject the conclusion may be drawn, that it increases the vital energy of the nervous system, and is thus not only antagonistic of the operation of the poison, but is also capable of removing its primary effect, or the cause of the subsequent lesions of function and structure; and lastly, from its clinical administration, it may be said that it is practically capable of curing yellow fever.

This last point leads us on to a new subject, viz., its mode of administration; however, as some details on this point have been published in one of my former papers, in *Braithwaite's Retrospect* (vol. xxviii.), I need only remark, that as a large amount of nervous depression is present in yellow fever, much beyond the ordinary dose is requisite to insure its good effect. (The same remark is applicable to wine in typhus fever, witness the almost incredible quantities consumed by some patients, without intoxication following, and how suddenly, as Dr. Corrigan remarks, it must be left off when crisis becomes established.) I usually give from ten to twenty grains, repeated every second or third hour, and proportioned to the amount of depression present in each individual case. No rules can be laid down, but the best way of measuring the amount of depression is by its effects; general congestion is the most prominent of these,—even the heart is often congested, or its action clogged.

Early deafness, or dilatation of the pupil, are bad signs; they show a disposition to the establishment of the secondary effect of the quinine, *i. e.* the depression following the first effect of all stimulants; and large doses are more apt to produce them than smaller ones, frequently repeated.

I think a long interval (more than from four to six hours if possible) should never be allowed to elapse between the doses of quinine, and when we wish to give up the medicine, it should be done by gradually decreasing the dose, rather than by laying it aside suddenly.

Quinine may cause complete amaurosis; I had two interesting cases of this kind, which occurred almost simultaneously, and it is curious that they were both natives of the south of Europe, one of Spain, and the other of Italy.—*Dub. Hosp. Gazette, June* 15, 1854, p. 148.

3.—ON THE USE OF CHLOROFORM IN FEVER.

By Dr. Gordon, Physician to the Hardwicke Fever Hospital, Dublin.

[The fever poison sometimes acts on the brain in a remarkable manner, producing symptoms of disease both in the chest and abdomen, although those two cavities may be, in reality, free from disease. One of the most frequent characteristics of lesion of the brain is *insomnia*, causing, or followed by, delirium, subsultus, &c., and accompanied, probably, by congestion of the substance of the brain.]

There are two axioms laid down in Dr. Corrigan's late work on Fever, which should always be remembered: 1st, "the loss of sleep, if it continue, is of itself sufficient to kill;" and 2ndly, "if even the shortest sleep be procured, some advantage is gained."

To procure this sleep, then, by means which will not do injury to the circulating

or digestive powers, would appear to be the most rational mode of treating this complication, and the rapidity with which the symptoms of great nervous exhaustion subside, when once sleep has been procured, proves the soundness of this mode of treatment.

To the different means which have been made use of for this purpose, I would now add the internal administration of chloroform. I have used it with the happiest results when all other means have failed, and I can speak with confidence of its certain and speedy action. The following case, reported by Mr. RICHARD ABBOTT, affords a good example of its effects, and the mode of its administration :—

Patrick Dempsy, aged 25, was sent from Santry to the Hardwicke Hospital, on the 8th of December; he was then eleven days ill of fever; his body was covered with dark-coloured maculæ; his pulse was 110, and very weak, his speech muttering and indistinct; he had subsultus in both upper and lower extremities. His head was shaved, he was ordered the bark mixture of the hospital, and half a pint of wine. Late in the evening he began to rave violently, and could not be induced to remain in bed; he was ordered large doses of hyoscyamus, and the back of his head was blistered; he was so violent as to require the use of a strait waistcoat all night.

December 9.—Has not slept since admission. Pulse 132; very weak. He continues constantly muttering and raving. Tongue dry and brown; eyes slightly suffused; head not very hot; respiration short, frequent, and irregular. He still requires the strait waistcoat to keep him in bed. He was now ordered twenty-five minims of chloroform in a draught, to be repeated in an hour.

After the second draught his agitation and restlessness ceased, and the waistcoat was removed. He dozed a little through the day, but only for a few minutes at a time. Towards night he again became restless and delirious; the same quantity of chloroform was again administered, and repeated in an hour, when he fell into a sound sleep which continued for nine hours. He awoke perfectly sensible; the subsultus had ceased, and his pulse had fallen to 100. He continued to improve, and in a few days was convalescent.

In this, and other similar cases, chloroform acted by producing anæsthesia of the sensory nerves, and exerting a paralysing influence on the muscular fibre; and this it appears to effect without depressing or deranging the nervous force, as is the case with sedatives in general, while it is altogether free from the objection of causing depression of the action of the heart, as is the case with some special sedatives. My colleague, Dr. Corrigan, has just treated a somewhat similar case by the internal administration of chloroform. I had an opportunity of daily witnessing the progress of the case; and, by his permission, I here append it, as reported by Mr. EDWARD SARGINT :—

Dennis Beahan, æt. 20, a porter from High-street, was admitted into the Hardwicke Hospital, January 4, 1854, the fifth day of his illness.

On the sixth day he was thickly covered with bright maculæ. His tongue was loaded, but moist; his pulse 112; respiration 22; no abnormal sound in the lungs; no tenderness of abdomen. He is reported not to have slept for two nights. His eyes are red and injected, and his head hot.

His head was shaved and cold lotion applied.

Seventh day.—Pulse 116: respiration 28; slept but little.

Eighth day.—Pulse 120; very feeble; respiration 32. Ordered bark and wine.

Ninth day.—Pulse 126; very feeble; respiration 32; head hot; constantly raving, and getting out of bed; no sleep; subsultus of hands; tongue dry; great difficulty of utterance. Vesicatorium nuchæ; eight ounces of wine.

Tenth day.—Pulse 130; weak; raving continually; difficult to restrain; requiring the strait waistcoat; constant talking; no sleep; tongue brown and dry in centre; thirsty; eyes very congested; pupils dilated.

Chloroform was now administered by inhalation, without any other effect than the pulse being slightly reduced in number. The patient was in no way quieted by it. Four leeches were now applied to the temples without any good effect. At 5 p.m., he took 3ss. chloroform by the mouth, and continued it every second hour till 11 p.m., when, as he did not sleep, and the delirium continued, he got same dose of chloroform every hour through the night. At 3 a.m., he was somewhat quieter, but the same dose was continued every hour till 8 a.m.

Eleventh day, 10 a.m.—Much quieter, but has not slept. Pulse 110; pupils natural size; subsultus nearly gone; tongue brown all over; sordes on teeth; bowels free; urine high-coloured, sp. gr. 1 020. Another dose of chloroform in same quantity was again administered; about twenty minutes after its exhibition he fell into a quiet sleep, which lasted for two hours. Shortly after waking, he took another half drachm of chloroform, when he almost immediately fell asleep, and awoke after several hours, much refreshed and quite collected.

His return to health was further indicated by the immense quantity of nitrate of urea, which an excess of nitric acid deposited from the urine.

In the above case the chloroform was longer in producing its effects than in any instance in which I have as yet used it. We learn from it, however, that we are not to be discouraged by the apparent failure of the first dose or two in procuring sleep, for, as in the present case, although actual sleep may not be at once procured, we may expect that a state of calm and quietness will be induced, which will soon be followed by "*Nature's sweet restorer, balmy sleep.*" We learn also from this case, that the inhalation of chloroform is, to say the least, useless in procuring sleep in cases of cerebral excitement in fever. I had on one occasion before, in the Hardwicke Hospital, fully tried this mode of administering it; its inhalation was followed by general convulsive movements, very similar to an epileptic seizure, and I have not since administered it by inhalation in any similar case. Dr. Corrigan carefully tried the effect of inhalation three times in the above case; each time without any good effect.—*Dublin Hospital Gazette, Feb.* 1, 1854, *p.* 11.

4.—*On Gout and Rheumatism.*—In a paper on this subject read before the Royal Medical and Chirurgical Society, Dr. Garrod shews that in 177 examinations of the blood, taken from 148 separate patients, in every case of genuine gout an abnormal amount of uric acid was found, while in acute rheumatism such was not the condition.—*Lancet, July* 29, 1854, *p.* 82.

5.—*Treatment of Gout.* By Dr. Hughes, Guy's Hospital.—[A few weeks ago, a man, aged 58, was admitted under the care of Dr. Hughes, into Guy's Hospital, suffering from chronic rheumatic gout, which affected all the joints of his hands. The attack had lasted for six weeks, and was increasing in severity. He had frequently before suffered from the same disease. Dr. Hughes prescribed the following:—

℞. Extract. acet. colch. gr. i., pulv. Doveri, gr. ij.; ft. pil. ter die sumend. Pulv. Doveri, gr. v. o. n. sumend., et pulv. rhei cum magnesiâ ʒij. o. m.

In a few days the pain had quite disappeared, and the man could sleep well, which he had not done for many weeks. Subsequently the decoction of bark was ordered as a tonic, and in about ten days he was well in every respect. Dr. Hughes remarked to the pupils present that the treatment adopted was one which he almost never found fail to afford a very speedy relief in all forms of gout. He believed the acetic extract of colchicum to be quite a specific for the disease, and was inclined to attribute the failures which some met with to impurities or adulterations of the drug.—*Med. Times and Gazette, July* 22, 1854, *p.* 86.

6.—ON GOUT AND ITS COMPLICATIONS.

By T. Spencer Wells, Esq.

[In a review of Mr. Wells's work on gout, it is pointed out that the chemical changes in the blood which constitute gout are the effects of derangements of nutrition. Mr. Wells reviews the doctrines of Liebig and Prout as to the changes which take place during the disintegration of tissue. The following in an extract:]

"Liebig has offered an explanation of these disordered processes; and, according to his commentators, his views lead to the inference, that gout is 'an inflamma-

tion in parts in which the usual changes which the oxygen effects are unable to take place, in consequence of an excess of the non-nitrogenous principles in the body.' (Bence Jones, page 61.) According to these views of Liebig the acids formed in a state of health from the amylaceous, saccharine, and mucilaginous components of non-nitrogenized food, are converted into carbonic acid and water, by combining with oxygen. When any circumstances prevent or diminish the supply or action of oxygen, the changes are more or less retarded, and the lactic and acetic acids remaining in the blood must be thrown off from it. This is the derangement of the first stage of assimilation. Then, during the metamorphosis of tissue, oxygen being conveyed to the worn-out tissues, combines with their elements, and forms products of which lithic acid is the principal. A further supply of oxygen and water converts the insoluble lithic acid into soluble urea and carbonic acid, so that the disintegrated tissues have their elements so arranged as to reach the excreting organs in a soluble form. This conversion of lithic acid into urea is only complete when the supply of oxygen is sufficient; and in this way non-nitrogenized food, by monopolizing a great proportion of oxygen to secure assimilation, and prevent development of lactic and acetic acid, prevents the conversion of lithic acid into urea, and the former remains in the blood in a greater or less proportion, and appears in the urine. All other causes which diminish the supply of oxygen act in the same way.

"Dr. Gairdner, as we have just seen in his recent work on gout, supports these views, which, though very ingenious, unfortunately for the theory, are completely contradicted by observation at the bedside. According to them, a vegetable, carbonised, or non-nitrogenous diet, should increase the formation of lithic acid; while an animal, or nitrogenous diet should diminish it. Yet observation proves that the very opposite of this is the case. Other things being equal, an animal diet increases, and a vegetable diet diminishes, the formation of lithic acid. Magendie found that lithic acid disappeared from the urine of carnivorous animals on restricting them to a vegetable diet. It is not present in the urine of herbivorous animals; and it was difficult to procure the converse of Magendie's experiment by making herbivorous animals take animal food,—they will not do so. Lately, however, M. Bernard thought of the ingenious, though cruel plan, of subjecting them to a prolonged fast. In this way they nourish themselves—they are fed by the absorption of their own tissues, and thus become for a time carnivorous animals. Then lithic acid appears in their urine. Dr. Lehmann, of Leipsig, proved, by ascertaining the quantity of urea and lithic acid excreted by himself in twenty-four hours, under four different systems of diet, viz., exclusively animal, mixed animal and vegetable, exclusively vegetable, and of food containing no nitrogen whatever, that a vegetable and non-nitrogenous diet diminishes, and an animal or nitrogenous increases, the quantity both of urea and lithic acid. More recent experiments, performed on ducks, by Boussingault, to show the quantity of acid excreted after the ingestion of various articles of food, confirm this view.

"These facts are sufficient to prove that Liebig's theory is unsound, and that practice founded upon it must be erroneous, and is likely to be dangerous. Some chemists also assert, that his numerical results are incorrect, and the tendency of the most recent researches is to establish the truths of the doctrine long since advanced by Dr. Prout, that urea and lithic acid are formed from the nitrogenized elements of disintegrated tissues, and from the nitrogenized food, which, being in excess, is imperfectly assimilated by the digestive organs. It is also extremely probable, though not absolutely established, that lithic acid is the special product of the disintegrated albuminous tissues, and urea of the gelatinous. This explanation is in perfect accordance with the results of practical observation, and proves the necessity of diminishing the supply of nitrogenized food when the formation of lithic acid in excess shows that the blood is receiving an inordinate supply of nitrogenized elements, either from the lacteals of the digestive organs, or from the lymphatics which return the products resulting from the disintegration of tissue."

An extract from the chapter on the medicinal treatment may be given as a specimen of the practical section of the work, and of the application of chemical discovery to bedside medicine.

"*Chemical solvents* are also very important remedies in gout. The solvents of lithic acid, or those which render it solvent by converting it into the lithates of

potass or soda, may be occasionally required and used as palliatives, while we are endeavouring to remove the state of system on which the formation depends. The Vichy water, or the artificial substitute made by Dr. Struve, is the best form for administering them; half-a-pint being taken early in the morning, and again an hour or two before dinner. The liquor potassæ is less useful than the carbonates of potass or soda. Dr. Golding Bird recommends half a drachm of bicarbonate of potass three times a day, with five grains of citric acid, which is just enough to render the solution sparkling. The patient should use diluents freely at the same time, to assist in the solution of the lithic acid.

"No means of rendering the urine alkaline, however, can be persevered in without injuring the general health, leading to muscular debility, or tendency to the formation of other urinary deposits. The borate of potass is strongly recommended by M. Bouchardat as a solvent of lithic acid. He gives fifteen grains in a tumbler of water several times daily. It is said to be less injurious than the alkaline salts just recommended, but I have had no experience of the results of its use. The phosphate of soda is another powerful solvent of lithic acid. It may be given in doses of a scruple or half a drachm, freely diluted. It even lessens the pain, and allays the feeling of general irritation during an acute attack, and after the attack has gone off, promotes convalescence. The phosphate of ammonia, recommended by Dr. Buckler, of Baltimore, in doses of ten grains three times a day, has been tried by Dr. Golding Bird. He says, 'It has always succeeded in keeping uric acid in solution in the urine, and in this respect, it has appeared to be at least equal, if not superior to borax and phosphate of soda; but I certainly have never seen it diminish the tophaceous deposits of chronic gout. In more recent effusion in the joints, in the subacute forms of rheumatic gout, it has certainly been of service.' I have, however, heard of very unpleasant symptoms following its use. Benzoic acid and benzoate of ammonia have been used with advantage, but Mr. Ure has not established his proposition that the benzoates are useful by converting insoluble lithic into soluble hippuric acid. The benzoate of ammonia is the most useful, as it has a decided tendency to promote perspiration. It is easily given by prescribing five grains of benzoic acid with an equal quantity of sesquicarbonate of ammonia, to be dissolved in two ounces of boiling water, and taken three times a day with a little syrup. But of all chemical solvents, I am disposed to regard the iodide of potassium as the most useful, as it has so great a solvent power on the lithate of soda, which is the most common impurity in the blood of gouty patients. A concentrated solution of the iodide dissolves the lithate of soda very readily out of the body, and to a much greater extent when the lithate is recently prepared and the solution is warm; but it has very little power of dissolving pure lithic acid. I have given it very extensively for the last thirteen years in almost all forms of gout, except during the acute attack, and in almost every case with the most encouraging results. I have tried it in doses from eight grains three times a day to one grain daily in divided doses. I have had patients who have continued the latter small dose for several months, and after carefully watching the effects of discontinuing its use and returning to it, I have been convinced, that the improvement in health which accompanied and followed its use, was really connected with, or dependent on, the use of even so small a quantity. It clearly affects the blood, for it may be detected in the urine; and I can only suppose that it must tend to keep small quantities of lithate of soda formed in the blood in a state of solution, although it is, I confess, difficult to see how so very small a quantity could act upon a sufficient quantity of lithate of soda to make any appreciable alteration in the condition of the blood. However this may be, the good effects are too uniform to leave any doubt as to the value of the remedy.—*Med. Times and Gazette, July* 22, 1854, *p.* 91.

7.—*Treatment of Rheumatism.* By Dr. Spurgin.—[The following case of rheumatism shows how successful the treatment may be even from a single prescription. This prescription was the following:]

℞. Liq. potassæ ℥xv., potassii iodidi gr. ii., mucil. acaciæ ʒi., aq. distillat. ʒxi. M. ft. haust. 6tâ quâque horâ sumend. Potassii iodidi ℈ii., morphiæ muriat. ℈ss., ung. cetacei ʒiss. M. ft. unguent.—Opii purif. gr. i., ext. colchici acet. gr ii., pulv. scam. co. gr. iii. M. ft. pil. ii. Horâ somni sumend.—Feb. 5, 1854.

This prescription was persisted in from the 5th of February to the 14th, with no variation but the addition of fifteen drops to each draught of the tinct. lupuli, and the cessation of the pills. On the 8th, during my attendance, the patient took two seidlitz powders, which, as they failed in their object, were superseded by two drachms of Cheltenham salts in a tumbler of warm water the first thing in the morning; these acted in a most satisfactory way, producing plentiful secretions of bright yellow bile. The ointment was applied to each joint as it became affected with pain, heat, and tumefaction, giving considerable relief, and bringing out an eruption of red and irritable papulæ. It was curious in this case to witness the shifting character of the complaint, for it would one day be seated in the left wrist, hand, and fingers, and shoulder, the next day I found it attacking the other limb, the former having but little the matter with it comparatively; it was the same with the knees and ankles; an aching of the shoulders was a distressing symptom. Palpitation of the heart on exertion, with faintness, sometimes occurred when the disorder was at its height; there was also great paleness of the countenance, with contracted nose. Previously to my seeing him he had been ill above a fortnight, better and worse. Warm baths, salines with colchicum and sudorifics, and morphia, had been prescribed, but with no satisfactory result. The urine was red and scanty, without sediment. Pulse varying from 80 to 90, and weak.—*Med. Times and Gazette, April* 29, 1854, *p.* 445.

DISEASES OF THE NERVOUS SYSTEM.

8.—ON EPILEPSY.

By Dr. R. B. Todd, F.R.S., Physician to King's College Hospital.

[Dr. Todd makes the following clinical remarks in a case of epilepsy which he considered to depend on renal disease. He says, that from the frequency of epileptic attacks depending on renal disease, he has been induced to construct a theory of these fits generally:]

I hold that the peculiar features of an epileptic seizure are due to the gradual accumulation of a morbid material in the blood, until it reaches such an amount that it operates upon the brain in, as it were, an explosive manner; in other words, the influence of this morbid matter, when in sufficient quantity, excites a highly polarised state of the brain, or of certain parts of it, and these discharge their nervous power upon certain other parts of the cerebro-spinal centre in such a way as to give rise to the phenomena of the fit. A very analogous effect is that which results from the administration of strychnine, which is best seen in a cold-blooded animal, like the frog. You may administer this drug in very minute quantities for some time without producing any sensible effect; but, when the quantity has accumulated in the system up to a certain point, then the smallest increase of dose will immediately give rise to the peculiar convulsive phenomena. The animal is thrown into a series of paroxysms of opisthotonos, which exactly imitate the phenomena which we often witness in tetanus, as it affects man and some of the higher animals.

This, then, is the humoral theory of epilepsy. It assumes that the essential derangement of health consists in the generation of a morbid matter, which infects the blood; and it supposes that this morbid matter has a special affinity for the brain, or for certain parts of it, as the strychnine, in the case just cited, exercises a special affinity for the spinal cord. The source of this morbid matter is probably in the nervous system, it may be in the brain itself. It may owe its origin to a disturbed nutrition—an imperfect secondary assimilation of that organ—and in its turn it will create additional disturbances in the functions and the nutrition of the brain. Probably, in no instance does an epileptic fit ever occur without leaving a damaged state of brain, which in some cases is permanent, in others remarkably transient.

According to the humoral theory, the variety in the nature and severity of the fits depends on the quantity of the poisonous or morbid material, and on the part of the brain, which it chiefly or primarily affects. If it affect primarily the hemispheres, and spend itself, as it were, on them alone, you have only the epileptic vertigo. If it affect primarily the region of the quadrigeminal bodies, or if the affection of the

hemispheres extend to that region, then you will have the epileptic fit fully developed.

To give a more definite character to the humoral theory, we need to discover a morbid matter in the blood, in variable proportions, in every case of epilepsy. This desideratum has, as yet, been only partially obtained.

The clue to a discovery of this kind was first given by the observations of Prevost and Dumas upon the effect of excision of the kidneys. These observers found that the removal of the kidneys always led to an accumulation of a considerable quantity of urea in the blood, and was followed by convulsions and coma, an epileptic state.

After this, clinical observations by practical physicians, showed that disease of the kidney was apt to be followed by attacks of convulsions and coma, when the excretion of urine fell in quantity to a very low amount; and it was found that, in such cases, a considerable quantity of urea was present in the blood.

A connexion was clearly thus established between the presence of urea in the blood, defective renal action, and the epileptic condition. But whether the active poison is urea, cannot yet be decided. Frerichs, indeed, has lately affirmed, that it is carbonate of ammonia, a product of the decomposition of urea. But even this is still *sub judice*. All that we really know is, that in certain states of diseased kidney, when the excretion falls below a certain point, urea will accumulate in the blood, and epileptic seizures will ensue; and, should the patient die, we find no brain-lesion to explain the phenomena; but we find unequivocal evidence of diseased kidney.

Still, imperfect as is the present state of our knowledge on this point, who is there that does not see, in the facts which I have detailed, a gleam upon the horizon, announcing the approach of some brilliant discovery, which no doubt the advancing state of organic chemistry will yet develope, and which will throw great light on the obscurest disease in the whole range of maladies, which affect the human frame?

You will find it convenient in practice to place the cases of epilepsy in three groups.

The first, and largest, is that in which I should place all those cases that are characterised by distinct, well-marked, often very severe symptoms, but in which we are not able to detect any distinct sign of lesion of the brain and spinal cord, either before or after death. Among these you find some which, from the frequency and the violence of the attacks, may be styled acute cases; but the great majority are chronic, and too many are not amenable to medical treatment; they afford an extensive field for the empiric to exercise his craft upon. Many of these patients will have fits for years, with variable intervals between them; in some mild, in others severe; yet, after death, notwithstanding the formidable character of the symptoms in some, and their long duration in others, we find no important lesion in the brain, which can be regarded in the light of a *cause* of the disease. And whatever change may have taken place in that organ, may be regarded as an effect of the disturbed consciousness and the impaired intellectual action, which follow each severe paroxysm, or the result of the shock and the injury to its nutrition conveyed by the excited state of the whole or a great part of it.

The second class of cases are those which may be grouped under the name of *renal* epilepsy.

You may place in the same category with the cases of renal epilepsy those in which the disease is associated with gout. I meet with many examples of this form of epilepsy occurring in men between 40 and 55. The patients are persons of decided gouty diathesis, and have gout rather of the asthenic kind, with a tendency to rapid effusions into the joints. In some, the gout may have shown itself but slightly in the extremities, and the patient may have been troubled with head feelings, giddiness, tinnitus, and may have found himself less capable of mental effort than usual. In the same class you may place the cases of convulsions occurring in the puerperal state, whether they take place after or before parturition; these are sometimes referrible to defective action of the kidneys.

And in the same class you may place cases which have connection with syphilis, although the syphilitic poison may not have produced any organic lesion of a tangible kind.

Epilepsy arising from exhaustion or perverted nutrition of any kind may be referred to this head; such cases as arise from the abuse of the sexual powers in

either sex, from prolonged mental effort and anxiety. In all such cases, a disturbed nutrition of the brain results, which may generate the irritating matter on which the morbid phenomena depend.

The cases contained in this group are those which are most amenable to treatment, and which may be treated by a rational method.

To a third class we may refer all those cases in which the epileptic state is associated with organic mischief of some kind, either of recent or of slow formation.

In this class you may include cases in which a clot of blood has been formed on some part of the surface of the brain, and those in which a tumour has been developed in its substance, or has grown from the dura mater, or even from the cranium itself, into the brain; as also cases, in which a tolerably extensive white softening is taking place in the hemispheres, and others in which the epileptic condition seems connected with disease of the dura mater of syphilitic origin, in which, in fact, something like a node has been formed on this fibrous membrane similar in character to that which we often see on the periosteum of the tibia. Epilepsy, and that of a very acute kind, is also sometimes associated with acute inflammation of the dura mater, of which we had an excellent example in the hospital about six months ago. The man was in Fisk Ward, and died from the exhaustion consequent upon repeated attacks following each other with great rapidity. Upon examination, we found a small tumour growing by a pedicle from the inner surface of the dura mater, which had pushed into the substance of the brain, and around which inflammation of a suppurative kind had taken place.

[Dr. Todd leaves the precise nature of the uræmic poison undecided. He merely insists on its existence. He thinks, however, that the poison is *urea*. He says:]

In practice, you will meet with, for the most part, two classes of cases of this uræmic poisoning, as it is called; the one acute, the other chronic. In the acute cases, the urine is suppressed, or greatly diminished; and the nervous symptoms come on quickly, consisting of rapid coma, with more or less frequent attacks of convulsions at brief intervals. In these cases, very active purging with a powerful drastic, such as elaterium, is often attended with remarkable success; and it is also a good plan to blister the occiput and nucha freely, so as to obtain an abundant serous discharge from the vesicated surface.—*Med. Times and Gazette, August 5, 1854, p. 129.*

First to be regarded, in the treatment of epilepsy, are rules of diet, and of a general hygienic character. All these should have immediate reference to the easy fulfilment of the digestive function, and the restoration and the maintenance of a vigorous nutrition. I cannot attach any importance to the exclusive plans of a milk diet, or a vegetable diet, or an animal diet, except where there is some distinct and obvious fault of primary and secondary digestion. After all, indeed, there is no intrinsic difference between an indiscriminate vegetable diet and an animal diet, excepting this, (which ought not to be lost sight of,) that a patient who feeds on cabbages, cauliflowers, peas, beans, &c., takes, in addition to a good quantity of nutritious matter of the same nature as that found in animal food, a considerable mass of ligneous and inorganic matter, which passes through the bowels undigested. Persons who feed on an exclusive vegetable diet, Vegetarians as they call themselves, eat large quantities, and make large masses of fæces. There is, however, an important difference between an *amylaceous* or *farinaceous* diet and an animal diet. The former kind of food is digested chiefly in the small intestine, the latter in the stomach. And you will often find it advantageous to adopt the one or the other, according as the digestive powers of the bowel or of the stomach are at fault.

I will just remark here, that important as attention to diet is in all cases of epilepsy, it is especially so in those in which the attacks are apt to occur in the night, when the patient is asleep.

In connection with diet, I may notice remedies which exert a direct influence on the digestive organs, such as purgatives. These have no special anti-epileptic power; they are valuable agents in the acute epilepsy which is connected with renal disease, such drugs as elaterium being particularly applicable. But in the ordinary chronic epilepsy they have no special value, beyond their use in aiding and promoting the digestive function, and in diminishing an unduly plethoric state. In child-

ren or young persons in whom the presence of worms may be suspected, anthelmintic purgatives are obviously indicated.

So, also, it may be said of bleeding. It is mainly useful in diminishing an unduly plethoric state of system, and in reducing blood too rich in colour. Sometimes, too, there may seem to be a local congestion, which local bleeding may relieve. But as it is clearly desirable to keep the blood at *par* as to the proportion of its staminal principles, it is not expedient that either local or general bleeding should form a prominent part of the treatment of epilepsy. And I would add, that whatever congestion of the nervous centres may occur in such cases must be regarded rather as the effect than the cause of the epileptic paroxysm. Nor is there any fact in pathology better established than that a state of anæmia, or a poor and watery blood, is favourable to the development or the continuance of the exalted polarity of the nervous centres which belongs to many, if not all cases of epilepsy.

Among the hygienic measures, a regulated system of exercise cannot be too strongly recommended; but it must be carefully adjusted to the powers of your patient. In persons of good muscular strength, I have seen remarkable benefit from strong and prolonged exercise. Active muscular exertion has, beyond doubt, a powerful influence in modifying general nutrition, and that of the nervous system in particular. Another very important hygienic measure is the regulation of sleep, which should not be too long at once. Many cases of epilepsy are benefited by taking sleep for two or three short periods in the twenty-four hours. It is desirable that the waking hours should not be too prolonged, nor extended to a late hour at night. There is a very remarkable connection between sleep and epilepsy (indeed, there is an analogy between the condition of unhealthy or perturbed sleep and that of epilepsy), which is one of the most interesting points of investigation in connexion with the malady.

Let me now briefly refer to some of the drugs that afford most prospect of use in the treatment of epileptic cases. Most of these have been used in consequence of their exhibiting a certain amount of physiological influence on the nervous system. As to others, it is difficult to trace the circumstances which led to their admission among the *juvantia* in this disease. First in the list I would place those drugs which belong to the narcotic class—opium and its various preparations, belladonna, henbane, conium, camphor. These certainly exercise a very powerful influence in calming the excitable state of the nervous system which accompanies epilepsy. They are applicable chiefly to the more acute cases, and especially when epileptic delirium is threatened or present. You should never administer them except when you have full opportunity of watching your patient, or of confiding him to the care of another. This remark, I need hardly add, applies chiefly to belladonna and the preparations of opium.

Belladonna has been greatly extolled by some French writers, far more than it deserves, as far as I can judge from my own experience. It is a remedy which leaves no permanent ill effect, and which, I think, is worthy of a more extensive trial in this country, than it has yet received. That it exercises a physiological influence on the nervous system, no one can doubt, who has administered it for forty-eight hours; but as it induces a state of pupil very like that which is so common in epilepsy, one might fear that it rather favoured the epileptic state. A good series of clinical observations are yet wanting upon the effects of this drug. The preparation chiefly used in France is the powdered leaves of the plant.

Opium is often useful in cases in which the fit is apt to occur in the night or early morning. A full dose given at bed-time will prevent the development of the fit.

The valerian was used formerly in epilepsy more extensively than now. It is a medicine, however, which should not be despised or discarded. Its use promises most in the complications of hysteria and epilepsy. Its virtue depends upon the presence of an oil or acid which is capable of entering into combination with metallic bases, as zinc and iron, and which also combines with quinine and other alkaloids. You may give it either in scruple doses of the powder two or three times a-day, or in infusion, or as an ammoniated tincture, or in combination with quinine or the metals.

Other nervine remedies may be added to your list, and you should keep them in reserve to employ as occasion may require, such as musk, castor, assafœtida, stramonium, sagapenum, garlic, remembering the moral influence of a change of drug.

I cannot say that the evidence of any special power of an anti-epileptic nature in any of these drugs is at all satisfactory.

In this class of drugs you may place the Sumbul, of which a tincture is prepared by Mr. Savory of Bond Street. One of the earliest cases in which I employed it, seemed to benefit very decidedly by it; and I have since used it many times with unequivocal good, so as to lead me to look upon it as a useful stimulant and anti-spasmodic remedy.

Digitalis has been greatly lauded by some. It may act favourably partly by its diuretic properties, and partly by its influence on the heart's action. There is quite sufficient evidence of its utility to warrant its being retained among the list of remedies applicable to this disease, and to justify its occasional cautious use.

The cotyledon-umbilicus may rank with digitalis as a remedy for epilepsy. I cannot say that I have met with a case distinctly benefited by its use; but the cases published by Mr. Salter, and those recorded by the late Dr. Graves, indicate that it possesses a certain anti-epileptic power. I incline to think it acts by a diuretic influence.

There are other drugs obtained from the vegetable kingdom which I think deserve a trial in epilepsy. One of these is the Achillæa millefolium, which, in the form of a decoction, exerts a very decided diuretic influence. On the same principle, taraxacum may do good in certain cases, as in those in which the epilepsy has a relation to gout in the system.

The alkaline salts, especially the bicarbonate and the nitrate of potass, appear to me often to be very useful on a similar principle, namely by increasing the activity of the kidneys.

The cardamine pratensis was greatly extolled by Sir George Baker. Turpentine has been used chiefly from its anthelmintic properties. It is a remedy that should be used very carefully, and never if there be suspicion of organic disease of the brain.

The *selinum palustre*, or *peucedanum montanum*, is highly commended by Herpin, on, as I think, insufficient grounds. The plant is very difficult to procure, and I have not yet been able to try it.

The various metallic tonics have been and are extensively used in epilepsy. Of these the safest is zinc, the sulphate, or the oxide. Either of these preparations may be given without any disadvantage that I know of for a considerable time, and in large doses. Some time ago I gave to a patient in this hospital as much as half a drachm of sulphate of zinc thrice a-day without any sensible effect. The dose had reached that amount by gradual increase. Whether these drugs exercise any special favourable influence I am unable to say; all I know is, that, under their long-continued use, patients sometimes ceased to be troubled with fits.

The various salts of iron are also applicable to the treatment of this disease, and especially to cases in which the blood is poor in hæmatin. But they are not so harmless as the salts of zinc. Their tendency in some persons to excite headache precludes their use with them; and they often disturb digestion, and on this account cannot be persevered with. The saccharine carbonate, the sulphate, the citrate, and the tincture of the sesquichloride, as well as Griffith's mixture, are the preparations of iron which you will find most suitable to the generality of cases.

Salts of copper and of silver are also used for epilepsy, with, I think, at best, but very doubtful efficacy. To the cautious use of the ammonio-sulphate of copper I see no material objection; in too large doses it may irritate the gastro-intestinal mucous membrane, but such irritation is easily removed.

There is, however, a much more serious objection to the use of the salts of silver. The nitrate and the oxide are the salts employed. Now, there is no doubt that the nitrate discolours the skin. We see repeated examples of this in the streets; scarcely a day passes that I do not recognise persons in the streets, as epileptics, who had been treated with nitrate of silver. If the nitrate of silver were a certain, or even a very frequent cure for this formidable disease, well and good; but, seeing that it is very far from being entitled to the credit of being such a remedy, I say, that we have no right to make blackamoors of our patients, and to stigmatize them for ever as epileptics, and too often *as epileptics uncured*, on the mere chance of doing good by nitrate of silver. No doubt few would hesitate long between choosing a discoloured skin, and being the subject of a disease so fearful as epilepsy.

But it seems to me, that the remedy ought to be a specific, to justify the practitioner in administering it, with the strong probability there is of producing such an effect. Remember, too, that the nitrate of silver stain communicated in this way, is probably indelible, and that by an experienced eye it is not to be mistaken. I have seen it after ten and fifteen years from the administration of the drug.

Finding, then, no special virtue in the nitrate of silver as against epilepsy, and that it is very apt to produce the serious result to which I have referred, with whatever care it may be administered, I have long since abandoned its use. I fear the oxide of silver is not more promising either as to its influence on the disease, or its freedom from the discolouring power.

Indigo has likewise been given in epilepsy, but with no other result, as far as I can learn, than what Penil observed, that it turned the nails of his patients blue.

The time has, I think, now arrived, when we must look in another direction for an anti-epileptic remedy. We must turn our attention more particularly to those substances which are capable of being exhibited by inhalation, of being brought to bear upon the nervous system without being subjected to the action of the gastric fluid.

Ether and chloroform, it is now well known, are capable of being taken into the blood in this way. Some years ago I tried experiments as to their power of controlling the convulsions produced by strychnine, and with the result of finding that they possessed that power very completely. As long as an animal is fully under the influence of ether or chloroform, no convulsion can be produced. I was thus led to try their inhalation in epilepsy. I have chiefly employed for this purpose chloroform, because it is less disagreeable to patients to take. The results of my observations so far, are these,—that in the more acute forms of epilepsy the inhalation of chloroform has considerable influence in controlling and modifying the attacks; it has also a very decided power over epileptic delirium; and over some of the concomitants of epilepsy, as the violent convulsive jerks of the muscles of both upper and lower extremities, which are always most distressing, and often very dangerous. It has a marked and immediate power over puerperal convulsions; and I am glad to see that my friend, Professor Simpson, of Edinburgh, sanctions, by his high authority, its use in such cases. It is also applicable, and with advantage, to the convulsions of infants, and to laryngismus stridulus.

There is no use in employing chloroform in epilepsy unattended by other symptoms, when the attacks are at uncertain times and at long intervals; nor do I advise you to attempt its use, unless the patient is fully under your control or will heartily co-operate with you. And, in cases where the heart is affected either functionally or organically, it should never be used. Its inhalation at two or three stated times of the day, exercises a very calming effect on the nervous system, diminishing its excitability very materially. But this must be carried on over a long period of time—many months, and even years; and the dose may vary from twenty to sixty minims, according to the effect produced, and you should aim at obtaining a gradual, not a rapid effect. The patient should always remain in the horizontal posture during the inhalation, and for at least half an hour after it.

You will expect me to say something on a proposal made within the last few years to open the trachea of patients suffering under severe epilepsy, with the twofold object of preventing the fits, or of obviating their effect in creating undue distension of the cerebral blood-vessels, if they should occur.

Before a proposition of this kind can meet with general favour from practical men, it must be satisfactorily proved that the hypothesis, or theory, if you will, from which the proposal emanates, is essentially sound, and next it must be shown that the operation is in itself one that is not seriously dangerous to life; and lastly, it ought to be in the highest degree probable that the attacks will not recur after the operation has been performed. It is, as you know, contrary to all principles of sound surgery to perform an operation unless it be for the complete removal of the disease, or at worst, to stave off the reappearance of the malady to the latest period.

The hypothesis from which this proposal emanates assumes that the exciting cause of all the evil in severe epileptic paroxysms, is the contraction of the muscles of the neck and glottis, which induces and maintains a congested state of brain, which, in its turn, excites and keeps up the convulsions. I have elsewhere examined

this congestion theory in detail, and I am not aware that the arguments which I then advanced against it have ever been fairly met. I have also shown, by experiment, that epileptic convulsions may be excited in dogs which had been freely tracheotomised previously, full provision having been made for the free ingress and egress of the air; and also in a dog in which the muscles of the glottis were paralysed by section of the recurrent nerve. These experiments the advocates of tracheotomy in epilepsy have found it convenient to ignore.

Again, I would ask, is tracheotomy either a very simple or a very safe operation? I do not think that the results of the operation either for other diseases, or for epilepsy, are very satisfactory. Patients often die from the effects of the operation, exhaustion, erysipelas, diffuse inflammation of the areolar tissue spreading into the mediastinum.

And, lastly, what are the prospects that, the operation having been done, the evil will not recur? This can only be ascertained by that which I cannot recommend, frequent experiment. I would only make this remark, that those who have the courage to try these experiments, and feel themselves justified in so doing, must carefully watch the results, and endeavour to draw a proper distinction between the actual physical effects of the operation, and that mental influence which, as I have shown you in a former part of this lecture, undoubtedly tends to stay the disease, when any new treatment is adopted, or any considerable change takes place in the patient's position and circumstances.—*Med. Times and Gazette, Aug.* 12, 1854, *p.* 154.

9.—*Epilepsy treated by the Bark of Black Elder Tree.*—The emeto-cathartic and hydragogue properties of the second bark of black elder (sambucus nigra), have long been known, and used in various forms of dropsies; but no mention has hitherto been made of the employment of this substance in the treatment of epilepsy. M. Bogetti relates five examples of cure obtained by this remedy alone. In order to prepare it for administration, the branches of the elder, of one or two years, are taken; the grey bark is removed, and the second bark which remains is scraped off. About five ounces of common water, hot or cold, are poured upon two ounces of the bark, and the infusion is allowed to stand forty-eight hours. The infusion, properly strained, should be taken at intervals of a quarter of an hour for a certain number of times when the fit is threatening, the patient fasting. It should be resumed every six or eight days.—*Rev. Thérap.*—*Med. Times and Gazette, July* 29, 1854, *p.* 119.

10.—ON THE USE OF CHLOROFORM IN TETANUS.

[In the 'Medical Times and Gazette' will be found a very interesting series of reports of cases of tetanus, forty-three in number, of which eleven recovered. The editor makes some good remarks on the use of chloroform as follows:]

The following propositions appear to be warranted respecting it:—

1. That, in the great majority of cases, inhalation of chloroform may be practised with safety as regards immediate consequences.

2. That it is always effectual in allaying spasm for the time.

3. That it exerts, however, no preventive influence whatever, the spasms usually returning, with even increase of severity, very shortly after its suspension.

4. That its continuous administration over long periods of time is not to be recommended, since the patients sink at least as fast, if not faster, than when the disease is allowed to display itself.

5. That it is of great benefit in certain protracted cases simply as an alleviant of the pain. In some of these it will procure rest for periods often of an hour or more after the suspension of the inhalation, and acts altogether much more favourably than in the earlier stages.

6. That, in certain protracted cases, it is of the greatest use in enabling a patient, while in a state of half-insensibility, to take food, who would otherwise be unable to swallow.

7. That, excepting for the two last-named purposes, its use does not seem to be attended by any commensurate benefit, while it may much interfere with the action of other remedies, and, very possibly, be actively injurious itself.

These conclusions must be understood to apply only to chloroform inhalation, since, from the cases published, there appears reason to believe that the results of that of ether have been more favourable. As, however, the latter agent has not been, of late years, used in London, we have no means of judging as to the proportion of cases in which it did not relieve, or whether in any it appeared injurious.

It may be worth a thought whether the employment of anæsthetic vapours *externally* in cases of tetanus might not promise some benefit. The spasms are for the most part reflex, and excited by peripheral irritants. A bath of vapour might be easily given by covering the bed with an impervious material, and exposing beneath the clothes a sponge saturated with ether. The patient's head should of course be left out, and the clothes well tucked in round the neck.

[These cases show the relative value of other modes of treatment. Two cases recovered under the use of belladonna: two cases showed the uselessness of tracheotomy: one recovered during the exhibition of sesquioxide of iron and Dover's powder. In several cases Indian hemp seemed useful. In one case nicotine controlled the spasm and repressed constitutional disturbance. On the whole, vegetable sedatives seemed most beneficial.]—*Med. Times and Gazette, June* 17, 1854, *p.* 621.

11.—*Cure of Tetanus.* By Dr. Edward Hodges, Somerset.—[In this case a boy, aged twelve, lacerated his great toe, which necessitated the removal of a part of it. In about twelve days the patient could not open his mouth, and other more general symptoms of tetanus followed:]

On the 24th January, the application of morphia (the hydrochlorate) to the wound was substituted for its internal administration—first in the form of powders, as the poultices were renewed, and subsequently by means of a piece of lint moistened with the following application:—Hydrochlorate of morphia, one scruple; tincture of hyoscyamus, two drachms; water, three ounces. This local treatment was continued until the 11th of February, when the general tetanic symptoms having much diminished, and the patient being able to protrude his tongue slightly, and to masticate a little, the foot was dressed and bandaged. The lad recovered perfectly, and is now to be seen daily about this neighbourhood, driving his father's (a baker) cart.

Though a considerable quantity of calomel was administered during the stage of arterial excitement, and also of tartar emetic, the gums were unaffected by the former remedy, and the latter produced no degree of emesis. On the subsidence of this stage, and the consequent discontinuance of the antiphlogistic treatment, the most liberal diet that could be taken by the spoon was allowed, and also some good bitter beer, and subsequently wine in small quantities.—*Lancet, Aug.* 5, 1854, *p.* 101.

12.—*On Idiopathic Tetanus.* By Dr. Hassall, Physician to the Royal Free Hospital.—[The following case of idiopathic tetanus is chiefly interesting as showing the use of chloroform in mitigating misery. The difficulty is to know how far to push the remedy with safety. The patient was a boy of fourteen. The affection could not be traced to any injury. The exacerbation of the spasms recurred every two or three minutes:]

The patient was ordered a hot bath and an enema of warm water; the enema did not return, nor did the bowels act, owing most probably to the spasm of the sphincter ani. The following draught was also to be given every four hours:—Chloroform, five minims; Hoffman's anodyne, twenty minims; water, one ounce.

Same day (nine P. M.)—The patient states that the warm bath relieved him, and he expressed a wish for another, which was accordingly given him. He desired very much to go to stool, but could not pass anything, though he used expulsory efforts.

Same day (ten P. M.)—The exacerbation of spasms has been recurring at intervals of only a minute, during which the agony of pain was intense and breathing suspended, so that fear was entertained that the poor sufferer would die of apnœa. He was now brought under the influence of chloroform by inhalations; under its

influence every muscle became completely relaxed, the lower jaw dropped, the breathing was tranquil, and the pulse fell from 160 to 120. The countenance of the patient was also very favourably modified by the chloroform, whereas under the fits he had a face expressive of exquisite agony; "the forehead corrugated, the brow knit, the orbicularis muscle of the eye rigid, the eyeball motionless and staring, the nostril spread, the corners of the mouth drawn back, the set teeth exposed, and all the features fixed in a ghastly grin—the true risus sardonicus." As soon, however, as he was brought under the influence of chloroform, this frightful "nightmare" as it were passed away, and a placid smile, indicative of peace and freedom from pain, took its place. So marked was the power of chloroform over the system that on the least cessation of its administration the contraction of the muscles immediately returned.

The patient was kept in a state of anæsthesia until six o'clock the following morning—a space of eight hours—but tetanic spasms returned with equal severity as soon as the influence of the chloroform passed off. The poor boy died at eight A. M.—viz,, two hours after he had ceased to inhale chloroform (immediately after a severe attack of spasm), partly from apnœa, and partly from asthenia, being exhausted by the intense muscular action and exquisite pain.

No post-mortem examination could be obtained.

The above case was manifestly an acute one, rapidly running its course, and terminating fatally in about forty-four hours after the commencement of spasm; it was also, as far as could be ascertained, idiopathic.

The patient was kept in a state of anæsthesia for the space of eight hours, during which time he was quite free from pain or spasm, and slept quietly. No signs of the fearful affection could then be discerned, but the invisible cause, lying dormant for the time, rendered the disease manifest in all its previous virulence, as soon as the agent that kept it in abeyance was removed.

We, of course, cannot say that chloroform is a *remedy* for tetanus, but with this compound we have the power of subduing for many hours the rack and torture of a fatal malady, and prolonging life. Perhaps the disease might, in favourable instances, be conquered by the anæsthetic agent. At all events, chloroform deserves a fair trial before it is relinquished; if it fails as a specific remedy, it will yet remain a very useful palliative, and we are justified from the nature of the disease in using any agent that will subdue pain, allay spasm, and calm the distressing agitation of the unfortunate creatures who are attacked by this dreadful malady.—*Lancet, June* 3, 1854, *p.* 597.

13.—*Treatment of Chorea.*—Sulphate of zinc is undoubtedly the remedy in most general use for the treatment of the ordinary forms of chorea in the London Hospitals. It is usually given in small doses at first (gr. j. to ij.) and gradually increased if the disease do not yield (gr. v. to viii.) Among other remedies in much esteem must be noticed the various preparations of iron, quinine, valerian, the ammonio-sulphate of copper, the iodide of zinc, and arsenic. In St. Thomas's Hospital Dr. Barker frequently prescribes, in severe cases, the last-mentioned remedy. He states that he has several times cured very quickly by its means, cases which had proved obstinate under other treatment. The dose is, of the liquor arsenicalis, three minims for a child, and from five to six for an adult, given in a bitter infusion three times daily. Chorea, in its more ordinary forms, is, as is well known, curable with tolerable certainty by almost any of the nervine tonics; hence the number of specifics which have been boasted against it.—*Med. Times and Gazette, July* 1, 1854, *p.* 11.

14.—ON THE USE OF CHLOROFORM IN DELIRIUM TREMENS.

By Dr. B. G. M'Dowell, Physician to Whitworth and Hardwicke Hospital, Dublin.

[In the exhibition of chloroform in cases of delirium tremens, the practitioner should ever remember that chloroform is a powerful sedative—it exhausts—depresses. It can, therefore, only be used in those cases which are the result of direct stimulation of the brain,—sthenic cases,—cases which are caused by some sudden, short, and

excessive use of alcohol. In those cases which are caused by suddenly *leaving off* the stimulant, as in some habitual drinkers, chloroform would be highly dangerous. This is a very important distinction to make. Dr. M'Dowell's first case seemed to be one of the *sthenic* character. The patient was violent, and altogether in a high state of excitement.]

Chloroform was prescribed; half a drachm in a draught to be given every second hour.

After the fourth dose he fell asleep. At first his sleep was frequently interrupted, and he awoke often, but soon it became more profound, and he slept all night. Next morning we found him still sleeping, the pupils were somewhat contracted; on rousing the patient, we found that he was perfectly rational, and that no delusions of any kind remained. After getting some food, he again fell asleep, slept for nearly twelve hours, and awoke perfectly convalescent. Discharged November 16th.

[The second case was equally successful.]

On admission, his manner was nervous and agitated. He was rational and collected in his answers, but, when left to himself, his mind wandered greatly, and he was unable to control his thoughts. The surface of his body was bathed in cold perspiration. The pulse was full and soft, there were remarkable tremors of the tongue and hands, and total loss of appetite.

Treatment.—Tepid affusions to the head. Fifteen minims of tincture of opium, in an ounce of camphor mixture, every third hour.

November 19. Slept none last night; disease more fully developed; patient raves and talks incessantly, but is easily persuaded to remain in bed; tongue dry and furred; urine scanty and high-coloured; bowels confined; skin has ceased to perspire.

℞. Mist. rosæ catharticæ ℥iv. statim. Thirty minims of chloroform every second hour, after the bowels are freed.

Towards evening the patient became so exceedingly violent, that the strait-waistcoat had to be used.

November 20. Slept little during the night, towards morning he had some hours' sleep, and the medicine was omitted. He is rational and sensible, answers questions correctly, and is evidently disposed to sleep more; the pupils are somewhat contracted.

To have good beef tea during the day, and no medicine until the evening, when ℥i. of chloroform is to be given.

November 21. Slept almost all night; and with the exception of some nervousness of manner, no trace of the disease remains.

The anodyne powers of chloroform are well illustrated in the preceding cases, in both of which the disease was fully developed, and the nervous system in a state of high excitement. In the one case, two, and in the other, three drachms of chloroform, administered in half-drachm doses, at short intervals, were sufficient to subdue the functional excitement of the nervous system, and to induce sleep.

Sleeplessness is the principal source of danger in delirium tremens: the longer the system is deprived of sleep, the greater is the degree of nervous exhaustion, which too frequently terminates in death, preceded by convulsions. Hence, the absolute necessity of directing our treatment to this symptom especially, and the value of any remedy on which we can rely for procuring sleep.

In detailing these cases, it is far from my intention to recommend chloroform in preference to opium, in the treatment of delirium tremens. There are, no doubt, many cases which will yield as promptly to opium, as did the cases here narrated to chloroform. But I conceive it may prove of great advantage to know that chloroform is capable of controlling the disease by its direct sedative and anodyne properties, for the following reasons:—

First; because there is a form of the disease in which opium is generally inadmissible; I allude to what has been termed the sthenic variety of delirium tremens, which is produced by the direct influence of stimulants, and is consequently more connected with active congestion of the brain, than the more common form of the disease, which is developed subsequent to the relinquishment of stimulants, and is consequently associated with a depressed condition of the nervous force. In the former affection, we generally rely upon purgatives, and the careful application of

cold to the head; in some few cases leeches may be applied; but if these means fail in procuring sleep, much danger is to be apprehended; the exhibition of opium is hazardous, yet death will certainly ensue, if sleep be not obtained. Under these circumstances, I conceive chloroform may be given with the greatest advantage.

Secondly; because it occasionally happens, even in the asthenic form of the disease, that opium fails altogether to procure sleep, and seems to increase, rather than to allay the excitement of the nervous system. Under these circumstances, and especially when the tongue becomes dry, and the pupils are contracted, the use of opium cannot safely be persisted in. But in chloroform we have a remedy, which, from experience, I can confidently recommend to meet the exigencies of such a case: the following instance is adduced in illustration of this remark.

Case 3.—In the month of September, of last year (1853), I attended, with Mr. Gorman, of Henry-street, a gentleman of about thirty years of age, who laboured under delirium tremens for the first time. When I was summoned to see him he had been ill for three days; the opium treatment had been most fully and properly employed, yet he had not slept for forty-eight hours; all the characteristic symptoms of the disease were fully developed: the patient never for a moment ceased talking, the activity of his mind was astonishing, and the mental delusions under which he laboured were of the most extraordinary and varied character. Tepid affusions were employed, and morphia given in full doses.

On the following day, we found that he had not slept, and that his symptoms were in no way improved. At times he was very violent, but was easily persuaded to do as he was desired.

The next day found him still perfectly sleepless. It was now four days since he had had any sleep. He was so hoarse as to be unintelligible from incessant talking and vociferating; eyes congested; pupils much contracted; tongue dry, and protruded with difficulty; urine scanty; tremors of the hands and arms, with subsultus tendinum. His strength was evidently failing, and he no longer struggled to leave the bed; the pulse had become smaller and more rapid. He was not sinking from want of stimulants or of nourishment, for both had been given in sufficient quantity, but death was impending for want of sleep, and opium had been pushed to the uttermost. Under these most unpromising circumstances, I recommended chloroform to be given internally; a drachm was immediately given, and repeated in an hour; soon afterwards he slept for a few minutes; a third dose also containing 3i. was now given, and soon he slept soundly. The next day he was entirely free from every symptom of the disease.

In conclusion, I have only to remark, that an extended experience is required to determine: 1. Whether chloroform may be safely relied on in the treatment of delirium tremens; 2. The comparative advantage of a mixed treatment; *i. e.*, combining the use of opium, with that of chlofoform; or 3. How far success would be rendered more certain by adopting the opium treatment in the first instance, and following it up by the administration of chloroform in full doses.—*Dublin Hospital Gazette, Feb.* 15, 1854, *p.* 25.

DISEASES OF THE ORGANS OF RESPIRATION.

15.—ON CONSUMPTION.

By Dr. Theophilus Thompson, F.R.S.

[With respect to the symptoms of phthisis, some are more local, and others more constitutional. The following remarks of Dr. Thompson refer to those symptoms which are more obviously constitutional:]

In a large proportion of individuals affected with phthisis, at a very early period of the disease, and even in the members of consumptive families, amongst whom distinct manifestations of the malady have not yet appeared, you may observe a peculiar, transparent appearance of the edge of the gums, where they are reflected over the teeth, apparently resulting from fineness of structure; in other instances, especially if erethism of the mucous membrane exist, you may observe a border of lake or vermilion tint, such as we might suppose to originate from congealed blood in the capillaries attracting oxygen with great activity.

Three years since, when my attention was first directed to this inquiry, I observed this appearance in twenty out of twenty-one consecutive cases of phthisical males, and in thirteen of twenty-one phthisical women. More recently, on repeating the investigation, the records being kept by a different assistant, thirty of forty-three women, and ninety-two of one hundred men exhibited one or other of the modifications of gingival border which have been described. I have pursued the inquiry to some extent as respects this condition of the gums in other diseases, and have hitherto rarely observed it, except in persons who either were the subjects of phthisis, or who have subsequently become so. The wax model which I now show you represents this appearance in a member of a consumptive family. It preceded the occurrence of cough, and of prolonged expiratory murmur; but the disease has made rapid progress, and is now in the last stage. You may observe that the teeth are perfectly free from tartar. The festoon which I have described differs from the uneven border of redness produced by that source of iritation, although the presence of tartar may doubtless modify the condition occasioned by tubercular affection.

In some doubtful cases Mr. Harrison did me the favour to remove the tartar; and although the vividness of the border was subsequently in some degree lessened, the appearance remained fully as characteristic as ever. The diffused inflammatory redness resulting from the administration of mercury, or other irritating medicines, is readily distinguishable from the clearly defined border under consideration; and if any similar state should be found in cases of purpura, the concomitant circumstances would probably be adequate to remove any uncertainty regarding the cause. I believe this line to have much diagnostic value in early, or, still more, in threatened phthisis, when unaccompanied by any other morbid condition adequate to explain its occurrence. You perceive that in the model the teeth are represented as fine and delicate. It has sometimes been asserted that the teeth of the consumptive are usually remarkably good. There is a fallacy in this statement. In consumptive persons they are sometimes carious, but usually peculiarly delicate, pearly, or glassy; teeth having the firm, ivory-like aspect characteristic of a healthy condition, being in such persons rare. When you reflect how soon they obtain their characteristic organization in the body, I think you will agree with me that the circumstance thus briefly noted furnishes powerful evidence for the opinion I am endeavouring to illustrate, that phthisis is for the most part related to a constitutional affection associated with the conditions of early growth.

Strong assertions have been made regarding the connection of phthisis with indigestion. My impression is, that such opinions have been too strongly expressed. It is true that persons of consumptive constitution often have a restricted appetite, and take little at a time, although a certain degree of hunger may quickly return. They are not in general good feeders. They are not amongst those who "unbutton after supper, drink old sack, and lie on benches in the afternoon." Sometimes you find them affected with a particular kind of indigestion recurring from time to time, without any appreciable dependence on irregularities of diet, or other ordinary causes, and perhaps attributable to the deteriorated condition of the blood with which the stomach is supplied, and the consequent inability of that organ healthily to perform its functions; but even this variety of indigestion is scarcely common, and it is probable that the various symptoms of disordered stomach are more frequent in the miscellaneous patients of general hospitals, than in those who are the subjects of phthisis. I am more inclined to think that the originating elements of phthisis are first apparent in the mesenteric glands, where a disturbed and hurried function, a condition short of that which induces obvious scrofula or enlargement, occasions an imperfect performance of their part in the elaboration of healthy blood. For clearer views on this important subject, our hopes must be directed to a microscopical investigation. The hurried kind of action which seems associated with the development of phthisis may, in constitutions prone to tuberculosis, exhibit itself early in other ways, and be manifested in those organs whose growth at any particular period is most rapid. Prior to the time when the lungs are most actively engaged, there is what may be called the cerebral era, when the growth of brain is especially rapid. At this period persons who, if they lived to the pulmonary era, would probably become consumptive, are apt to fall victims to tubercular meningitis. The phenomena observable in such subjects are consistent with the evidences which we have described of hasty, abortive growth, as characterising tubercular affection.

Precocity, terminated by hydrocephalus, is in such instances common. It is amongst such we find individuals pale and prematurely wise, exhaling like the early dew before the morning sun.

At a later period, other significant evidences of a similar condition may be observed. Amongst individuals thus affected, I have found many remarkable for intellectual activity, ready memory, quick perceptions, alert emotions, sensitiveness of sympathy, poetical temperament; they can work early in the morning; they are not easily fatigued by intellectual exertion; they engage energetically in the ambitious pursuits of life. A young gentleman, full of ardour, working day and night on some animating object of study, was told he ought to rest. He replied he did not need rest; that he never had the headache, and was never tired. He was told he ought to be tired by such exertions, and that not to be fatigued by them was morbid. He disregarded the admonition. Shortly afterwards he had hemoptysis. Other indications of mischief in the lungs appeared. In a few months he died of consumption.

It is my impression that there is often an interchangeable relation between insanity and phthisis. I have known early symptoms of consumption subside, the friends of the patient disregarding medical admonition, and imagining the physician to have made an incorrect diagnosis. Some eccentricity of mind has appeared. As the chest seemed to recover, the brain became obviously affected, and the patient has become an inmate of a lunatic asylum.

I am convinced that there are many individuals of the consumptive diathesis in whom external circumstances alone determine which organ shall be most involved in the morbid condition. A person thus constituted, under the influence of a depressing life and confined air, will become consumptive: impelled by the stimulus of inordinate ambition, overworked in any one direction of intellectual exertion, he will become insane. Under favourable circumstances of prosperous, well-regulated activity, he may attain the consolidation of mature life, and escape either catastrophe.

But the blood itself, when minutely examined, may perhaps exhibit evidence of accelerated action. If any of us were to run for a few minutes, and then, pricking his finger, submit the blood to examination under the microscope, the discs, instead of retaining for a quarter of an hour or more their proper outline, would probably, in a few minutes, assume a strawberry form, or become stelliform, serrated, or corrugated. A similar change is often rapidly apparent in the blood of the phthisical, especially in the cases which are proceeding badly. My observations have not been sufficiently repeated on this subject to authorise my offering the statement positively, or presuming to assert that the rapidity with which this phenomenon presents itself, may fairly be regarded as of moment in the way of diagnosis; but irrespective of this suggestion, considerable evidence has been afforded that, in the consumptive constitution, there is a change of material and a change of action; and indications have been presented of a course of inquiry, which promises to be most fertile in elucidating the nature of the changes associated with the consumptive organization. With so many active minds engaged in the investigation, we may hope for speedy accessions to our knowledge; but there is much to learn. We know something of the *materials* of our frame—not much of the conditions by which they are regulated. The text of my former lecture—"Maximè in minimis"—requires to be illustrated by a clearer light. The most minute in form is regulated by laws sublime, but unknown. We can recognise the shape of living organisms, but as yet we cannot explain the forces by which they are determined.—*Lancet, July* 22, 1854, *p.* 45.

[In the palliative treatment of phthisis various modifications have lately taken place.]

In the treatment of *night perspirations*, sulphuric acid was formerly the remedy most commonly employed, and with very partial success. Other acids, such as the gallic and the acetic, are probably more effectual for the purpose. Considerable abatement of the symptoms may often be derived from the use of a night-dress dipped in sea-water or a solution of salt, and dried; but a most important improvement in the treatment of this symptom is the administration of oxide of zinc at bedtime, in doses of about four grains, combined with any anodyne extract, such as henbane or hemlock. This property of the oxide was first observed by Dr. Robert Dickson. The mode of action of the remedy is undetermined, but the relief induced

by its administration is more decided than from any other medicine with which I am conversant.

One of the most trying of the complications of phthisis is *diarrhœa*. It is not my present intention to attempt a description of the various modifications of this condition, but a few practical remarks on the subject may not be inappropriate. When diarrhœa is associated with intestinal ulceration, or with an appearance of the tongue and other circumstances indicating irritation of the mucous membrane, the diet, notwithstanding the existence of debility, must be bland and unstimulating. Instances dependent on a relaxed state of the intestinal exhalants may require astringents. The peculiarities of the individual case must determine whether we select the preparations of silver, copper, or lead. My experience, however, during the last six years, has confirmed my impression, that of all the remedies which have been recommended for this affection, there is none more generally available than trisnitrate of bismuth. This medicine, combined with a little gum and magnesia, and when necessary with Dover's powder, perseveringly employed, usually effects the object more permanently than other measures, and without inducing any opposite or collateral evils.

The variety of remedies employed for the relief of *cough* serves to show how intractable this symptom often proves; and I think we may assert that, till within a few years, sooner or later, opium, in some one of its many preparations, has usually become the chief reliance of the practitioner in its treatment. I rarely meet with instances where the inhalation of chloroform, combined with three times the quantity of spirits of wine, fails to relieve; but there are objections to the indiscriminate use of so powerful a measure. Often the inhalation of camphorated spirits is equally effectual; sometimes the vapour of warm water, or infusion of hops. The simple expedient of swallowing a little oil slowly often gives more relief than the use of what are called intentional medicines. In some irritable conditions, tincture of aconite in doses of four minims, is worth the trial. When expectoration is profuse, creasote, pyro-acetic spirit, infusion of pitch, or balsam of Tolu, may be administered; but of all the remedies referable to this class, appropriate for cases accompanied with profuse expectoration, not one has proved in my hands so efficient as petroleum, or Barbadoes tar, the cough being frequently relieved, and the expectoration rapidly moderated, under its use.

On the treatment of *hæmoptysis* I need not dwell. Single doses of mercury, and a few saline aperients; antimony, even bleeding, may, in special cases, be requisite. Direct astringents, such as gallic acid and acetate of lead, may occasionally be required. In a larger proportion of cases, turpentine is the most satisfactory remedy, but not unfrequently it is inexpedient to attempt the sudden arrest of hæmoptysis, since, when moderate, it is rather useful than dangerous. Such are a few simple hints which I would venture to offer as derived from my own observations of the treatment of individual symptoms. It is not my present object to give an elaborate essay on the treatment of phthisis, but rather to confine my remarks to the statement of opinions either formed or confirmed by my special opportunities for investigation. I feel that a detailed review of the subject would scarcely harmonize with the broad principles which it was the object of preceding lectures to illustrate. The symptoms thus briefly noticed are but incidental evils springing from that error of the organism which I formerly described as exhibiting itself locally in faulty cell-formation, generally in defective blood, and disturbed, imperfect, hurried action. No satisfactory or systematic view can be presented on the subject of treatment without regard to the different degrees of progress of the morbid action.

The disease may be only threatened. Rapid growth, some approach to the phthisical physiognomy, a pearly or somewhat coloured margin to the gums, observed in a member of a consumptive family, although not considered an invalid, may be the chief indications of the impending disease. In such instances well-arranged exercise, especially of the arms, as in fencing or archery, the adoption of judicious plans of education, and the appropriate choice of climate or employment, may often avert decided mischief.

But supposing local pulmonary disease to be commencing, as evinced by a disposition to chilliness; a feeling of oppression under the sternum; gnawing sensations about the scapula; effort in making a thorough expiration; prolonged expiratory murmur; hæmoptysis; expectoration resembling isinglass in appearance; a pulse

evincing the characteristics formerly described.—Congestion should then be obviated. A dose of blue-pill, followed by an aperient, may be administered. Sometimes the application of a few leeches applied over the threatened part, and the administration of liquor potassæ with infusion of cascarilla, may be advantageous.

But softening may occur, the second stage may be established, with the clicking, or "humid crepitation," as an auscultatory sign, expectoration exhibiting, perhaps, elastic tissue under the microscope, as well as characteristic cells of tubercular character. The treatment must then be cautious, unstimulating, but strengthening; exposure to cold or heat must be shunned; change to a tropical climate is now dangerous, and even the excitement of sea-air may do harm.

When the third stage is established, indicated by gurgling or amphoric respiration or cough, we have to adopt such treatment as shall strengthen the constitution of the patient, so as to become tolerant of the local disorganization, reparation of which can be expected only under very favourable circumstances and assiduous care.

Such general statements as those which I have offered will not, however, satisfy minds anxious to make a distinct and decided step in the treatment of this fatal disease. Do the modern acquisitions furnished by pathology, zoo-chemistry, and histology, assist us in establishing any principle calculated to determine the appropriate treatment? The chemical condition of the blood of the consumptive patient is, in some respects, analogous to that characterizing chlorosis; but it is not the true olichæmia of that disease, and iron will not cure, although it may often be beneficial as an auxiliary.

We have described the deviation from the natural condition in the process of cell-formation as the earliest local manifestation of phthisis. Will our knowledge in this respect assist our decision? Fatty degeneration, tubercular degeneration, and cancerous disease, in their cellular relations, may be contrasted. Cancer-cells as compared with phthisical, have, perhaps, more of albumen and less of fat, but the nature of the action is more important than the attendant chemical conditions. In fatty degeneration, we have life failing; in tubercle, running out by undue rapidity; in cancer, running riot.

Is there any kind of treatment calculated to moderate this undue action, and at the same time give tone? I have lately tried the influence of digitalis. The effect of this remedy in lowering the pulse is well known; but I have been interested to observe that the rule obeyed by the phthisical pulse is no longer strictly applicable when the sedative influence of digitalis has been produced on the heart. If the anomaly of pulse formerly described depends on the presence of the tubercular element in the blood, the restored susceptibility to change of posture might seem to imply that some effect is produced on the tubercular disease. It is worthy of note that some of the most able physicians of the last century attached great importance to the use of digitalis in pulmonary affections. Dr. Ferriar's remarks on the subject evince an honesty and intelligence which entitle them to our respect. "Digitalis," he observes, "has often quieted the pulse, relieved the cough, and given the patient feelings of recovery." He adds, "the patient's ultimate recovery is not to be confidently expected, even when the pulse is reduced in velocity, and the symptoms are evidently mitigated by the action of the medicine. Many disappointments have taught me not to be elated by one or two instances of success, and I should deceive the public if I presented to them only examples of fortunate practice. To cure (such disorganization as we sometimes observe after death) would require an effort of the power which first created the living body."

It would be unreasonable to disregard the assertion made by so cautious and trustworthy a physician, that, after seven years of careful experiment (from 1792 to 1799) he had come to the conclusion that digitalis, with iron for the scrofulous phthisis, with opium for the florid, enabled him often to hope for a cure under circumstances which would formerly have precluded all hope of recovery. On the whole, we may venture to suggest that the endermic introduction of digitalis, by means of a blistered surface, may occasionally prove worth the trial, as an auxiliary measure, at the commencement of some cases of consumption.

The importance of careful and discriminating attention to the peculiarities of each individual constitution must not be overlooked: still there is one medicine so safe, and, in a large proportion of cases, so useful, that I shall be excused for giving it a prominent consideration in the remainder of the lecture. Perhaps there is no

substance, the value of which has been in a short time established by so large an accumulation of evidence, as cod-liver oil. About ten years since, in my office as one of the Fothergillian adjudicators, some remarks in one of the competing essays, regarding the efficacy of this remedy in scrofula, induced me to try the medicine in consumption. As far as I am aware, the earliest extensive trial of cod-liver oil in London was made by me at the London department of the Hospital for Consumption, and the results of that investigation were detailed to this society, the coarse kind, such as is used in tanning leather, being employed. Of the first thirty-seven patients, ten were greatly benefited, and increased materially in plumpness and strength; twelve were slightly improved; twelve remained unchanged; and three were obliged to discontinue the remedy, on account of the nausea which it occasioned. Under the use of this oil all the symptoms were ameliorated; the pulse became slower, the cough and expectoration lessened, the night perspirations abated, diarrhœa ceased, and the urine often ceased to exhibit the excess of uric acid. The diminished liability to hectic in patients thus treated furnished remarkable evidence that this variety of fever has more relation to conditions of blood than to local disorganization. It is obviously a question of great practical interest, whether cod-liver oil owes its efficacy to some ingredient not present in other oils. With a view to determine this question I tried various other oils, such as sperm and seal, and the result was a conviction that fish oils generally resembled one another in their remedial properties, although differing in their aptitude for digestive assimilation in the human stomach. It seemed to me an object of interest to determine whether other animal oils not derived from the liver possessed similar powers; and in the year 1849 I made many experiments in the use of neat's-foot oil. Of the first fourteen patients to whom I administered the substance, seven were essentially benefited (in three the disease was arrested); in five there was no obvious improvement; only two lost ground. The oil of neat's-foot, therefore, can fairly be compared with that of cod's liver, although, being equally disagreeable to the palate of most patients, it is not likely to come into general use. Dr. Radcliffe Hall, of Torquay, having incidentally heard of this remedy, gave it a trial, and published a favourable account of the effects in the Monthly Journal for July, 1852. This physician, not having seen the description of my experiments, his statements furnish an independent confirmation of my conclusions. Neat's-foot oil would appear to be adapted to some cases in which there is too much irritation of the intestinal mucous membrane to bear the administration of the cod-liver oil.

I have also tried an animal oil obtained from a soft, solid fat, found between the parchment and the leather skin of animals. The most remarkable properties of this oil are, its very slight tendency to become rancid, and its freedom from drying properties. It seemed not unreasonable to expect that it might prove as efficacious as neat's-foot oil; but as yet I have not been able to trace any important advantages to its use. I have tried an oil obtained from a species of fish abounding on the Malabar coast, and frequently with encouraging results. The inefficiency of almond and olive oil, excepting when combined with phosphorus, I have already reported. The result of these trials at first induced me to distrust vegetable oils in general; but happening eighteen months since to have a patient engaged in a cocoa-nut manufactory, it occurred to me to try the oil of this nut, which, from the nutritious qualities of the kernel, and the peculiar properties of the soap obtained from the oil, seemed worthy of special consideration. In the first instance I tried the common oil—sometimes the solid, sometimes the fluid part. Subsequently, I have used a purified oil (of which I show you a specimen), which is obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the copperah, or dried cocoa-nut kernel, and refined by being treated with an alkali, which deprives it of its unpleasant smell and taste. It is then repeatedly washed in distilled water, in order to separate any of the saponaceous compound formed by the combination of the alkali with the impurities and rancid matter of the oil. It burns with a feeble blue flame, and is undrying. For the supply of this oil, and for much valuable aid in my investigations, I am indebted to Mr. George Wilson, of Messrs. Price and Company's establishment. Mr. Wilson has given to my suggestions the ready attention of an enlightened mind, and I am happy to bear witness to the value of the sympathy and co-operation which the well-informed

laity can often render in our medical endeavours to apply scientific research to purposes of practical usefulness.

Of the first thirty cases of phthisis in which I administered the cocoa-nut oil, nineteen were much improved, five remained without material change, and only six got worse. Of the last twenty-three, fifteen were materially benefited, three remained unchanged, and five became worse. Three of these five were in so hopeless a condition at the time of commencing the remedy, that my clinical assistant thought it scarcely fair to include them in the report. I was careful, for the most part, to avoid the concurrent use of any other medicine; and it is proper to mention, that a large proportion of the patients had previously taken cod-liver oil, and discontinued it, from being disappointed of benefit.

Another which I have tried is that of the sunflower seed. It is slightly drying, and burns with a smoky flame, showing a large proportion of carbon. Although the sunflower seed is much used for feeding and fattening poultry, the oil has not, medicinally, afforded me much satisfaction. I have occasionally alternated it with the animal fat oil. Each of these oils has occasionally done a little good after the failure of the other, but no results have occurred worthy of record.

I felt curious to ascertain whether chemical science could suggest any explanation of the diversity of remedial properties in different oils, and, with this object, obtained from Mr. Dugald Campbell an ultimate analysis, which is subjoined, of some of the oils which I have employed.

Ultimate Analysis of Oils.

	Carbon.	Hydrogen.	Nitrogen.	Oxygen.
Cod-liver oil.....	80·18	13·72	0·246	5·854
Neat's-foot oil.....	64·33	12·50	0·064	23·106
Cocoa-nut oil.....	69·62	12·49	0·060	17·830
Olive oil.....	69·38	13·47	0·058	17·092

It will be observed that cod-liver oil differs materially from the other oils in its elementary composition. The remarkable difference apparent between the proportions of these elements, especially of carbon and oxygen, in cod-liver oil, as contrasted with the other oils, might lead us to suppose that its superiority might thus be explained; but the inadequacy of such an explanation becomes obvious, when we further pursue the inquiry, and observe a close correspondence in elementary composition between olive oil, which may be regarded, when introduced into the stomach, as, medically, almost inert, and cocoa-nut oil, which is fairly comparable in efficacy with the oil obtained from the cod.

[The class of remedies for phthisis, it should always be remembered, is that which is also adapted to other states of the system, as anæmia in its various forms of scrofula, neuralgia, hypochondria, hysteria, &c.; and in hereditary taints of the blood, we should, as early in life as possible, introduce oleaginous medicines and food into the system, not forgetting the inunction of the body with oily substances.] —*Lancet, August* 5, 1854, *p.* 97.

Dr. Thompson has found, that during the administration of cod-liver oil to phthisical patients their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. Franz Simon to the same effect. The use of almond-oil and of olive-oil was not followed by any remedial effect; but from cocoa-nut oil, results were obtained almost as decided as from the oil of the liver of the cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleine, obtained by pressure from the crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the copperah or dried cocoa-nut kernel, and refined by being treated with an alkali, and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a comparatively small proportion of carbon, and is undrying. The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagu-

lum was drained on bibulous paper for four or five hours, weighed, and divided into two portions. One portion was weighed, and then dried in a water-oven, to determine the water. The other was macerated in cold water, until it became colourless, then moderately dried, and digested with ether and alcohol, to remove fat; and, finally, dried completely, and weighed as fibrin. From the respective weights of the fibrin, and the dry clot, that of the corpuscles was calculated. The following were the results observed in seven different individuals affected with phthisis in different stages of advancement:

		Red corpuscles.	Fibrin
First stage before the use of cod-liver oil ...	Female	129·26	4·52
	Male	116·53	13·57
First stage, after the use of cod-liver oil ...	Female	136·47	5·00
	Male	141·53	4·70
Third stage, after the use of cod-liver oil ...	Male	138·74	2·23
Third stage, after the use of cocoa-nut oil ...	Male	139·95	2·31
	Male	144·94	4·61

Med. Times and Gazette, June 10, 1854, *p.* 607.

16.—*Night-sweats in Phthisis.*—[Mr. Hutchinson, of the City Hospital for Chest Diseases, shows that night-sweats in this disease may be checked with impunity and even benefit.]

Under the usual treatment of phthisis (full diet, cod-liver oil, and tonics,) the tendency to night perspiration often ceases spontaneously. If it becomes desirable to expedite the process, it may be done by the sesquichloride of iron, the mineral acids, or, best of all, by the gallic acid. The following is the prescription for a night-draught containing the latter:—

℞. Acidi gallici gr. vij.; morph. acet. gr. ½; alcohol q. s. (a few drops;) syr. tolutan. ʒss.; aquæ ℥j.

The night-pill, as we find it in the Pharmacopœia of the Brompton Hospital for Consumption, is—

℞. Acid. gallic. gr. v.; morph. hydrochl. g. ½; mist. acac. q.s. Ft. pil. ij.

It is also of advantage to adopt an astringent regimen as far as convenient. The patient should be directed to sleep on a mattress, alone, and not heavily clothed; he should wear no flannel in bed; as dry a diet should be taken as conveniently can be borne, and fluid should be especially avoided in the latter half of the day, none whatever being allowed later than several hours before bed-time.—*Med. Times and Gazette, June* 10, 1854, *p.* 594.

17.—*An Example of the Concurrent Development of Cancer and Tubercle.* By Septimus Wm. Sibley, Esq., Registrar to the Middlesex Hospital.—[There has been a good deal of interest excited of late by the opinion that cancer and tubercle could not co-exist in the same subject at the same time. We have seen several cases reported which made this opinion doubtful, but the following seems conclusive. It may, however, be true, generally, that tubercle and cancer do not co-exist, but it is evident that there are exceptions.]

This was the case of a woman, aged 48, admitted into the Middlesex Hospital, with a sloughing cancerous sore in the left breast; there was a hard tumour on the inner side of the size of an orange, and several small nodules of cancer at its edges. In the course of five days after her admission nearly the whole of the remaining portion of the tumour sloughed away, leaving a clean-looking surface, which immediately began to cicatrize. Subsequently, pulmonary symptoms became developed, profuse expectoration followed, and she sank and died three months after her admission. On making a section of the structure of the left breast, it was seen to be an extremely dense form of infiltrating scirrhus, traces of breast tissue, such as ducts, being very apparent. In the thorax, large masses of tuberculous lung tissue were observed. Tubercular cavities existed in the apices of both lungs; a part of the lower lobe of

the right lung was in a state of grey hepatization, and the bronchial tubes were thickened and dilated. In the left pleura were numerous crude tubercles. On examining the dates of this case, positive proof was obtained that a cancerous tumour was increasing in the breast simultaneously with the increasing of tubercular disease of the lungs, and that for a period of at least six weeks. The author thought that a single instance of the concurrent existence of these diseases was sufficient to destroy the doctrine of the absolute incompatibility of tubercle and cancer with each other.—*Lancet, July* 8, 1854, *p.* 10.

18.—ON PLEURO-PNEUMONIA.

By Dr. R. B. Todd, F.R.S., Physician to King's College Hospital.

[We give the following remarks of Dr. Todd as being very interesting in showing the peculiar symptoms which are sometimes observed in the urine in cases of pneumonia and pleuritis. The expectoration also is worthy of more notice than is generally given to it. When the expectoration is viscid, of a rusty-red hue, adhering firmly to the vessel, we have *sthenic* pneumonia. In the less sthenic, or in the typhoid pneumonia, there will be either no sputa at all, or they will be less viscid, less adherent, and the colour will be *much darker*, somewhat like *prune-juice*. The case which Dr. Todd comments upon was one of pleurisy, accompanied with pneumonia and consolidation of a portion of lung.]

The treatment pursued was of the simplest kind; turpentine stupes were ordered to be applied to the affected part of the chest three times a-day, and half an ounce of the liquor ammoniæ acetatis was given every two hours; beef-tea and milk were allowed as diet. This plan you have now seen me follow in many cases with decided success. Bleeding and all kinds of depletion have been carefully avoided. The bowels have been moderately acted upon; but we have been anxious rather to avoid pulling down the patient, the influence of the morbid process being quite enough for that purpose. A principal object of treatment, as regards physiological effect, was to procure sweating, and to promote the free action of the kidneys. Large doses (from four to six drachms) of liquor ammoniæ acetatis, given at short intervals, almost always operate beneficially in these ways. The cases which we find do not do so well, on this plan, are those in which a large portion of one or both lungs is affected; but these do not get on better under any other plan that I have yet seen adopted, and certainly an antiphlogistic course (so called) is not by any means more successful. When only half or a third of one lung is affected, the patients do very well on the plan I have just referred to.

I have said, that we avoid pulling down our patients—not only so, but, on the other hand, we endeavour to uphold his strength, by giving him, at short intervals, moderate quantities of nourishment, such as his stomach may easily digest; and, if signs of exhaustion or debility show themselves, we often add to these brandy or wine, in small quantities likewise, and carefully adjusted to the patients' digestive organs.

In this treatment of pneumonia no attempt is made to cut short the disease. It is founded upon the observation of the way in which the disease is often spontaneously cured, through critical evacuations of sweat or urine, or of both, and consists in an attempt to promote both these secretions. I am not, indeed, aware of any mode of treatment which can be said to cut short the disease; the plan by bleeding and tartar emetic does not do so certainly. I have heard it stated, that large doses of digitalis will sometimes cut short the pneumonia. Digitalis is an uncertain drug, not always possessing an equal amount of power, and sometimes not very controllable. Moreover, there are certain idiosyncrasies which do not bear the use of digitalis; nevertheless, it is not undeserving of trial in cases chosen with judgment. I hold, that if the treatment is of a simple kind, and at the same time efficient, we cannot wish for more; and I believe we do more good in pursuing such a plan of treatment, than in endeavouring to cut short the disease by remedies whose action is at most uncertain, but which may under certain circumstances actually do harm. By following the plan I have just mentioned, if the patient dies it will be rather from a negation of treatment than from any other cause. The treatment will prove

unsuccessful simply because the disease went on unchecked by it, and in such cases it is often a question if, by any known plan of treatment, such a result could have been averted.

Let me allude to a very interesting point which has been noticed in these cases of pneumonia. It may now be looked upon as an established fact, that in this disease there is either a great diminution in quantity of the chlorides in the urine, or these salts are altogether absent from that fluid. Upon following up this subject, Dr. Beale has made out, that while there is a deficiency of the salt in the urine there is a corresponding excess in the fluid poured out into the lung; in fact, that the chloride appears to be drawn from different parts of the system to the inflamed lung.

The amount of chloride in the urine gradually diminishes up to the period of hepatisation, at which time, in the majority of cases, not a trace is to be detected. As soon as resolution commences, the chloride gradually re-appears until the normal quantity is found. Now, the method of testing the presence of this chloride is so simple that you should always ascertain for yourselves the accuracy of this statement wherever an opportunity occurs. All you have to do is to add a few drops of nitric acid to a portion of the urine in a test-tube, and then a few drops of a solution of nitrate of silver. If chloride be present, a dense white precipitate of chloride of silver, which is insoluble in acids, will fall. If chloride of sodium be altogether absent, no precipitate whatever will occur.

What appears somewhat contradictory of the above statement is, that on the 11th we tested this boy's urine, and found evidence of the presence of much chloride; but on the 12th there was a diminution, and on the 14th the quantity had again increased; so that in this case there was no total absence at any period; and from this circumstance alone we might infer that the hepatisation of the lung was not very extensive. The disease began as pleurisy, and when hepatisation took place, the chloride in the urine diminished in quantity. I may contrast this with the case of Mary Coley, which was one of simple pleurisy of an acute form. The rubbing sound was very loud and well-marked. She was treated with opium and large doses of liquor ammoniæ acetatis, and recovered rapidly. At no period of the case did we find any diminution of the quantity of chlorides in this patient's urine. In this case of pleurisy, then, the chlorides in the urine did not appear to be affected, while in pneumonia the quantity of these salts is observed to undergo a marked diminution. At present, however, we must not generalize too quickly on these interesting points.—*Med. Times and Gazette, July* 15, 1854, *p.* 51.

19.—ON HOOPING COUGH.

By Dr. George D. Gibb.

[Dr. Gibb sums up his views of the nature and seat of hooping cough as follows:]

1. Toxication of the blood, produced by some unknown specific influence, peculiar in its nature, not unlike that of measles and scarlet fever, in the circumstance of its affecting persons once during their lives, generally children under five years of age.

2. Irritation of the terminal loops of the nerves supplying the mucous membrane of the bronchial tubes, producing vascularity and consequent secretion of a greater or lesser quantity of mucus.

3. Reflex action of the pneumogastric and respiratory nerves, followed by congestion of the vessels of the medulla oblongata and pia mater surrounding it, and also at the origins of its nerves.

4. Spasmodic contraction of the circular and longitudinal muscular fibres of the bronchi, consequent upon the foregoing, manifesting itself in the series of sudden expiratory efforts, and the well-known sonorous back-draught of hoop.

5. The immediate result of which is frequent and rapid respiration to compensate for its temporary absence, producing a highly oxygenated or super-oxidized state of the blood, with a tendency to the formation of fibrinous concretions in the heart during the spasms.

6. As a secondary result of the spasmodic muscular contraction of the bronchi, we have a temporary hypertrophy of the muscular fibres thus acted upon, which disappears again after the cure is established.

7. The disease is at first irritative and catarrhal, and afterwards nervous and spasmodic, both due to the unknown peculiar exciting cause present in the blood.

8. It manifests the peculiarity of running a special course through its different stages, three in number, which may be cut short, or greatly diminished by medicinal treatment.

[Dr. Gibb next brings forward his remedy, which he considers nitric acid to be.]

It would be presumption in me to say that this substance has been discovered, but in its effects upon the disease, *nitric acid*, in whatever manner administered, not only arrests the paroxysms, and removes the hoop, but shortens the disease almost as effectually as quinine does intermittent fever.

It not only produces a powerful antispasmodic effect, but an equally tonic influence, and supplies to the blood an element—nitrogen—which removes or neutralizes the excess of fibrin existing in that fluid—one of the dangerous elements of the disease—and so destroys the poisonous principle combined with it, which is the primary cause of the affection.

If this remedy cures pertussis, it differs from the great majority of others in the fact that its action in the economy can be explained upon rational grounds.

Nitric acid possesses antiseptic properties in a high degree, and probably the influence of these assists materially in the cure by acting directly on the blood, as well as supplying its nitrogen, and prevents the too rapid generation of the fibrin from the albumen.

The nitrogen acts also as a sedative, in diminishing the stimulating effect of the oxygen so rapidly absorbed, and as an antispasmodic in allaying irritation, and therefore lessening the severity of the paroxysms. If hydrocyanic acid is useful in many cases, it is as much owing to its nitrogen as the sedative and antispasmodic properties which it possesses.

The presence of the excess of fibrin has been elsewhere explained to depend upon the frequent inspirations, immediately after the paroxysms have ceased: it may also be influenced by the peculiar poison itself which has contaminated the blood. Whatever remedy will check this tendency materially assists in the cure; it is in this manner that ether and chloroform act by arresting the process of sanguineous super-oxidation, and, with the other properties possessed by those agents, equally assist in the cure.

As a general tonic, nitric acid possesses the advantages in pertussis of rapidly allaying the dyspepsia, which is sometimes accompanied with sickness and irritability of the stomach; it restores the healthy state of the mucous membrane of the bronchi, it arrests the spasm, diminishes the cough, and finally dispels the hoop altogether.

Dr. Arnoldi and myself have never met with any ill effects from its use, and all the cases treated by it were carried to a successful issue, the cure in all being very speedy, with few exceptions, and the disease being at the same time abridged in the length of its ordinary duration. So far as experience has shown, relapses after cure are very rare indeed.

Should inflammation of the lungs or abdominal organs have set in previous to its employment, it must be avoided, and the treatment must be adopted as recommended for those complications in the next chapter.

But if we have reason to fear congestion of the brain or convulsions, the acid may still be safely given, and with good results, for the purpose of lessening the paroxysms. It will also prove useful in the remittent fever, combined with other substances, if there be no existing irritation.

The details of a few cases only will now be given to illustrate the successful effects of this remedy, as all possess a similarity in their general features.

My friend Dr. Arnoldi has treated upwards of one hundred cases of pertussis with nitric acid, with the most satisfactory results; and since I commenced its use, sixty-seven cases were cured at intervals varying from two to fifteen days, but averaging between six and seven days.

It may be as well to mention here that the object entertained by Dr. Arnoldi, in using this acid as a remedy in pertussis, was to introduce the elements of the atmosphere into the blood by the process of gastric digestion, so as to enable the lungs to outstand the stage of temporary asphyxia, which always is induced during a severe

paroxysm. Whether the theory be correct or not, the result, he says, of his practice has been almost universally successful.

I think it not improbable that the greater portion of the oxygen is chemically appropriated before the blood becomes sensibly relieved by the nitrogen; because if it were not so, a still greater amount of fibrin would be formed, and a tendency to aggravation by inflammatory complication would be the result.—*Lancet, Aug.* 12, 1854, *p.* 125.

DISEASES OF THE ORGANS OF CIRCULATION.

20.—ON CHRONIC HEART DISEASE.

By Dr. Alexander Harvey, Physician to the Aberdeen Royal Infirmary, &c.

In the following notes, an attempt is made to exhibit, within as small a compass as possible, and in a form approaching the tabular, the leading facts connected with the pathology, diagnosis, and prognosis, of the more common organic affections of the heart. Imperfect as the notes are, the author is not without hope that they may be of service to some of his colleagues in the Association, who, though familiar with the subject of heart-disease, may not always be able at once or readily to call to mind such particulars respecting it as they must often have occasion for in the course of their practice, and which they may not always have time or opportunity to collect from larger treatises.

It may be added, that the notes were originally drawn up for the students attending the author's class, and delivered to them as a recapitulation of the facts just previously detailed at greater length in the lectures; and that the materials of them are chiefly taken from Dr. Watson's 'Lectures' and Dr. Allison's 'Outlines of Pathology and Practice of Medicine,' the works used in the class as text-books.

I. DISEASES OF VALVES AND ORIFICES.

I. The valves and orifices of the left side of the heart are at least twenty times more liable to disease than the valves and orifices of the right side.

Disease of the semilunar pulmonary valves is so rare as to be matter of pathological interest merely. Disease of the tricuspid valves is more frequently, although still seldom, met with; and still seldomer without coexisting disease of either the aortic or mitral valves.

II. In early life and in women, the mitral valve and corresponding auriculo-ventricular orifice are oftenest diseased; in advanced life and in men, the aortic. In many cases, at all ages and in both sexes, the two sets of valves and openings are simultaneously diseased.

III. The diseased states of the valves and orifices are mostly the result of (*a*) antecedent endocarditis of rheumatic origin, and consist of concretions forming on the valves; or (*b*) mere perversion of nutrition, independent of inflammation, and occurring in connexion with atheromatous and other kinds of deposit in the aorta and arteries generally.

IV. The effects of these changes on the functions of the valves and orifices are two-fold:—

1. By contracting and narrowing the orifice, to impede or obstruct the passage of the blood (*valvular obstruction, obstructive disease of valves*).

2. By thickening, corrugating, and shortening the valves, to make the orifice more or less permanently patent, and allow regurgitation or reflux of blood (*valvular insufficiency, valvular patency, regurgitant disease of valves*).

V. Valvular obstruction and valvular insufficiency often exist separately, but are oftener coexistent in the same valve. Each of them may obtain in very different degrees, and the two may be very variously combined.

VI. In a very few cases, the cause of patency is preternatural widening of the orifice without disease, but with no adequate extension by growth, or the valves; and, in a few others, it is owing to disease with shortening of the chordæ tendineæ, the valves themselves being sound.

II. DISEASE OF WALLS AND CAVITIES (*Hypertrophy and Dilatation*).

I. Hypertrophy and dilatation may be either partial or general, oftenest partial. Dilatation, unaccompanied by hypertrophy, is rare.

II. Hypertrophy and dilatation are very generally *secondary* morbid states. They may be produced by various causes:—

1. In a very few cases, by unknown causes, or causes unconnected with obvious change of structure elsewhere (*primary*).

2. By adhesion of the pericardium to the heart, consequent on plastic exudation from pericarditis.

3. By preternatural widening, with or without change of structure, of the ascending aorta.

4. By diseased states of the valves. This is the most common cause.

III. The *rationale* of their production may be thus stated:—

1. Disease of the valves, widening of the aorta, adhesion of the pericardium, create a demand for increased action of the heart; and this increased action, in a constitution otherwise healthy, leads to increased nutrition of the organ. The walls of the heart are thereby gradually increased in bulk and in contractile power (*simple hypertrophy*).

2. When, along with this increased action, there is (*a*) a full quantity of blood in the body, the cavities of the heart are gradually dilated at the same time that their walls are thickened (*hypertrophy with dilatation, eccentric hypertrophy*). But (*b*) when the quantity of circulating blood is much diminished, the tonic contraction of the thickened fibres of the heart, which are no longer distended, leads to a diminution in the size of its cavities (*concentric hypertrophy*). This is rarely met with, except either as a congenital affection, or, in connection with the *rigor mortis*, as a temporary *post mortem* condition.

3. Dilatation without hypertrophy, consequent on obstructive disease of the valves, occurs chiefly (*a*) in a weakly habit of body, when the excitement of the heart by the preternatural amount of its stimulus is feeble; but (*b*) it may be produced, in a habit otherwise healthy, by mere morbid debility of the muscular fibres of the heart, as after some cases of rheumatism affecting them.

IV. In the great majority of cases it is the left ventricle that is the first and chiefly affected, and hypertrophy and dilatation combined (eccentric hypertrophy) that obtains, and in connexion with pre-existing mitral or aortic disease. With mitral disease, there is commonly rounding of the apex, and often simple hypertrophy merely; with aortic disease, there is elongation of the apex, and commonly eccentric hypertrophy.

Pure dilatation may affect either ventricle, oftenest perhaps the right, and that chiefly in females, and in such cases seems to occur in connexion rather with disease of lung (*e.g.*, habitual asthma) than with diseased valves.

III. DIAGNOSIS OF CHRONIC HEART DISEASE.

Diagnosis of Disease of Valves and Orifices. Practically, in at least nineteen out of twenty cases, the questions to be determined are whether it be the mitral or aortic valve that is diseased, or both; and whether the disease be of the nature of valvular obstruction, or of valvular insufficiency, or both. The diagnosis is mainly founded on these two things: first, a bruit or murmur heard, called, from its blowing character, *bellows murmur*, or, from its seat being within the heart, *endocardial* murmur; and secondly, the state and character of the pulse at the wrist.

I. *Bruit or murmur.* Its import varies according (*a*) to the time when it is heard, (*b*) the place where it is best heard, and (*c*) the direction in which it is conveyed furthest. The second and third may be taken together, and included under one head.

(*a*) *The time when it is heard.* This may be either with (or rather in place of) the first natural sound (systolic bruit); blood then passing from the left ventricle into either the aorta before, or the auricle behind; or with the second natural sound (diastolic bruit); blood then passing into the ventricle, from either the aorta before, or the auricle behind.

(*b*) *The place and direction in which it is best heard and conveyed furthest.* This

may be either over the base of the heart, and thence upwards along the aorta and carotids, or at and in the direction of the apex. Accordingly—

1. Bruit, systolic and loudest at base, indicates obstructive disease of the aortic valves or orifice.

2. Bruit, systolic and loudest at apex, indicates mitral insufficiency (patency) with regurgitation.

3. Bruit, diastolic and loudest at base, indicates aortic insufficiency with regurgitation.

4. Bruit, diastolic and loudest at apex, indicates mitral obstruction. Rarely if ever heard.

*** Various combinations of these murmurs are sometimes heard, constituting double bruits. Their import is to be gathered from that of the single murmurs. Thus, *e. g.*, there may be a systolic bruit, equally loud at the base and at the apex, indicating at once aortic obstruction and mitral insufficiency; or there may be a systolic and a diastolic bruit, both loudest at the base, and indicating aortic obstruction and aortic regurgitation.

II. *The Pulse.*

1. Pulse at wrist irregular, intermittent, unequal, and soft, small, or weak, indicates mitral disease, obstructive or regurgitant, particularly the latter, on which this condition of the pulse is specially dependent.

2. Pulse regular, full, or hard, indicates aortic disease; if jerking and resilient, aortic insufficiency (with hypertrophy).

The foregoing (I. and II.) may be thus briefly represented on a card carried in the pocket.

> BRUIT: If *systolic* and loudest at
> *Base*=AORTIC *obstruction.*
> *Apex*=MITRAL *insufficiency.*
> BRUIT: If *diastolic* and loudest at
> *Base*=AORTIC *insufficiency.*
> *Apex*=MITRAL *obstruction.*

And on the reverse side of card:—

> PULSE: If *regular*, Full, or strong, Jerking, resilient } =AORTIC disease.
> PULSE: If *irregular*, Intermittent, unequal, Soft, small, weak } =MITRAL disease.

Diagnosis of Disease of Walls and Cavities.

I. Of Hypertrophy.

1. The action of the heart preternaturally, sometimes excessively strong, with corresponding impulse felt on laying the hand or the ear over the præcordial region.

2. The sounds of the heart's action, and especially the first sound, less audible than natural,—obscured and muffled.

II. Of Dilatation.

1. The action of the heart weak, somewhat undulatory, with indistinct impulse.

2. The sounds of the heart's action, and especially the first, preternaturally clear, short, or abrupt.

III. Of Hypertrophy and Dilatation.

With various modifications, as the one or the other condition predominates, of the abnormal impulse, and the abnormal sounds of the heart's action.

1. The area of the natural dulness on percussion increased, often with squareness of outline of this area.

2. The apex of the heart seen and felt to beat lower down than natural, often as low as the seventh or eighth ribs.

3. The præcordial region in some cases obviously bulging and prominent.

IV. PROGRESS AND RESULTS OF CHRONIC HEART DISEASE. PROGNOSIS.

I. It is important to discriminate between (*a*) the affection of the heart itself, and (*b*) the secondary affections,—*i.e.*, affections of other parts and organs resulting from it.

(*a.*) As time advances, there occurs a gradual aggravation of the heart affection; an increase of the valvular obstruction or of the valvular insufficiency, or of both; and likewise an increase of the hypertrophy or of the dilatation, or of both.

(*b.*) Sooner or later secondary affections, of various kinds, extrinsic to the heart, supervene, *e.g.*

1. Dyspnœa; bronchitis; pneumonia; pulmonary hemorrhage, with or without pulmonary apoplexy.

2. Enlargement of the liver, or of the liver and spleen; hæmatemesis.

3. Cephalalgia; tinnitus; vertigo; syncope; epilepsy; epistaxis; cerebral hemorrhage; apoplexy; palsy.

4. Dropsical effusion; œdema of face, or of face and hands,—of lungs; hydrothorax; ascites; anasarca or general dropsy.

5. Various combinations and successions of the foregoing.

II. Both the aggravation of the heart disease and the supervention of some secondary affection take place more surely and more quickly, in young and full-blooded individuals, than in those already old, or thin or emaciated.

III. As to the relation that obtains between the secondary affections and the diseased heart—

1. The connecting link between them is the altered (abnormal) condition of the general circulation produced by the state of the heart. This condition may be one or other, or a combination of three kinds: (*a*) impeded venous current behind (pulmonic and systemic), as in mitral disease, whether obstructive or regurgitant; or (*b*) undue impulsion of the arterial current in advance, as in hypertrophy; or (*c*) too feeble impulsion, as in purely passive dilatation of the ventricle.

2. Nevertheless, the diseased state of the heart and these effects of it on the general circulation are commonly only *permanent predisposing* causes, not the direct or exciting causes, of those secondary affections,—the immediate causes of them (often in a great measure *avoidable*) being intemperance, over exertion and fatigue, mental emotion, and especially exposure to cold or cold and wet.

3. Some of the secondary affections are themselves the occasion, or concurrent and co-operating causes, of certain other secondary affections, *e.g.* bronchitis—of œdema of the lungs, or of general dropsy; enlargement of the liver, or of the liver and spleen—of ascites, hæmatemesis, epistaxis, apoplexy; or consolidation of lung and enlargement of the liver combined, with bronchitis—of general dropsy.

IV. The worst kind of heart affection is that in which there is at once obstructive and regurgitant disease of the mitral valve, because the most effectual in deranging the general circulation, and the most fruitful therefore of secondary affections. The least unfavourable is a diseased state of the aortic valve only, with moderate hypertrophy of the left venticle; the hypertrophy, though reckoned a diseased state, being, in fact, a provision of nature for overcoming the resistance offered to the exit of the blood.

Insufficiency of the mitral valve, however, with much hypertrophy of the left ventricle, is very hazardous; the hypertrophy in this case enhancing the evil naturally attendant on the patency of the valve.

V. When death ensues, the fatal event may take place suddenly, in the way of syncope, and be directly referrible to the diseased state of the heart; or it may take place more gradually through the medium of some secondary affection, and in very various modes,—in the way of asthenia, or in the way of coma, or in the way of asphyxia,—according to circumstances.

VI. The prognosis in any case must mainly be based upon, and may in general readily be determined from,—

1. The kind and extent of heart affection.

2. The tendency that may appear towards secondary affection; and the kind and extent of this.

3. The age and bodily habit of the patient.

4. The degree to which the habits and circumstances of the patient can be controlled and regulated, and the probable efficacy of the remedies applied, with the view in respect of them all (*a*) of retarding the progress of the heart affection,—(*b*) of warding off secondary affections,—and (*c*) of removing those when they supervene: these several objects furnishing at the same time the main indications of treatment.—*Association Med. Journal, Sept.* 1, 1854, *p.* 785.

DISEASES OF THE ORGANS OF DIGESTION.

21.—ON EPIGASTRIC NEURALGIA.

By Dr. Lees.

[On the *slightest* pressure in the epigastric region great pain is felt in these cases—not occupying a space above the size of a five shilling piece. The following illustrates this kind of case, which might easily be mistaken by the young practitioner:]

He says that he never experiences pain in the stomach, but that, after eating, he has been often obliged to lift his shirt from contact with the epigastrium, as the tenderness caused by even its apposition was intolerable, although it was not aggravated by increasing the pressure. The state of his general health and appetite is good; functions natural; tongue clean; bowels regular: no thirst, nausea, or vomiting. He does not suffer from flatulence, has no headache, but is very low spirited. The circulation is languid, pulse 56; heart's action feeble, but regular, and without any murmur; the abdominal muscles are in a state of constant tension, hard and rigid; there is no evidence of any tumour, nor abnormal pulsation in the epigastric region; neither pain nor tenderness in the back, nor in the course of the intercostal nerves. His easiest posture is on his left side, with the right leg drawn up. When he lies on his back with the right leg extended, the spine is arched, so that the hand can be passed freely under it, but it becomes quite flat and in contact with the bed, on the leg being flexed. The urine is of a natural appearance, sp. gr., and reaction; no visible sediment, but on microscopic examination there are seen numerous very minute crystals of oxalate of lime aggregated in long masses.

He could not assign any cause for his complaint, except that he has been constantly employed in a very dusty meal-room, but he has neither suffered from cough, nor from any difficulty of respiration.

Tincture of aconite was applied over the tender part, but without any relief. I then ordered:—

℞. Infusi valerianæ, infusi cinchonæ, āā ℥ iij.

Sumat ℥ i. ter in die; a small blister over the part, and to be dressed with acetate of morphia ointment, gr. i. ad ℥ i.

He was allowed meat every day, and advised to walk about and amuse himself. Under this treatment he rapidly improved, and was nearly quite free from his complaint when I resigned him to the care of Dr. Stokes. He, however, unfortunately relapsed, but was cured eventually by continuing the same medicine, and by the repeated application of leeches over the part. If the subject of the above case had been an unmarried female, I should have had little hesitation in regarding it as one of those anomalous forms of hysterical affection occurring in that class of persons; but I confess that at first I felt in doubt as to the cause of this man's sufferings. The superficial seat and character of the pain evidently showed that it was not gastralgia, while the absence of thirst and of fever satisfied me that there was no inflammatory action in the mucous membrane of the stomach. There was no evidence of inflammation in or under the skin, nor in the sheath of the rectus muscle, as occurred in a case under the care of Dr. Neligan, and which presented nearly similar symptoms. My case was examined carefully for aneurism of the abdominal aorta, for disease of the spine, and for psoas abscess, as the peculiar arching of the spine in the horizontal position led us to suspect that there was some change of structure

in these parts which might account for this occurrence; but the most careful investigation by Dr. Stokes and myself could not detect any evidence of disease there, nor in the circulatory or respiratory apparatus. I, therefore, came to the conclusion that it was a form of neuralgia, and the result of my treatment satisfied me that I was right.

Dr. N. Gueneau de Mussy, in a late number of the 'Archives Generales,' has described a species of tenderness very similar to this, as frequently occurring in diaphragmatic pleurisy, but in the cases recorded by him the patient suffered from severe dyspnœa and orthopnœa, whereas, in my case, the respiration was tranquil. M. Beau has described a variety of rheumatism in the skin, which he terms dermalgia, and which bears much resemblance to this case, but differs from it in occupying a greater extent of surface, and in occurring chiefly on the extremities.—*Dub. Hosp. Gazette, Feb.* 1, 1854, *p.* 2.

22.—ON SOME REMEDIES FOR STOMACH DISORDERS.

By Dr. George Budd, F.R.S., Professor of Medicine in King's College.

[In his 8th lecture on disorders of the stomach, Dr. Budd makes some useful remarks on the use of the mineral acids in various kinds of indigestion. But these have been so well treated of by Dr. Prout and others, that we will here only give Dr. Budd's remarks on *vegetable bitters.*]

Quinine, and the bitters generally, are especially grateful to persons who have injured their stomachs by hard drinking. With such persons they improve the appetite and strengthen digestion, and have a bracing effect upon the system at large.

In persons exhausted by over-work, or wherever weakness of the stomach is the result of general debility from other causes, they often do much good in the same way—*by improving the appetite* and strengthening the digestion.

They do harm in the organic diseases of the stomach; in plethoric states of the system; and generally where there is a furred tongue, or where the urine throws down a sediment of lithic acid, or of lithate of ammonia. Their most striking effect is, to improve the appetite, when this has been impaired from hard drinking or from over-work, or from nervous exhaustion from other causes; and the best time for giving them is from half an hour to an hour before meals.

The different bitters have not precisely the same effect. Calumba has a sedative influence not possessed by the others, and probably on this account has had a wider reputation as a remedy for mere indigestion. Gentian and chiretta (which is of the gentian tribe, and is much employed by practitioners in India) tend to increase the secretion of the liver, or, at any rate, do not impede the secretion of the liver, which quinine and quassia seem often to do. They are, therefore, better suited to bilious persons, and to those cases of indigestion where the secretions of the liver are defective.

The different preparations of steel are especially useful in the indigestion that occurs in chlorosis, and generally where weakness of the stomach results from anæmia.

They do harm in plethoric states of the system, and generally where there is a furred tongue, or where the urine throws down a sediment of lithate of ammonia or of lithic acid.

The citrate, or ammonio-citrate, is the most agreeable preparation to the taste, and generally the most grateful to the stomach. If there be any disposition to sickness or nausea, or any tendency to furring of the tongue, it may be given in conjunction with the bicarbonate of soda or potash. This makes a mixture having much the same effect as Griffith's mixture,—the mistura ferri composita,—and far more agreeable.

The muriated tincture of iron is more astringent than the other preparations, and may be given in conjunction with dilute muriatic acid, in the forms of indigestion suited to this latter medicine, when these exist in states of anæmia.

The sulphate of iron, like other metallic sulphates, has a tendency to cause sickness, and should not be given in cases where a disposition to sickness exists.

Steel medicines do good by improving the quality of the blood rather than by their immediate action on the coats of the stomach, and are best given at meal

times. They then are mixed with the food, and gradually absorbed with the products of digestion, and are less apt to offend the stomach and to cause headache than at other times.

Whenever steel medicines are given, it is essential that a regular action of the bowels be kept up. These medicines tend to confine the bowels and to cause evolution of sulphuretted hydrogen in them; and, unless this tendency be counteracted, they are apt to fur the tongue and cause headache.

The choice of purgatives is a very important matter in stomach disorders. The different purgatives exert their chief action on different portions of the intestinal canal: some excite the secretion or the peristaltic movement of one part, some of another. In disorders of the stomach and bowels, where a purgative is required, care should, therefore, be taken to select those which are least prone to irritate the injured or disordered part.

Castor-oil, for example, offends the stomach, but acts very mildly on the large intestine. It should not be used in stomach disorders, or where, from any cause, a tendency to vomiting exists; but is better than any other purgative in dysentery, or during convalescence from typhoid fever, when the intestines are ulcerated, and in various other conditions where a speedy and sure purgative, and one not apt to irritate the large intestine, is required.

Senna acts chiefly on the small intestine, and, besides exciting its peristaltic action, increases the secretion from its mucous membrane. It acts, also, on the liver, increasing the secretion of bile. In conjunction with a few grains of calomel or blue-pill, it is, as every one knows, one of the best purgatives in bilious states of the system, or where an evacuant is required; but in mere disorders of the stomach, it is often objectionable, from the tendency it has to cause sickness.

The best purgatives in stomach disorders are aloes and colocynth, which exert their chief action on the large intestine. These medicines may do much harm when the large intestine is ulcerated or inflamed; but in simple ulcer of the stomach, and in the most severe functional disorders of the stomach, they may generally be given without causing either pain of the stomach or sickness. In some kinds of functional disorder of the stomach, aloes seem, indeed, like other bitters, to improve the appetite and strengthen digestion.

Aloes appear to act more exclusively on the large intestine, and irritate the stomach less than colocynth, and hence, in stomach disorders are generally preferable to it.

Where, from the existence of piles, or from pregnancy, or some other condition, these medicines are objectionable, the best substitutes for them in stomach disorders are the saline purgatives, which exert their chief action on the small intestine, and have little tendency to cause pain in the stomach or sickness.—*Med. Times and Gazette, April* 29, 1854, *p.* 425.

23.—*Lactic Acid in Dyspepsia.* Dr. C. Handfield Jones, in the Association Medical Journal, advises the use of lactic acid in dyspepsia. He has chiefly given it in cases of irritative dyspepsia, where the digestion was painful and imperfect, and had been so for some time; he does not advise its use at the commencement of the treatment in a severe case, but only after the irritation and vascular erethysm is somewhat reduced. It should be employed in doses of fifteen to twenty minims, in a half ounce of water, and taken *at* meal times; he states that it seems to mingle with the food, and to supply one of the constituents of healthy gastric juice, which is probably imperfectly produced. Its use need not be confined to cases of dyspepsia, but may be extended to all cases where it is desirable to improve the tone and power of the stomach. It is pleasant, occupies but little space, and the only objection to its use is its present high price; but if much employed it could probably be obtained cheaply.—*Dub. Hosp. Gazette, Sept.* 15, 1854, *p.* 254.

24.—ON DILATATION OF THE STOMACH AS CONNECTED WITH SARCINÆ.

By Dr. R. B. Todd, F.R.S., &c.

[One peculiarity of the vomiting of sarcinæ is that the fluid soon begins to ferment,

and looks like yeast. It is strongly acid, and by means of the microscope the sarcinæ are easily seen, often accompanied by torulæ. Dr. Todd describes the fermentation as follows:]

It is a special characteristic of this peculiar form of vomit, and one which should particularly arrest your attention, that it begins to ferment immediately after its rejection; and to such an extent does this fermentation frequently proceed, that if the contents have been received into a basin, they will be found within half an hour after their rejection, to exhibit more than twice their original bulk, and will form a soft, spongy head or scum, such as occurs on the surface of saccharine fluid to which yeast has been added. In consequence of this peculiarity, this vomit has been termed, aptly enough, "yeasty vomit."

It is in the brownish matter which exists so abundantly in this vomit, that you will find, in great multitudes, the vegetable bodies to which the name of "sarcinæ" has been applied.

Besides sarcinæ, we frequently find a great number of torulæ of the same kind as those vegetable organisms which exist so abundantly in yeast. They certainly seem to occur in the majority of the cases in which sarcinæ are found.

Sarcinæ may occur in small numbers in the stomach, as is probably often the case in diabetes mellitus; but then the vomited matters will not exhibit the process of fermentation to the extent which existed in this case. They have likewise been found in other places besides the stomach, such as in the bowels, the bladder, and in the ventricles of the brain; whence we may infer, that the conditions favourable to their development may be obtained in other organs, and that these bodies are not peculiar to the stomach.

[On a former occasion Dr. Todd pointed out the fact, that the vomiting of these sarcinæ with abundant yeast-like matter, is very frequently accompanied with a more or less dilated state of the stomach. This is a very important diagnostic symptom.]

This dilatation generally depends upon an obstruction to the onward passage of the contents through the pylorus, in consequence of which they accumulate in the stomach. No doubt a considerable absorption takes place by the walls of the stomach, as in health, otherwise the patients would soon die of exhaustion; but a large quantity of dissolved and undissolved food remains for three, four, or five days, or even for a longer time, and never passes into the bowel at all; it accumulates, until the stomach, impatient of its contents, is stimulated to contraction, and at once disgorges the whole of its contents. The ejected matters immediately ferment, and in the yeast-like mass we invariably find the sarcinæ. Now, I do not affirm that sarcinæ are never found but in this fermenting matter; on the contrary, it is well known that they occur in fluids of the stomach, and elsewhere, without signs of fermentation, but *only in small number*. What I think we are justified in affirming, from my own cases, and many others since published, is, that *this peculiar yeast-like vomiting, with its myriads of sarcinæ*, is characteristic, and I would almost add pathognomic, *of a dilated state of stomach, with more or less of obstruction at the pylorus*. The most common cause of the obstruction and the consequent dilatation in the cases which have fallen under my own observation, has been the contraction of the cicatrix of a former ulcer, or the presence of recent ulcers, with hard thickened bases, and thickening of the submucous areolar tissue. Other causes might produce a similar effect, such as a cancerous tumour surrounding the pylorus, or a tumour simply pressing on it.

Our patient went on for some time without exhibiting any change in the character of the symptoms, excepting that, by careful feeding, the urgency and frequency of the vomiting were diminished. He was fed exclusively on that kind of food which is readily digested by the stomach itself, such as milk, beef-tea, or other animal broths; and he was not allowed to take more than a very small quantity at a time. After he had been sufficiently long in the hospital to afford us full opportunity of observing his exact condition, I determined to try the effect of the sulphate of soda, which has lately been ingeniously suggested by Dr. Jenner as applicable to these cases. The peculiar virtue of this remedy consists in its power of destroying vegetable life; and, if the symptoms depended on the development of these organisms in the stomach, owing to some peculiar process of fermentation, this drug would be an invaluable agent in the hands of the practitioner.

But I have yet to be convinced that the peculiar sarcina-vomit can be regarded in any other light than as the effect of an organic disease of the stomach. This is the conclusion at which I arrived when I discussed this subject before. It seemed to me, therefore, that, in the treatment of the cases, the practitioner need not concern himself much about the presence of these bodies.

The result of our trial of the sulphite, while it confirmed the efficacy of that drug as an agent destructive to vegetable life, also gave support to the view I expressed, that the stomach symptoms were not dependent on the development of the sarcinæ. On the 3rd of Jan., the patient had vomited a large quantity of brownish-looking matter, which fermented freely, and contained sarcinæ in great numbers. On the 5th, he began to take the sulphite of soda, the report on that day being that he had not felt sick since the vomiting on the 3rd.

We gave him half a drachm of the salt dissolved in an ounce and a half of water every four hours, and, after he had taken the remedy for two days, we obtained these results. In the evening of the 6th he vomited freely of a brownish liquid, and about four o'clock in the afternoon of the 8th, he was again very sick, and vomited a large quantity of a similar fluid. On the 8th, the quantity of the sulphite was increased to a drachm. On the 10th, the man vomited about three pints of a brownish liquid, which fermented slightly. In this we were unable to detect sarcinæ; still some few might have been present and have escaped observation, as, in the microscopical examination of such fluids, we are only able to subject a very minute quantity to observation. The vomiting then continued as before, but, upon subsequent examinations, we were unable to detect the presence of sarcinæ. On the 10th, 12th, 13th, and 14th, the man vomited, although he was taking large doses of the sulphite of soda. During all this time he suffered very much from a burning pain referred to the lower part of the œsophagus and the cardiac orifice of the stomach.

After the sulphite of soda had been taken for nine days without any manifest alleviation of the symptoms, its use was given up; and he was put upon alkalies and opium. On the 16th, the report says he had not vomited for forty-eight hours, and the pain was less. He was then taking half a drachm of the bicarbonate of potash, with five grains of nitre dissolved in an ounce and a half of water twice a day, and he was allowed four ounces of brandy.

On the 17th, the vomiting returned, and forty-two ounces of fluid were rejected, in which sarcinæ were present in considerable numbers.

On the 19th, vomiting occurred again, and he appeared much weakened and exhausted.

On the 22nd, he vomited a large quantity of black blood, and he died on the 24th.

The disease in this case was limited to the stomach. This organ was a good deal dilated, although rather less than, when distended with gas during life, it appeared to be. In the pyloric region you may observe two ulcers, each as large as a half-crown piece, with considerable thickening of the submucous tissue. These ulcers were placed opposite each other, one on the anterior, the other on the posterior wall, and, like two fungoid growths, came in contact, obstructing the channel of the pylorus, which with difficulty admitted the passage of the little finger. Here, then, was obviously the cause of the obstacle to the passage of the food, and of the dilatation of the stomach; and here, too, was the cause of the generation of the peculiar yeast-like matter.

I have thought it right to bring this case under your especial notice, as affording additional evidence in favour of the view which I expressed in 1851, that the sarcina-vomiting, when taking place in large quantity, may be regarded as diagnostic of dilatation of the stomach, and, with great probability, of pyloric obstruction too. Whether this peculiar form of vomiting in large quantities occurs simply from functional derangement of the stomach, I have yet to learn. Sarcinæ are undoubtedly formed in the stomachs of diabetic patients (which are always large), and perhaps also in the bowels. But in none of these cases do we meet with the extraordinary yeast-like vomiting.

A second reason for my calling your attention to this case is, that it has served to furnish a very clear indication of the value of the sulphite of soda in cases of this description. This drug certainly was destructive to the sarcinæ, but it did not in any degree alleviate the suffering of the patient. As a remedy, it may prove of

great value if in any case the generation of sarcinæ can be shown distinctly to be the sole or the principal cause of the symptoms, or to aggravate those symptoms. Our patient, however, experienced relief chiefly from a regulated diet, from opium, and alkalies.

Nevertheless, the sulphite of soda is an addition to our resources, and must not be rejected; nor must we expect too much from it. When fermentation is too active, it may prove useful in checking or completely destroying it.

On the 5th of April, 1853, a case of enormously dilated stomach was detailed to the London Pathological Society by Dr. Miller for Mr. Humby. The stomach was capable of containing ten pints and a-half of fluid. Dr. Miller found abundant specimens of the sarcina ventriculi in the rejected fluids. Two very able physicians arrived at different conclusions as to the diagnosis of this case. Had the nature of the vomiting been taken into account by both, it is probable that they would have formed the same diagnosis.

Several cases of sarcina-vomiting have been put on record by Dr. Ransom, of Norwich, in a valuable paper, published in the 'Medical Times' for Nov. 12 and 19, 1853. In most of these pyloric obstruction and dilated stomach existed, and, from an analysis of a large number of cases, Dr. Ransom infers that sarcinæ very commonly co-exist with the following group of symptoms, viz., obstinate chronic vomiting of large quantities of an acid, watery fluid, which forms a scum on the surface; epigastric pain, constipation, and wasting. And he adds, "that the cases which have occurred presenting this group of symptoms have been pathologically most varied; but *pyloric obstruction* either proved by *post-mortem* observation, or inferred from the symptoms during life, was *common to all.*

In the months of March and April, 1853, a woman named Laws, aged 30, was in the hospital, in whom the symptom of chronic vomiting of yeast-like matter with sarcinæ was very prominent. The case was diagnosed as one of pyloric obstruction and dilated stomach. She died early in May. The stomach was found much dilated, and readily held eighty-one ounces of water. An indurated condition of the pylorus by cancerous deposit was found to be the obstructing cause. The pylorus was so constricted that the little finger up to the first joint only could be introduced through the orifice of the stomach into the duodenum, and that with difficulty.

In December, 1852, and January, 1853, we had in the hospital another case of chronic vomiting, with extreme emaciation,—a woman named Taylor, aged thirty-three. In this case there were no sarcinæ, nor were the vomited matters yeast-like, nor could we by palpation or percussion discover any evidence of a dilated state of stomach. A tumour was detected near the pylorus, and after death this proved to be cancerous, compressing the pylorus and the pancreas; but there was no dilatation. Here, then, was an instance of obstructed pylorus without sarcinæ, and these were absent probably *because* there was no *dilatation.*

For excellent notes of both these cases I am indebted to my clinical clerk, Mr. Plowman.—*Med. Times and Gazette, July* 1, 1854, *p.* 1.

25.—*Sarcinæ Ventriculi.* By Dr. W. L. Lindsay, Dumfries.—[From the subjoined remarks of Dr. Lindsay, it will be seen that the presence of sarcinæ in ejected fluids from the stomach, does not prove so surely as some writers assert, the existence of pyloric obstruction.]

Sarcina appears to me to be of interest chiefly as indicative of a disordered condition of the intestinal mucous membrane, as showing its proclivity to become a nidus for the development of certain entophytes. I have met with it in such a variety of circumstances, that I am almost compelled to regard its presence in the stomach as a result, and not a cause of disease. I have found it alike in most severe cases of dyspepsia, attended by chronic vomiting, and in cases where the general health was in no appreciable degree affected, or where there was a total deficiency of evidence of gastric disturbance. When abundant, it may cause serious symptoms; but it probably does not cause any given disease. Its abundance has generally been proportionate to the severity of the gastric symptoms in cases seen by myself. I believe therefore, that:—

1. Sarcina is not a cause of many cases of obstinate vomiting of a peculiar kind, as is usually supposed, but is only a sign of a disordered condition of the stomach or general system.

2. It is not pathognomonic of pyloric obstruction, gastric dilatation, or other lesion of parts of the digestive apparatus, though it may occur very frequently in these pathological conditions.

3. When it does occur in connexion with these or similar lesions, it is as a concomitant or result.

4. It will probably be found comparatively common in a great variety of diseases, and in very slight deviations from the state of health.

5. The facility with which it is broken up and disintegrated, and the fact of its not having been sufficiently looked for, are the most probable reasons why it is seldom or never found lower in the alimentary canal than the stomach.—*Med. Times and Gazette, Aug.* 5, 1854, *p.* 135.

26.—*On Hæmatemesis.* By CHARLES NEATE, Esq., Uttoxeter.—[In a case of hæmatemesis which resisted the usual remedies of acetate of lead and opium, sulphuric acid, turpentine, &c., Mr. Neate was successful as follows:]

I was now induced to try ergot of rye, and commenced by giving him a drachm and a half of the powder, divided into eight doses; one to be taken every four hours; and to omit the other medicines.

15th.—Better; has vomited no more blood since taking the second dose.

16th and 17th.—Very much improved; no return of blood, except in expectoration, which is rather sanguineous. To continue ergot.

20th.—All the distressing symptoms have entirely disappeared, and there is no tinge of blood whatever in expectoration; indeed, from this time he very rapidly convalesced, and, with the assistance of wine and nutritious diet, gradually recovered strength, and is now able to do light work.

Remarks.—Now, in this case the ergot has shown itself as possessing considerable virtues, and evidently of an entirely specific nature. At any rate there is one recommendation to its use, that it is neither an excitant nor a stimulant.—*Lancet, Aug.* 5, 1854, *p.* 101.

27.—TAPE-WORM UNSUCCESSFULLY TREATED BY MALE FERN AND KOUSSO.

By DR. GEORGE PATERSON, Tiverton.

[The following cases show that we must not place invariable confidence in these medicines. The first was a man aged 38, who, after drinking some cider, parted with a large quantity of tape-worm. Dr. Paterson then says]

The first thing I prescribed for him was a dose of turpentine and castor-oil, which acted powerfully on the bowels, but brought off no portions of the worm. He was then directed to take a draught containing half-a-drachm of the oleo-resin of male fern, then recently recommended in the Edinburgh Monthly Journal, and which I had expressly procured from Messrs. Duncan and Flockhart, of that city, and to follow this up in the course of a few hours by a dose of castor-oil. The following day he brought with him upwards of twenty feet of tape-worm, the greater part of which had come off entire, but dead, after taking the medicine prescribed. The male fern had not produced any action of the bowels by itself; but soon after taking it he felt as if something had given way, and were descending in his bowels, accompanied with great faintness and prostration. The worm did not come off until after the oil had begun to operate. The portion of worm discharged tapered exceedingly towards one of its extremities, the joints becoming extremely small and slender. Other equally minute joints were found broken off from the rest of the worm passed; but, as is generally the case, it was impossible to distinguish the head. All the uneasy symptoms, however, disappeared from that moment. By way of precaution, I repeated the medicine about eight days afterwards, and again a third time eight days after that, but no more portions of worm came away.

Thus far nothing could have been more satisfactory than the effects of the male fern. It confirmed to my mind all that had been reported in its favour, and I set down the case as a decided cure. But after the lapse of two months, on the 25th January, 1853, this man again applied to me on account of a return of his symptoms.

It appeared that he had only remained free of them about four weeks. Then, one morning while going to his work, he was seized with overpowering sickness, vomited blood, and fainted in the street. In the evening the old itching of the nose and anus returned, and on awaking the next morning he discovered about the bed from thirty to forty joints of tape-worm, all dead. He had seen no more since that day; but, as the itching and depression of spirits continued, he believed that there were more to come away. I immediately made him take the male fern in the manner formerly prescribed. About three hours after taking it, followed by the castor-oil, a dozen joints or more, dead and discoloured, were brought off. The uneasy sensations immediately ceased; and, although I took the precaution of repeating the dose as before, no more joints came away. I met him several times after in high spirits, and flattering himself that he was now fairly quit of his troublesome inmate.

On the 18th of April he applied a third time for relief. This time he had remained quite comfortable for nearly three months—in fact, until a week before his application—when, while walking along the street, he felt a return of pain in his right side, and a little itching. Conscious of what these symptoms indicated, he examined his clothes immediately on reaching the work-shop, and found that some joints had passed from him. More came away while he was at work, and from that time more or less had been passing nearly every day, all alive, and all single. He stated that they had been passing in "scores," and that on the previous day there had come from him "one living mass," more than ever he had passed at one time, except after the first dose of the male fern. As several trials of this remedy had failed to eradicate the parasite, I now resolved to try what the kousso would do, and ordered him to take half an ounce in the manner usually recommended. Half an hour after taking it, he felt as if something were dislodged within him, with much pain and exhaustion. Almost immediately upon this the bowels acted, and he brought off upwards of twenty feet of tape-worm, a very considerable length of which was unbroken. The joints of this were generally larger than in the one brought away by the male fern, and presented more of the tapering and gradual diminution observed in it.

As respects a complete cure, however, the kousso has proved equally inefficacious with the male fern. For on the 12th of July, the man again presented himself with a return of his complaints. He had continued quite comfortable until within a fortnight previously, when he was again suddenly seized in the street with faintness and vomiting. He then began to bring off live joints, and had been doing so ever since, with his old symptoms as bad as ever. Another dose of the kousso was prescribed for him, which brought off several more feet of tape-worm, with much pain and griping. His symptoms were once more relieved, and he gave up attending. The man has since left the town; but I am informed that he still continues subject to returns of his complaint.

[The second case was a young woman, aged 24. Both the male fern and kousso were tried in succession, but both failed, while turpentine and castor-oil succeeded in bringing away several yards.]

Remarks.—1. It would appear from the above cases, that the kousso and etherial extract of male fern are, like all other remedies that have hitherto been prepared for tape-worm, liable to, at least, occasional failure. At the same time it is certain, from the effects that followed upon their administration in these instances, that they are remedies of great power, very obnoxious to the parasite, and therefore valuable additions to our means of treatment.

2. I should be disposed to believe from what was observed in these cases that the same remedy is not suited alike to every case; but that in one subject the kousso, in another the male fern, and in another the turpentine, will be found to be the most appropriate remedy. Thus, in the second case, neither the male fern nor the kousso brought off any of the worm, while turpentine invariably did so. In the first, on the contrary, turpentine and castor-oil were repeatedly given before the male fern without having the least effect. Again, we sometimes find the worm, not only detached and dislodged, but carried downwards and expelled by the male fern and kousso, unaided by any purgative; while at other times they require to be followed by the administration of a purgative dose. As to the comparative energy of the kousso and male fern, it would appear from the above cases (in which both were

successfully given to the same person) to be nearly equal, and in the phenomena attendant upon their operation both remedies are very much alike.

3. The first of the cases I have narrated is in many respects a remarkable one. It furnishes a striking portraiture of the symptoms of the disease in a very aggravated form; and the immense number of joints that were discharged at various times, both singly, and in pieces of several feet in length, would seem to indicate either that there must have been more than one of the parasites in the man's body, or else that it is capable of reproduction and of attaining its former growth in an incredibly short space of time after large portions of it have been detached.

The same case affords an instance of the worm finding its way into the stomach, portions of it being stated to have been brought up by vomiting in round balls. This is a more common occurrence with other kinds of intestinal worms. Thus I have met with several instances, both in children and adults, where lumbrici have escaped by the mouth or nose. In one case of an hysterical and hypochondriacal female the circumstance of one of these worms having escaped by the mouth produced an impression that she still had one in the wind-pipe. The sensation, which was partly hysterical and partly perhaps due to an enlargement of the thyroid body, led to continual efforts to bring up the worm and dislodge it with the finger, which necessarily produced increased local irritation, and the failure of which aggravated her despondency and low spirits. She vainly resorted to several medical men, including myself, for relief—neither reasoning nor such remedial measures as the nature of the case suggested having succeeded in overcoming the delusion.

5. In the second case we have an instance of the co-existence of another kind of worm with the tape-worm. This is worthy of notice, principally as showing (what has been pointed out by Dr. Watson and others) that the term "solitaire," which some French writers have applied to this worm as a distinguishing characteristic, is not correct.

6. Besides the above three cases falling under my own notice in the course of a few months, I have heard of another instance under the care of one of the medical gentlemen of this town, in which the kousso brought off several yards of the tape-worm, but equally failed to eradicate the disease. One would almost be disposed to argue from so many instances of a somewhat rare affection observed within a short period among persons, the majority of whom are natives of the place, that the disease is more than usually common in this locality. On inquiry, however, among the oldest practitioners of the town, they inform me this is by no means the case, and that whatever fortuitous circumstances may have concurred to multiply instances within so short a period, their experience of such cases is not larger than that of the generality of medical men in other places.—*Monthly Journal, July*, 1854, *p.* 40.

28.—ON SOME TÆNIFUGE MEDICINES.

By M. Strohl, Strasbourg.

The fruits called *tatzè* and *zareh* are the produce of a shrub of the family of the myrsinaceæ, the "*myrsina Africana.*"

This plant is found in Abyssinia, in moist rocky places at the Cape of Good Hope, in the Azores, in Algeria, and other parts of Africa. According to M. Schimper, it is met with in Abyssinia at an elevation of 9,000 feet in dry shady places.

The author details six cases in which this drug has been used:—

Case.—Professor Tourdes.—H., a boot-maker, of lymphatic temperament, and delicate constitution, came to Strasbourg in 1842. At times he suffered from general indisposition, want of appetite and indigestion; but never suspected that he was the subject of tape-worm until August, 1853, when he first discovered some large fragments in the stools. Frightened at this discovery, he went to consult M. Tourdes the same day, and as there existed no contra-indication he was ordered a dose of tatzé the following day in soup of lentils. The patient did not take it without great repugnance. No colic nor motion followed for three hours; a dose of castor oil was then administered and an hour after there followed a very abundant evacuation, along with which was expelled a bothriocephalus, measuring eight or ten yards. The tænia was divided into two fragments, one of which terminated in very fine, al-

most filiform joints; some joints of small size were disseminated through the fæcal matter; but the head was not discovered—very accurate search was not made for it. For some time after the patient's health was much ameliorated: the moral impression had a salutary effect; but now, his fears returning, he affirmed that he could not take *tatzé*. So invincible a repugnance does he bear against this remedy that, although aware of its efficacy, he cannot bear the idea of taking it.

From these cases it would appear that tatzé is taken with much greater repugnance than saoria, and that the taste of it is more acrid and persistent. In three, vomiting followed soon after the administration of the drug, nevertheless the portion which remained was sufficient to cause the expulsion of the tape-worm. Thence, we may infer that the dose was, possibly, unnecessarily large, and that a smaller quantity would be more easily swallowed, and cause less disagreeable symptoms, yet be equally efficacious. The patient never complained in the least of colic. The purgative effect was not constant, perhaps on account of the quantity of tannin which tatzé contains. One of the patients only suffered from general indisposition and headache, not, however, of much moment. Tatzé seems to be tænicide.

As to the urine; where this was watched, it was found to be of an inky colour, probably analogous to the coloration that has been already noticed after the use of saoria.

Tatzé is a powerful tænifuge, which has succeeded in all the six cases in which it was administered. This number is too small to draw any conclusion as regards the constancy of its action, but it is quite sufficient to assure us that it at least deserves to be placed among the resources of materia medica. Tatzé is less gentle in its action than saoria; yet it would be worth trying whether by a diminution of the dose or the combination with it of some other pharmaceutic preparation, as opium, for example, it would not be possible to diminish the troublesome symptoms attending its use. As to its mode of administration, it should be given in doses of about half an ounce, reduced to powder, and blended in a ptisan or aromatic infusion; if after three hours it has not caused a motion from the bowels, or if in stools which have followed no traces of the worm are to be discovered, a dose of castor oil should be administered.

In conclusion, from these trials, insufficient it is true, it results that saoria and tatzé deserve our attention, and it is probable that saoria will hold the first place among our tænifuge remedies. Mayor, who has divided the bothriocephalus into two kinds, according to its joints, says the etherial extract of male fern expels that with the long joints, whilst that with the short joints requires the powder of the bark or pomegranate root.—*Gaz. Medicale.—Dub. Hosp. Gaz., August* 15, 1854, *p.* 217.

29.—TREATMENT OF DIARRHŒA BY CHLORIC ETHER.

By George B. Mead, Esq.

[We often find that many cases of diarrhœa resist all ordinary treatment, opium amongst the rest. In 1846, '7, and '8, an epidemic prevailed at Bradford, Yorkshire, which was attended by a diarrhœa of an intractable nature.]

This diarrhœa was combined in many cases with vomiting and spasmodic pain of a very distressing character. We had very little difficulty in treating those cases in which pain was absent, but we found a certain proportion in which the diarrhœa obstinately continued despite the use of a multitude of remedies; and the pains, though temporarily relieved by the use of opium, returned directly the narcotism passed off; the opium suspended, but did not remove, the spasms. At length we adopted the following formula:—

℞. Ætheris chlorici ʒij.; speciei pro conf. arom. ʒss.; misturæ cretæ compositæ ad ℥vj. M. Fiat mistura.

The fourth part was directed to be taken directly by an adult, and repeated every half hour, or at still longer intervals, according to the severity of the attack, and its effect upon the patient. Occasionally, opium, either in the solid form or the tincture, added to the mixture, was given; but this was seldom necessary, and I think every case would have recovered without its use. The effect of the ether in every case was marvellous. The spasms and pains were relieved, as if by a charm; the diarrhœa ceased; warmth returned to the extremities; the pulse, before perhaps flagging, increased in force and volume; one bottle never failed to relieve, or two

to cure even the worst cases in which all other plans had failed. The relapses were infrequent, and were generally checked at once by a single dose. After the introduction of the chloric ether, we had no further trouble with diarrhœa. The medical man in whose practice this occurred is since dead; otherwise I am sure he would have felt great pleasure in confirming this statement. Often has he expressed to me his high opinion of the efficacy of the ether in cases of diarrhœa combined with spasmodic pain. After this, at the time the cholera last visited this country, I was residing with Dr. Morris, of Spalding. It reached Boston. Great alarm existed in Spalding, and the public were fully aware of the importance of checking the premonitory diarrhœa which began to prevail, and was exceedingly troublesome in many cases to check. Recollecting the ether, I ventured to suggest its use. My friend acquiesced. It was tried, and in not one case failed. As at Bradford, hundreds of cases in which alarming cramps existed were cured like magic; none ran on into Asiatic cholera, though many appeared to be on the verge of it, or, at any rate, were equal to those of English cholera of the severest character. So prevalent was the diarrhœa, that the surgery was thronged from morning to night by applicants for medicine; the policemen in the streets, sailors in the vessels on the river, travellers upon the rail, were equally affected: the cause was evidently ubiquitous, whatever it might be.

At last, one morning early, I was summoned in haste to a patient, an old woman, residing in one of the most unhealthy localities in Spalding. I found her presenting unmistakeable signs of labouring under Asiatic cholera in an advanced stage; she was blue, almost pulseless, and the cramps were of a most violent character. I had seen English cholera in its worst and most fatal forms, but never aught like this. I have a vivid recollection of the scene, and remember well I stood as it were aghast at the sight of her agonies. Having directed her attendants how to act, I instantly returned home, and sent a mixture, increasing the dose of ether, but with very little hope of success. I ordered it to be given at very short intervals, and, in the course of an hour or two, saw her again, in company with my friend. We were delighted and surprised to find her greatly improved. We continued the remedies, and by the evening she was out of danger. Symptoms of relapse appeared several times during the next day or two, but were at once checked by the use of the remedy. The patient, though advanced in years and greatly debilitated, having led a somewhat irregular life, completely recovered, and died about a year afterwards, of a totally different complaint. The remedy obtained quite a notoriety in that locality; to such an extent was it used, that we were told by an extensive wholesale druggist he had sold more to us than to all the rest of his connexion put together.

A gentleman travelling by the Great Northern Railway was attacked by severe diarrhœa. He got out at Spalding, and consulted Dr. Morris, who prescribed this remedy; it at once relieved him. In conversation, he mentioned to him how valuable he had found it in the treatment of diarrhœa. On his return home, the gentleman told his usual medical man of the circumstance; and he, in consequence, was induced to try it, with great success.

In 1850 and 1851, I was again residing in Yorkshire, and there saw much diarrhœa. I used the ether extensively, and never found any case in which it was properly administered where it failed; indeed, I recollect some equally striking cases with those I have enumerated, in which it might indeed be said to have acted like a charm. I could refer to many of my medical friends who have used it upon my recommendation, and whose experience, after numerous trials, has strongly corroborated my own. Though I have used it in at least fifteen hundred cases myself, and indeed I think not improbably three thousand cases, I have never yet found it fail.—*Association Med. Journal, Sept.* 8, 1854, *p.* 819.

30.—*Sulphate of Bebeerine in Diarrhœa.*—Mr. Mathews, of Manchester, strongly recommends in the 'Lancet' the use of sulphate of bebeerine as a remedy for severe diarrhœa, in fact, he states that he has found it 'a very near approach to a specific, and that, with a very few doses, as a rule; indeed, in many instances, its effects are magical, for in the course of half an hour I have seen a patient restored from intolerable anguish to perfect ease." He generally first gives a pill, consisting of two grains of calomel and half a grain of opium, and if there is great pain, or the

vomiting is urgent, orders a sinapism to the scrobiculus cordis, and then follows up with the following mixture:—Sulphate of bebeerine twelve grains, sulphuric acid and rectified ether, of each, twelve minims, cinnamon water six ounces; mix. One ounce to be taken every four hours.—*Dub. Hosp. Gazette, Sept.* 15, 1854, *p.* 254.

31.—ON SOME POINTS IN THE PATHOLOGY OF THE LIVER.

By Dr. W. T. Gairdner.

[In an interesting paper on this subject read before the Physiological Society of Edinburgh, Dr. Gairdner gives us some interesting results of his inquiries on the aggregate constitution of the liver in certain diseases.]

The *normal liver* is composed of very numerous hepatic glandular cells, disposed amid a network of capillary vessels and biliary ducts, and fed by a large amount of blood. It may be regarded as consisting, in a chemical point of view, of water to the amount of about three-fourths; of an insignificant amount of fatty matter; and of various albuminous solids, amounting to nearly one-fourth of its weight.

The *fatty liver* consists of precisely the same elements in a similar structural arrangement. The aggregate of solids and water may be slightly increased or slightly diminished; their percentage, however, is invariably considerably reduced, owing to the enormous development of oil in the glandular cells, by which the volume of the organ, as well as its weight, may be almost indefinitely increased.

The *waxy liver* is a true degeneration; for this condition is not, like the fatty liver, due merely to a morbid increase of one of the non-essential elements of the organ, but to a metamorphosis of its most essential, *i.e.*, its glandular structure, into a much more dense albuminous material than in the normal condition; a change which results in total destruction of the function, as well as the structural characteristics of the gland-cells, and in a great increase of the solids of the organ at the expense of the fluids, the blood ceasing to circulate freely, and the water being diminished from three-fourths to three-fifths, or possibly in some cases to one-half of the weight; the solids being correspondingly increased, the fat either increased or unchanged.

Cirrhosis of the liver is either an independent degeneration, or it may spring from the waxy or the fatty conditions. In the former case, it assumes at once the atrophic form, and all the elements may simultaneously disappear; the atrophy commencing, however, in the glandular epithelium, and thus giving rise to an apparent relative hypertrophy of the fibrous tissue.

Finally, the *waxy and the fatty liver* are not unfrequently met with as a combined lesion. In such cases the liver is usually very much enlarged, dense and opaque, yellow on section; intermediate in specific gravity between the two typical forms of disease, and therefore approaching the normal density; and it presents under the microscope a degenerated epithelium, and very numerous but unusually small fatty granules and globules. I have never made the analysis of such a case, but it is impossible to doubt what would be the result. In the purest forms of the waxy disease, again, I believe the amount of fatty matter would be found very small indeed, at least if one can judge from the results of microscopic examination.—*Monthly Journal, May*, 1854, *p.* 398.

32.—ON THE USE OF TURPENTINE IN JAUNDICE, &c.

Turpentine has long been used in purpura hæmorrhagica, and cases where the blood seems to become more diffluent than natural. Its efficacy in yellow fever has latterly been well shown by Dr. Laird, who has compiled his report from his own experience, and from the records of the naval hospital at Bermuda, where this disease is so frequently epidemic. He considers the pathology of yellow fever as primarily connected with a morbid alteration of the blood, and that the turpentine, besides its general properties as a sedative, styptic, and antiseptic, possesses also peculiar power of action on the skin and kidneys. I can bear ample testimony to its success in another form of jaundice, which we meet with much more frequently in fever, and

arising from a totally distinct cause; I allude to jaundice from excessive secretion of bile. In the year 1845, Dr. Corrigan treated, with turpentine, a case of this kind ("considerable discharges of blood, mixed with bile, from the intestines,") which occurred in the Hardwicke Hospital, and which recovered, even after the brain began to be affected. I have had in my wards several cases of this kind within the last three months, all of which recovered under the use of turpentine. The usual dose ordered is a drachm every two or three hours. It is frequently given with nitrous spirit of æther, to promote perspiration and avoid strangury. The following case, out of several, will answer as an example of this form of jaundice.

Case reported by Mr. Abbott. John Colgan was admitted into the Hardwicke Hospital, January the 11th, complaining of rigors, headache, and pain in his back. He was four days ill; he was universally jaundiced. Tongue broad, white, and dry; great thirst; abdomen swollen and tympanitic; great nausea; bowels free; no tenderness, except in left hypochondriac region, where the spleen can be felt much enlarged. Pulse 100, weak.

Ol. terebinth. ʒi. 2dis horis ex aquæ cyatho.

12th. Nausea continues, with vomiting of dark bilious matter; alvine discharges copious, fluid, well tinged with blue; urine also loaded with bile; skin burning hot. Pulse 84, weak.

Cont. ol. terebinth.

14th. Pulse 90; tongue moist; perspiring slightly; no vomiting; nausea much diminished; splenic enlargement less; jaundice diminishing.

Recovered slowly; convalescent Jan. 30th; discharged cured Feb. 14th.

In this form it would appear that there was an excess, rather than a suppression, of the secretion of bile; as in the former all the tissues are tinged of a yellow colour, seldom, however, so deeply or so bright; the conjunctiva is less injected, and the face is not so congested, nor the appearance of prostration so great. If there be vomiting, there is usually copious discharge of bile, and the alvine evacuations are also well coloured therewith. There is frequently bilious diarrhœa. These cases are with us generally epidemic, while the former class are sporadic. About two years ago there were a great many of them, and several of them proved fatal. The post mortem appearances which we found were constant, and differed remarkably from those found in such cases as Mahon's. The surface of the body was yellow, and had frequently spots of purpura hæmorrhagica diffused over it. The spleen was invariably greatly enlarged and very soft, in many cases almost diffluent, and could with difficulty be removed entire from the body. The liver was generally enlarged; it was in some cases friable, and more yellow than natural; the gall-bladder was always full of dark bile, which flowed freely into the intestine; the mucous membrane of the stomach was generally congested, as were also the depending portions of the lungs.

This form of jaundice I have occasionally treated with sulphate of quinine, but without any good effect. Occasionally the stomach is so irritable that the oil of turpentine is rejected; this, however, seldom continues, and can in general be obviated by the application of a blister to the epigastrium. When the stomach continues to reject the officinal preparation, I have seen the æthereal oil tolerated, and in this disease prove efficacious.

There is a third form of jaundice occasionally met with in fever, and which is of great practical importance to be acquainted with. A patient in continued fever, without any appearance of jaundice or hepatic symptoms on the previous day, suddenly becomes quite yellow about the fourteenth or fifteenth day of fever, and all the tissues become impregnated with bile; besides giving the usual tint to the skin and conjunctiva, the bile passes off by the bowels most copiously, and the urine is loaded with it. In one case I observed that the posterior part of the dorsum of the tongue, the soft palate, and fauces, had assumed the yellow hue, and that not merely the saliva but the expectoration was deeply tinged with it.

These cases are purely critical, and differ from other forms of crisis only in the organ in which the critical action takes place. Instead of occurring in any of the usual modes, such as perspiration or diarrhœa, &c., it occurs by a profuse secretion of bile, which endeavours to pass rapidly away through all the outlets of the body. Such cases require but little treatment; the rapidity with which all the icteric symptoms pass off is most surprising. There are two facts, however, to be noticed re-

garding it; one is, that the premonitory symptoms of this form of crisis are more severe than I have ever seen in any other; they were well marked in the case of Ellen Smith, æt. 26, admitted into the Hardwicke Hospital June 6th, 1853. She was, in the course of fever, suddenly seized with bright flushing of the face, delirium, and increased rapidity of pulse. This was followed by universal jaundice and excessive discharges of bile per anum and in the urine. From this she as rapidly recovered, but again afforded an example of the other important feature in this mode of crisis, which is, the excessive debility which follows it. Although she was well supported and properly nursed, she suffered a premature confinement, and was very long in gaining her natural strength.

I have also very frequently seen jaundice arise in the course of fever, as a symptom of pneumonia.—*Dublin Hospital Gazette, July* 1, 1854, *p.* 166.

DISEASES OF THE URINARY ORGANS.

32.—ON DIABETES AND ITS RELATION TO BRAIN AFFECTIONS.

By Dr. Richard H. Goolden.

[The following views of Dr. Goolden on the nature of diabetes will be read with interest. We have been accustomed to look upon the stomach or kidneys as the organs most implicated; but Dr. Goolden considers that the brain has more to do with the disease than has generally been suspected.]

Diabetes was regarded by Dr. Prout as a form of dyspepsia—a "paralysed function of the assimilating organs"—and it was a great advance in its pathology to relieve the kidneys from the blame of all the wrong that they are constantly doing their best to remedy; and I am not aware that any considerable advance has been made in our knowledge since that time, if we may except some negative knowledge as to the effect of remedies, and Heule's argument, in his "Handbuch der Pathologie," that the sugar is not necessarily the product of food; but that subjects fed entirely on nitrogenous food produce it in considerable quantities, and that it seems to be formed rather at the expense of the muscular tissue of the body than of the fat. My own experiments have clearly demonstrated the fact that a hospital milk diet, with a good allowance of extra bread, butter, barley-water, and soda-water, does not increase the amount of sugar excreted; and that an exclusively animal diet does not diminish it with any such certainty as to justify the inference that the sugar is formed from the farina or sugar of the food.

The experiments of Bernhard prove the formation of sugar in the liver and its destruction in the lung, determined by some nervous influence, the centre of which is situated in the floor of the fourth ventricle at the origin of the eighth pair, and these experiments have been repeated by most of our own physiologists with success. The mode of performing them, and their difficulties and cautions, are well described by Dr. Parry, in the last publication of the 'Guy's Hospital Reports.'

The inferences drawn from these experiments seem to be, not only that sugar is abundantly formed and excreted by the kidney, when the pyramidal body is delicately punctured, but that the same result of irritation may be produced by the influence of galvanic currents on the pneumogastric nerve, and that such stimulus must act through the medium of its cerebral organ; the pneumogastric nerve not being that which presides over saccharine secretion, but rather being a nerve of sensation, and conveying the sensation which regulates all the respiratory actions, whether of normal or of hurried breathing, yawning, sneezing, coughing, crying, laughing, &c., and is possibly the nerve that conveys the physical impressions of mental emotion, especially in connexion with visceral disorder.

It is not my object at present to enter into this subject as a physiologist, but to contribute some clinical facts which may lead to a more rational course of treatment of the disease in question than hunting after specifics; and when we shall have ascertained something more definite than the present state of medicine possesses, even if we do not pretend to cure the disease, we shall have grounds for separating in our treatment the cases which may be organic from those which are functional. We may even hope to see a large class of functional cases detected and cured, and

if we cannot restore organic changes, we may at least do something towards relieving the distressing consequences, or at all events we shall not add to the distress by needless privations and useless remedies.

Diabetes is not so common a disease, in its aggravated form, that we get a large field of experience even in our great hospitals; and it is only by seeking for such cases amongst our medical friends that we are able to obtain more than two or three cases in a year; indeed I have known a year pass over without meeting with a single case, even amongst the large number of applicants as out-patients, from whom we might have selected them, and I have generally observed that two or three cases have appeared about the same time. But saccharine urine is a much more common affection, especially in children, and is often concomitant with other matter in excess, as the lithic acid, lithate of ammonia, and especially phosphate of lime and albumen; and on the examination of the urine of every patient admitted into the medical wards of St. Thomas's, we have found sugar in a considerable proportion, of which I intend to give a statistic report; by that report it will be observed that in cerebral or nervous diseases the absence of sugar is the exception and not the rule.

For some months I have been engaged in considering this subject. I have examined every case of epilepsy, paralysis, and chorea that has applied at the hospital during my weeks of admission; and not only have I found sugar in a large number of these cases, but I have found the sugar disappear as the symptoms are relieved by treatment. Ordinarily there is no diuresis, and the urine is often turbid, and frequently ammoniacal, and the specific gravity from 1·020 to 1·024. As the ammoniacal salts interfere with the operation of Trommer's test, I boil the urine for a few minutes, with the addition of a small quantity of pure soda. This sets free the ammonia, and precipitates the phosphate of lime, and then I proceed as usual with the addition of three or four drops of a solution of sulphate of copper, and boil it again with a slight addition of alkali. A yellow precipitate then indicates the presence of sugar or some deoxidizing substance.

Several years ago, a case which ended fatally occurred under my treatment in St. Thomas's Hospital in which the urine suddenly lost all traces of sugar. On the one day he passed twenty-four pints, specific gravity 1·048, clear, straw-coloured; and on the following day the quantity decreased to fourteen ounces, with pink sediment, specific gravity 1·012, and the patient was bathed with perspiration. He was seized with pneumonia, and died within five weeks of phthisis, or at all events with vomica in the apex of each lung. There was no trace of pulmonary disease prior to the change in the condition of urine. This case impressed me with the idea that Prout's theory was not altogether correct, and I thought it improbable that the symptom which so suddenly gave way on the development of pulmonary disease must depend upon some cause other than partial paralysis of the digestive functions, or chronic dyspepsia, and that in that case it could not have depended upon any disorganization of structure.

From that time I omitted no opportunity of obtaining diabetic patients for observation, but I did not meet with any other case in which the diuresis suddenly disappeared; and although I thought some of my patients were benefited by treatment, the amount of benefit never satisfied me that the disease itself was at all removed. Fluctuation in the amount and specific gravity of urine appeared quite independent of any treatment adopted; but I assured myself of the fact, that the amount of sugar excreted by the kidneys was not materially diminished by a pure animal diet and opium, nor increased by a milk diet, with a plentiful supply of bread, rice, and barley-water, and even cane sugar. In drawing our deduction from this fact, it is necessary to bear in mind that diurnal fluctuations are constantly taking place, and that, under the same treatment, constantly persevered in, a variation of considerable amount occurs from one day to another, so that we must take the average of a week at least to determine whether treatment is doing anything towards the diminution of the symptom, unless, indeed, the diminution be so considerable as to leave no doubt that it is the effect of treatment, or of some change in the exciting cause.

The next case that occurred to me, which seemed to throw light upon the subject, was the following:—

James C——, aged forty-six, railway-guard on the Great Western Railway, a man in previous good health, met with an accident, in consequence of the slippery state of the rails, which prevented the break from effectually stopping the train, and

a slight collision took place. He was thrown forward, and the handle of the break, which is a lever, turning a screw against the force of a strong spring, slipped out of his hand, and the spring forcing the screw back turned the handle with increasing velocity, and it struck him on the side of the head. He was stunned, but after about an hour was sufficiently recovered to be led between two men to a house a mile distant, where he was put to bed. He suffered from concussion of the brain, having no recollection of the accident, or how he got to the house. The accident occurred at Exeter. I did not see him till Dec. 21. He had recovered from all the urgent signs of concussion, and had even taken the charge of a short train to Windsor and back, but was so much affected by the shaking, that the superintendent sent him to me to examine and report. He complained of giddiness and confusion, but no pain, except tenderness where his face had been much bruised; pulse 110; skin very dry and harsh; tongue very red and sore, with no mucus, but not dry; he complained of excessive thirst, and said, that his appetite was remarkably good; bowels costive; and on being asked about his urine, he told me that he passed a most extraordinary quantity, and had done so ever since his accident. In fact he passed so large a quantity, that in the cottage where he was residing they could not get vessels enough to contain it till morning, as he filled all the urinals and the washhand-basin. Having obtained a specimen, I found it loaded with sugar; specific gravity, 1·052.

The treatment adopted was to keep him quiet in bed, applying evaporating lotions to the head, the hair cut close, and purgatives. Chloride of mercury, eight grains, and compound extract of colocynth, five grains, immediately; sulphate of magnesia, one drachm; infusion of roses, an ounce and a half, three times a day. His diet was to be milk and farinaceous, with as much water or barley-water as he desired.

At the end of the week (28th December) I saw him again. He was purged by the calomel, but not since. He felt better the day following, but still I did not find any improvement, except the urine was rather less in quantity, but it contained a trace of albumen, and he had pain in his loins, and numbness in his legs and thighs (a common symptom in diabetes, but might be attributable to the brain). The pulse was 98. I prescribed calomel and compound extract of colocynth, of each five grains, every night, and compound senna draught, every alternate morning; to continue the sulphate of magnesia draught; and blister the head.

On the 2nd of January, he called upon me again, and stated that the medicine purged him considerably, and that something in his head gave a click, when immediately a kind of cloud passed from his mind, and he seemed to wake from a dream. The urine became scanty and high-coloured, specific gravity 1·012, no sugar. He was still purged by the medicine, and felt himself otherwise quite well. The tongue had lost its raw-beefy appearance; pulse 80, and small. Medicine was discontinued, and in a few days he returned to duty. He is now in perfect health.

About the time referred to, I had a case in King's ward, No. 12, Jacob M——. I was induced to use purgatives, and apply the blister to the head, in consequence of the result of the guard's case, and the event was most satisfactory. The man is now in perfect health, passing an ordinary quantity of healthy urine, without a trace of sugar or albumen; moist skin, and cheerful-looking face.

The next case, William A——, aged twenty, No. 14, King's ward, though a young man, was not so satisfactory, for he left the hospital to go into Devonshire, still diabetic; but at the time he left, the urine was reduced from about twenty to eleven pints, and the specific gravity reduced from 1·050 to 1·030. The interest of this case consists in its being traceable to an injury to the head, and in the partial relief produced by the blisters and purging; but it must be observed that it was of eighteen months' standing; and the attention must be directed to the improved state of the tongue, diminution of thirst and canine appetite, the restoration of perspiration to the skin, and the improvement in aspect, strength, and cheerfulness.

Another case to which I must refer is now in King's ward, No. 12, who came up from Cheltenham, and had been under treatment for two years before admission. He seems to have derived more benefit from strychnia than any other remedy he had had recourse to; but when admitted, after a blister to the head and a calomel purge, the urine was reduced from twelve quarts to six pints, and the specific gravity from 1·048 to 1·032; but since then the amount has increased to an average of

eleven pints, fluctuating two or three more or less, and the specific gravity varies, averaging 1·038.

A case was related to me by Mr. Gibbs, a surgeon at Birmingham, of a manufacturer who applied to him for advice in consequence of his suffering from diabetes in an aggravated form, with all the most prominent characteristics and symptoms of the complaint. He was kept upon a diet of flesh and porter, and opium was administered for six weeks, without any material alleviation, when he was suddenly seized with an attack of paralysis. A calomel purge, and a blister to the nape of the neck were administered, and he was treated generally for cerebral congestion. The diabetic symptoms suddenly disappeared, he recovered partially from his paralysis, and no sugar has since been traceable in the urine.

These cases individually would prove nothing more than an accidental alleviation of morbid action, which might or might not be connected with the treatment; for the same sudden subsidence of saccharine diuresis might occur at any time, (for anything we know to the contrary, as far as the pathology of diabetes is concerned,) which took place in the first case that I mentioned, where there was no treatment of cerebral disease; and in two other cases the relief was neither sudden nor complete: but taking them in the aggregate, and reasoning upon the probabilities, that the brain was implicated in all these cases as the organ at fault, can scarcely admit of a doubt; a conclusion which is confirmed by Bernhard's experiments, whatever may be the explanation of the physiologist as to the particular part played by the liver or the lung in the formation or destruction of the saccharine principle. I should observe that in the case of the guard, and Mr. Blake's case, the relief was sudden and complete, and that in both these cases the disease had not been of long duration. In cases No. 4 and 5, the disease had been in existence a year and a half and two years, and the result is proportionately tedious and incomplete, but still sufficient to indicate the effect of treatment. One point is remarkable, that in every case the action of the skin was restored, so that even the old chronic cases were seen perspiring copiously, and the appetite moderated, though not impaired.—*Lancet, June* 24, 1854, *p.* 656.

[Dr. Goolden mentions that saccharine urine without diuresis is very common, and that few cases of chorea and epilepsy in young people occur without showing some sugar in the urine; and even in children who are teething the same occurs.]

Severe cases of neuralgia, especially tic douloureux and sciatica, are often, but not universally attended with saccharine urine, which disappears when the nervous affection is relieved. The exceptions appear to me to be those cases where the neuralgia is of gouty or rheumatic origin; but of this I am not sure; yet I hope, on still further observation, to form a more certain opinion. I have seen it disappear under the use of quinine in large doses, and opium; and my friend, Dr. Dundas Thompson, to whom I am indebted for much valuable assistance in the laboratory, and for many suggestions, tells me that he has only lately been consulted by a young lady suffering severely from facial neuralgia. The urine, as he suspected, betrayed a considerable amount of sugar; and that both the pain and the saccharine condition of the urine suddenly disappeared after the application of a blister, not to the head, but to the face. This case is important for illustration, because it is given upon the authority of so eminent a practical chemist, and whose analysis would satisfy any one that he could not be mistaken as to the presence of sugar.

A girl, aged thirteen years, No. 23, in Anne's ward, was admitted under my treatment. She was suffering from very severe and constant headache, and likewise from pains in various parts of the body, but without any swelling or tenderness on pressure. There was great disinclination to make any exertion; the pulse 98—120; face flushed; the skin not perspiring; she passed about twenty-four ounces of urine, clear, and of the colour of Madeira wine; it was alkaline, and specific gravity 1·024. The report was that the urine contained no sugar; but observing it to be very ammoniacal immediately after it was voided, I boiled it in the laboratory with a small quantity of pure soda, and then I found an abundant precipitate of orange-yellow on the application of the copper test, and Dr. Thompson confirmed my analysis. A blister to the back part of the head completely removed the headache, the urine ceased to be ammoniacal, and no more sugar was to be detected in it. This patient is the subject of incipient phthisis, and there is much probability that the brain was suffering from strumous congestion, which the blister relieved.

Now the inference I would draw from these few observations is, that the seat of the disease is some portion of the brain, and that the disease itself may be primary, as when produced by injury; or secondary, as when it originates in some strumous or other cachectic condition of the system;—that the sugar is the product of the secondary, not of the primary assimilative functions; assuming Dr. Prout's definition —viz., "The primary assimilating processes comprise all the processes of digestion, and all the intermediate processes up to sanguinification; while the secondary assimilating processes comprise the processes by which the different textures of which the living body consists are first formed from the blood, and afterwards redissolved and removed from the system." The appetite and thirst are to be considered as natural indications, to be gratified with prudence, and more with reference to the wants of the system than any fear of increasing either the sugar or the amount of fluid.

Even if the amount of fluid be diminished, and the quantity of sugar in the excretions lessened, through inanition, the advantage is not equivalent (except in particular inflammatory cases) to the effect produced by consequent loss of nutrition to the system; and no possible advantage is gained by a diminution of morbid secretion under such circumstances, since such diminution is no index of improvement. All remedies should have reference to the morbid action taking place in the brain, and not in the stomach, liver, or any other digestive organ, although we may require to act upon the brain through their agency.

The idea of diabetes being a cerebral disease is so recent that we have no record of autopsies in which cerebral lesion has been observed or looked for; and I have never had an opportunity of examining a brain with that object of search. This subject in future will form an important object for the pathological anatomist.—*Lancet, July* 15, 1854, *p.* 30.

[The connexion of diabetes with an affection of the nervous system is not new, as will be seen from the following:]

"Bernhard has shown that the secretion of sugar by the liver is under the direct control of the nervous system. If the pneumogastric nerves be cut in the region of the neck, the production of sugar very soon ceases. If the extremity of the nerves connected with the lungs and liver be artificially irritated, no change occurs; but if the upper extremity be excited, the consequence is a reflex action through the spinal cord, which, following the spinal nerve, reacts upon the liver, and again causes the production of sugar. This influence may be compared with the irritation in the lungs transmitted by the pneumogastric nerves—during the inspiration of chloroform, gas, or ether, for instance—when the amount of sugar produced by the liver is sensibly increased.

"If, instead of irritating the lungs, we puncture the fourth ventricle of the brain or the olivary bodies, an increased action of the liver takes place, producing an excess of sugar, which is excreted by the kidneys. This excessive production of sugar, which can be obtained by an artificial irritation of the lungs, or of the superior extremity of the pneumogastric nerves when cut, or, finally, by the puncturing of the fourth ventricle of the brain and olivary bodies, may also be observed to occur in the human body under certain morbid conditions."

Accordingly, Messrs. Robin and Verdeil consider diabetes to be owing to various affections of the lungs, or perhaps also to some disease of the medulla oblongata.

Some experiments very recently performed by Dr. Harley, at Paris, and communicated to the Société de Biologie, appear to show that the nervous reflex action, upon which depends the secretion of sugar by the liver, is generated in the liver itself, by the stimulating power of the blood of the portal vein upon the hepatic branches of the pneumogastric nerves. Dr. Harley increased the exciting influence of the blood of the vena porta by injecting into that vein alcohol, sulphuric ether, chloroform, or ammonia, and observed, that two or three hours after the operation, the dogs experimented upon voided sugar in their urine, and remained diabetic from two or three hours to two or three days. The presence of sugar in the urine of those animals was detected by means of the double tartrate of potash and copper, and also by fermentation, so that no doubt exists as to Dr. Harley's results.—*Brit. and For. Med. Chir. Rev.—Dub. Hosp. Gaz., July* 1, 1854, *p.* 173.

34.—ON THE EMPLOYMENT OF TORULA CEREVISIÆ IN DIABETES MELLITUS.

By Dr. W. Bird Herapath, F.R.S.E.

It has been abundantly shewn, that the morbid product in diabetes is the presence of a saccharine substance in the urine, of a low character, perfectly identical with the sugar of grapes and other analogous fruits. The source of this sugar or glucose has been traced to the mal-assimilation of the starchy or amylaceous element of diet, which substances, as soon as they have entered the stomach of a patient suffering from this disease, are at once converted into glucose. This is as rapidly taken into the current of the circulation by the venous system, and more slowly by the lacteals, and thoracic duct, and is there eliminated by the kidneys, together with an abundant excretion of water. The name of glucosuria has therefore been most appropriately given to the disease.

In healthy digestion it appears probable that starch, although previously converted by the saliva into glucose, is further metamorphosed in the stomach into lactic and acetic acids. The former predominates; hence the amylum of the food is the probable source of most of the lactic acid of the gastric juice; which has been shown by Lehmann to owe its digestive powers in great measure to the presence of hydrochloric and lactic acids.

In diabetes, therefore, it is evident that the conversion of starch stops in the metamorphic series at glucose, a substance highly diuretic and abnormal to healthy blood, and which, appearing to be incapable of undergoing further metamorphosis in the circulating current, and being useless in the production of adipose tissue, and also incapable of subserving the process of respiration, is eliminated from the system as an effete substance, constituting the most evident sign of the diseased condition of the patient.

Although the quantity of sugar is materially lessened in diabetic cases by the abstraction of all the starchy elements of diet, yet it does not wholly disappear, and some recent experiments point to another source of this morbific matter; but at present we are not in a condition to say more than this, that starch is "*the principal* source of sugar in glucosuria, if not the only one."

We are hence led to the following conclusions: either, 1. That the existence of sugar is an abnormal product in the digestive process; or, 2. That, if produced, it should be merely a transition state to some other proximate element of less stability, and nearer to the ultimate element of its assimilation; either lactic acid on the one hand, or adipose tissue or carbonic acid and water on the other.

We have merely to recollect with what facility certain chemical processes change glucose, and induce it to continue its metamorphic condition. Of all these processes, that by means of certain vegetable agencies appears to be the most suited for the purpose we have in view—namely, the prevention *of the arrest of the conversion of starch at the stage of sugar*, when undergoing the process of digestion.

It has lately been shewn, too, that certain vegetable organisms have the power, whilst endowed with vitality, of resisting the digestive powers of the stomach. The sarcina ventriculi may be mentioned as a case in point. It therefore easily follows, that the torula cerevisiæ may be presumed to have an efficient action within the stomach, similar to its power of inducing fermentation in saccharine fluids out of the body, if it were capable of resisting the assimilative powers of this viscus for sufficient length of time.

These views led me to employ the torula in diabetes; and I am happy to say, that in the only case in which I have yet had an opportunity of trying this remedy, it answered fully the expectations which I had previously formed of it. Under ordinary circumstances, glucose, at a temperature of 60° to 70° Fah., would be converted into alcohol and carbonic acid by the fermentative agency of torula cerevisiæ or yeast, as is very well known to most persons; but if the action were to take place in the dark, in the presence of albuminous substances or other protein compounds, and at a temperature of 98° Fah., the products would be lactic and acetic acids, with possibly alcohol and carbonic acid.

It is clear, therefore, that the former products would assist in the conversion of the protein compounds, as in the normal state of digestion, and would pass out of

the system in combination with some alkaline or earthy base, and be eliminated by the kidneys, skin, or other emunctories; whilst the alcohol and carbonic acids would act as agreeable vital stimuli, whilst existing in the system, and could eventually escape by the pulmonary mucous membrane, after having perhaps served to assist the respiratory process, being thus converted into carbonic acid and water.

It is known that the yeast-plant, during fermentation, undergoes a progressive growth and decay; and that after long action it becomes spent and exhausted, when it is found that the cells of which it consists have become ruptured, and the vitality destroyed. Presuming, therefore, that the yeast plant became no longer capable of continuing the process of lactic or alcoholic fermentation in the stomach, it would die; and then would itself become a nutritive nitrogenous substance, like any other article of vegetable diet.

In January, 1853, I determined to put these views in practice; and having a patient then under my care, I commenced the administration of the remedy. Before the treatment, the patient had been voiding urine of specific gravity 1044, and containing 850 grains of sugar in the imperial pint. Within two days of the employment of this substance, the specific gravity sank to 1020, and the sugar to 300 grains per pint. By steadily pursuing the same course during six weeks, the sugar eventually disappeared altogether; the urine assumed its healthy character; and the patient lost all symptoms of his ailment, regaining his usual health and strength. Since that time, he has rapidly recovered his flesh.

It was exhibited, in this highly satisfactory case, in that form in which it is usually employed by confectioners, and known by them as German yeast. This is prepared on the continent by expressing ordinary yeast in linen bags, by which means all the fluid portions are separated, and a semi-solid mass remains, consisting entirely of the active vesicular structure of the plant. This is packed in bladders like lard, and keeps fresh for some time. Supplies are sent to the London confectioners regularly twice a-week; and it can be always obtained from the trade in any large town, at a very moderate price—about sixteen pence per pound.

The dose given in this case was one tablespoonful twice or three times daily, in milk. It was thus rendered very palatable. At first, the only inconvenience felt was a slight nausea, sometimes passing into actual vomiting, with eructation of carbonic acid probably; by giving the remedy after a meal, these disagreeable symptoms disappeared, and it was then borne very well. It was further serviceable in removing the obstinate constipation which had previously existed, and produced regularity in the alvine evacuations.

As diabetes is a comparatively rare disease in private practice, it is to be hoped that by communicating these results, even in their present incomplete shape, it may be the means of hastening the progress of the inquiry, and thus establish the real value of the remedy, as the attention of numerous experimenters will be called to the investigation, and the causes of success or of occasional failure will be at length ascertained.—*Association Med. Journal, April* 28, 1854, *p.* 374.

35.—ON THE PROGNOSIS AND TREATMENT OF DIABETES.

By Professor Schutzenberger.

The conclusions, &c., drawn by the author are as follows:—

1. Glucosuria is not an incurable disease; if the disposition to relapse cannot be disputed, it is, however, possible, with perseverance, not only to banish the sugar from the urine, but gradually to bring the patient to bear a mixed regimen into which farinaceous matters enter in considerable proportion without causing relapses.

2. The quantity of glucose passed by the patients is sensibly proportional to the quantity of farinaceous matters taken as food, and it is possible to infer departures from the prescribed diet from the increased amount of glucose in the urine. The quantity of urine is perceptibly increased in proportion as a larger bulk of fluid is ingested, and that of the glucose is similarly influenced by an augmented consumption of farinaceous principles.

3. Regimen should incontestibly have a prominent position in the treatment of

glucosuria. Milk, fatty matters, butter, oil, eggs, and meat, ought to form the bulk of the patient's food.

4. Total abstinence from farinaceous substances seems necessary to cause a complete disappearance of glucose from the urine.

5. Small quantities of bread (between three and four ounces a day) are generally well borne, and do not cause the reappearance of the sugar in the urine when the glucose has once ceased to be produced.

6. The powers of assimilation gradually increase, and it is possible, by means of chemical analysis, to demonstrate the point at which it is necessary to stop.

7. Regimen is powerfully seconded by certain medicines, and more especially by the use of opium in progressive doses, and of alkaline drinks:—in this disease the tolerance of opium is very great.

8. The glucose is undoubtedly produced in the digestive organs, but it is generally again removed by absorption, so that the solid fæces do not contain a trace of it.

9. Purgatives may diminish the quantity of glucose in the urine by removing with the fæcal matters a greater or less amount which would have been eliminated with that secretion.—*Bulletino delle Scienze Mediche de Bologna. January*, 1854. —*Dublin Quarterly Journal of Med. Science, Aug.*, 1854, *p.* 224.

36.—NEW TEST FOR SUGAR.

By Mr. John Horsley, Cheltenham.

If a freely alkaline solution of chromate of potash be mixed with urine suspected to contain sugar, and boiled, the liquor will assume a deep sap green colour, arising from the decomposition of the chromic acid; the reduced oxide of chromium being held in suspension by the potash. Such is the sensitiveness of this test, that five or six drops of saccharine urine diffused through water is sufficient to show the effect, which is infinitely more striking than even Moor's potass, or Trommer's copper tests.

I would, therefore, recommend a mixture in equal parts of a solution of the neutral chromate of potash and liquor potassæ to be kept in the chemical cabinet of every medical practitioner, labelled—Test for Sugar. When the quantity of sugar is very small, a piece of white paper placed at the back of the test tube will render the colour more distinct.—*Med. Times and Gazette, July* 15, 1854, *p.* 68.

[Dr. Bence Jones and others have raised the question whether any of the other constituents of urine are likely to produce a similar result to that of Mr. Horsley's test. Mr. Horsley thinks not. He thinks his test perfectly unique and reliable. He says:]

Perhaps the most satisfactory mode of showing it, would be in using a white ware evaporating dish to heat the urine in; then, on adding a sufficiency of the test and boiling, the green colour gradually developes itself, commencing at that part which is most contiguous to the flame, and spreads throughout the mass.

[Mr. Horsley asserts that no other constituent of urine than sugar will interfere with his test. He says:]

In the absence of diabetic urine, the following experiments will suffice to demonstrate the effects:—

Experiment 1.—Having adjusted your apparatus, put into the evaporating dish ten or twelve drops of simple syrup (cane sugar) and dilute it with water. On adding a little of the test, and applying the heat of the spirit lamp, *no change* of colour will be produced.

Experiment 2.—Take another evaporating dish, and having put in the same quantity of simple syrup diluted with water, add two or three drops of acid sulph. dil. and boil for a few minutes. This will convert the cane into grape (diabetic) sugar. If we now add the test and apply heat, the effect becomes apparent in the change of the colour of the liquid to that of an *intense green*, the shade varying with the quantity of sugar contained, as well as the test used.—*Association Med. Journal, July* 21, *p.* 653.

[Dr. Day says that Mr. Horsley merely describes a slight modification of Hüne-

feld's test, as will be seen by referring to 'Simon's Animal Chemistry,' p. 67 of the introduction. In answer to Dr. Day, Mr. Horsley says:]

I have seen Hünefeld's test, which is a solution of chromic acid ($Cr O^3$), and essentially different from that which I have proposed. If a solution of chromic acid, mixed with urine containing sugar, be exposed to the action of the sun's rays, or even diffused light, the liquid will become bistre brown. Every chemist knows that chromic and iodic acids are readily decomposed by organic matter; therefore, as a test, chromic acid ($Cr O^3$) alone is objectionable; not so, however, when a binalkalate of the teroxide of chromium ($Cr O^3 + 2 KO$) is used, as then sugar only is acted on in the manner I have stated. On following out my experiments, I find that by using the chromates of potash in different states of oxydation, we have a ready means of discriminating both grape and cane sugar, viz:—

Test for grape (diabetic) sugar,	Sol. chrom. potass. Liq. potass.
Test for cane sugar	Sol. bichrom. potass. Liq. potass.

In each case the liquors assume a deep sap green colour on being boiled, but the effect is not produced with cane sugar unless the solution be of a concentrated and syrupy thickness.—*Med. Times and Gazette, July* 29, 1854, *p.* 128.

37.—ON IRRITABLE BLADDER.

By Dr. H. Bence Jones, F.R.S., Physician to St. George's Hospital.

[The bladder is frequently made irritable by different states of the urine and various concretions, but it may be irritable without these being the primary cause. Dr. Jones gives us some of the most interesting examples of *direct* and of *sympathetic* irritation of this organ.]

The most simple instance of direct irritation may be observed when some irritating liquid substance, as cantharides, turpentine, or copaiba, exists in the water, or when some solid body, as a calculus, is lying in the bladder. These act as, for example, chlorine or a foreign body, in the bronchial tubes, would do in producing cough. I need not read you examples of irritation thus produced; I may, however, repeat, that simple excess of acid or alkali in the urine alone, rarely by itself, causes irritable bladder, though most decided instances of their effect may be observed.

Here, for instance, is an example of the effect of acidity. I was sent for one evening to a gentleman who was suffering intense abdominal pain, which came on at intervals of a minute, and which he attributed to indigestion. The fits of pain, on reaching to the greatest intensity, relaxed, soon again to recur. With a full dose of calomel and opium, ether, and sal volatile, in three hours this colic subsided; but he began then to complain of scalding and difficulty of passing the water. During the night he had constant calls, passing only a teaspoonful of urine at a time. For this he took nitre, carbonate of potass, and barley-water. The following morning the irritability of the bladder had diminished very much, and during the day it disappeared. On examining the urine made during the time the bladder was so irritable, I found no trace of blood-globules, mucous, or pus-globules, but only a state of very high acidity, which, in twelve hours, gave rise to a very large deposit of uric acid. I believe this excessive irritability of the bladder arose only from the intensely acid state of the urine.

Sometimes, instead of the direct irritant being applied inside the bladder, it acts from the outside. As examples of this, I may mention the irritable bladder of pregnancy, of abdominal tumours, and even of fæces which have accumulated in the bowels. As this last cause not unfrequently occurs, I will mention a remarkable example.

An officer, 60 years of age, complained of much irritation of the bladder. He was disturbed three times each night, and constantly in the day. The urine was healthy; it contained no blood-globules, no albumen, no gravel of any kind, when examined carefully. He stated that his general health was very good, but that he was troubled with very irregular action of his bowels. His usual aperient consist-

ed of a teaspoonful of powder consisting of two parts of sulphur, two magnesia, and one carbonate of soda. I advised some camphor and henbane at night, with cold water enema, and an aloetic aperient. In ten days I saw him again; the bowels were irritable with the aloetic pill; the pint of cold water had been injected each night. For the three nights previous to his visit, he had been disturbed only twice.

After a month I again saw him; he had persevered with the cold-water injection and the pills. He had so much less irritation of the bladder that he considered himself as cured.

[It is very likely that this case depended on the state of the bowels, and the improvement might be owing to their regular evacuation by cold water injections. Dr. Jones next proceeds to consider that direct irritation which depends on idiopathic inflammation, independent of the irritating qualities of the urine.]

Idiopathic inflammation of the bladder is the most common in advancing years, when the prostate gland ceases to be healthy; then some very slight cause is sufficient to determine the commencement of the inflammatory action which gives rise to most urgent symptoms of irritable bladder. Exposure to cold, the fatigues of a journey, inconvenience causing voluntary retention of the urine too long, each of these I have known to be the immediate cause of acute inflammation of the bladder, in consequence of which the water has been passed every hour, and even oftener. At first it is scanty, high-coloured, with blood-globules, and quickly pus-globules are found; and these continue when the inflammation is subsiding, and the frequency and urgency of making water are much lessened. Such an attack I have watched through its whole course; the symptoms rapidly becoming more and more serious, constant hiccup and nausea, producing prostration, which threatened the life of the patient. These symptoms, however, I have seen yielding after treatment; and in two months no trace of blood or pus remained in the urine, and the patient in all respects entirely recovered. More commonly the acute attack passes into chronic disease, and then with more difficulty is removed; or, after months and sometimes years of suffering, the kidneys become affected, and the patient sinks.

[He illustrates this irritability by some very interesting cases, such as by crushing a calculus, cutting for stone, and from a stricture.]

In most cases, as the mucous membrane becomes diseased, the urine gradually becomes so also, and then it also acts on the bladder, greatly increasing the suffering and the difficulty of the cure. Thus the two causes acting together, re-act also on one another. The same result is produced when the irritable bladder is caused at first by the state of the urine alone, as, for example, when it is highly acid or alkaline, then it slowly acts upon the mucous membrane, and finally, both the state of the membrane and the state of the urine together hasten on the disease.

In distinction to these cases of direct irritation, there are others which I may class together under the head of sympathetic irritation, in which apparently neither the urine nor the mucous membrane of the bladder are perceptibly different from the healthy state.

The most frequent cause of this sympathetic irritation is probably disease of the kidney itself, although it is by no means the fact, that in all diseases of the kidney irritation of the bladder is produced. Most commonly the irritation occurs when some calculus or inflammatory action exists in the pelvis of the kidney; then a small quantity of purulent matter mixed with the urine may indeed pass into the bladder, but this usually bears no proportion to the amount of irritation which occurs. The first explanation of this irritation that presents itself is that the inflammation extends down the ureter to the bladder; but on *post mortem* examination this cannot usually be proved, and hence these cases must be classed under the head of sympathetic, and not of direct irritation. The following case will show the relief which in this state may sometimes be obtained:—A gentleman, aged 58, consulted me for irritable bladder. He said that he was usually disturbed eighteen times at night, and some nights not less than twenty-four times. He had suffered thus for four months, and, to a less degree, for three years. Three months previous to my seeing him he was examined for stone and prostatic disease, and neither was found. Long since, on one occasion, he passed some blood. The left loin was tender on pressure; the left testicle was enlarged. The urine was highly acid, and contained pus and albumen, and doubtful blood globules. I advised the use of

ten grains of extract of henbane, and ten grains of soap and opium pill as a suppository each night.

I saw him again six months afterwards. He said that the suppository had given him immediate relief. The frequency was soon reduced to seven times, and sometimes to six times nightly. The quantity passed each time was under two ounces. The urine was strongly acid, and some infusoria were perceptible immediately after the water was passed. He had used the suppository with benefit every night, but he considered that it caused dryness of his mouth, and obliged him to take three times as much castor-oil as he used to take. He finds that when the oil does not act on the bowels he passes his water much more frequently than he otherwise would do. From the albumen, blood, and pus, with the tenderness in the left loin, it is probable that in this case the left kidney was affected; and from the acidity of the urine, after the disease had been so protracted, it is probable that the irritation of the bladder was sympathetic. Sometimes instead of the suppository great relief may be obtained by means of an opium injection, but the radical cure must depend on the removal of the cause, and when this is not possible, then palliative treatment may become the means of prolonging life. Hysterical irritation of the bladder belongs to the class of cases of which I am now speaking. In all supposed hysterical diseases, the first thought should be, whether the hysteria is the cause or the consequence of the disease. Is the hysteria idiopathic or symptomatic? Often it will be found that hysteria is but one symptom of a complaint, which, in addition to other effects, produces irritable bladder; but occasionally no other complaint can be detected, and no cause for the irritable bladder can be found except the hysteria itself. The previous history will often assist in giving the true answer to the question which you must ask before you can commence your treatment on reasonable grounds.

Moreover, whatever may be the cause of the irritable bladder, yet in all cases, whether of direct or sympathetic irritation, both in men and in women, a smaller or a greater part of the irritability may be always traced to nervousness. This is usually seen in the fact, that in such patients a passing thought will suddenly cause the most urgent desire to empty the bladder, and that while the mind is fully occupied, the irritability will be less distressing. Occasionally not even a passing thought is required, but a recurrence of the circumstances which had on some former occasion preceded the desire to pass the water will be followed by the same effect. Thus, one patient told me that he never could enter his own house without an immediate desire to pass his water, and this desire would become urgent even while his mind was fully occupied with some engrossing subject. Another patient told me, that whenever he went into his dressing-room, it mattered not for what cause or how soon after he had passed his water, immediately on entering the apartment the desire occurred. Another, who seldom could retain the water for an hour when he had an opportunity for passing it, could remain for two hours in church without inconvenience. I am often told, "I could remain in church or elsewhere for two hours without any inconvenience, provided the thought of the impossibility of making water did not occur; but the moment it occurs I am compelled to retire." These instances are sufficient to show how irritable bladder may become a habit. In children we admit this without difficulty; but in adults, where there is also disease, it is more difficult to determine the amount of effect that belongs to each cause. I have known the habit caused by the pressure of pregnancy to last afterwards, and to become a constant annoyance. In young persons, it may be sometimes observed that no remedy is sufficient to overcome the habit, and it is broken only when feelings of shame become most powerful; while in the old I have frequently seen the loss of all feeling of shame fix the bad habit and render it more irresistible.

In irritable bladder, more perhaps than in most diseases, it is requisite that, before any method of treatment is begun, you should endeavour to obtain clearness as to the cause of the disease. It is produced by so many different causes, that specific treatment of the irritation will rarely suffice. You may palliate it as you would a cough by opiates and sedatives; but to effect a cure the cause of the disease must be found, and be capable of removal. Ask yourselves, then, first, is the irritation direct or sympathetic; if the former, is it simple or compound, internal or external? While endeavouring to remove the cause, you will often find palliatives of great importance. Of these the most important are camphor and opium. Three grains of camphor and half a grain of opium made into a pill taken twice or three times daily,

will often prove beneficial. In some cases, palliatives fail, and cure is impossible. In these, mechanical contrivances of the present day do away with much of the discomfort and annoyance which otherwise would have to be endured.—*Med. Times & Gaz., June* 3, 1854, *p.* 561.

38.—ON RETENTION AND SUPPRESSION OF URINE.

By Dr. H. Bence Jones, F.R.S., &c.

[Retention and suppression of urine are sometimes of great value as means of diagnosis. Dr. Bence Jones subdivides stoppage of the urine according to the place at which the stoppage occurs:]

Thus we shall have—1st. Stoppage in the urethra. 2nd. Stoppage in the bladder. 3rd. Stoppage in the ureter and pelvis of the kidney. 4th. Stoppage in the secretory structure of the kidney.

1st. Stoppage in the Urethra.—Stricture of the orifice is occasionally met with by the surgeon: on this I shall not dwell. Rarely I have known a calculus so firmly fixed that it could not escape, and filling the orifice so that the urine could not pass. In one case, the slightest surgical treatment liberated the calculus. In another, the gradual dilatation by the fluid behind effected a spontaneous cure. In women stricture of the orifice, or rather, I should say, of the urethra, I have known to occur from prolapsus uteri, as in the following case:—A patient, fifty years of age, was admitted into a surgeon's-ward, with excessive prolapsus. It had lasted for many years, had gradually increased, and, on admission, there was great external protrusion, and considerable inflammation of the womb. She had been in a state of muttering delirium for some days, and was excessively emaciated, and in a state of great weakness; her delirium increased, the brown tongue never improved, she became perfectly comatose, and died a week after her admission. On examination, the bladder was enormously dilated, the ureters were both much enlarged, the mammary processes of the kidneys were gone, and the calices of the kidneys encroached on the secreting structure. The mucous membrane throughout was inflamed.

2. I pass on, secondly, to partial or complete stoppage of the urine in the bladder.

The most usual cause of partial obstruction at the neck of the bladder is prostatic enlargement. A patient comes to you complaining of inability to retain his water. He says, that at night it passes in his sleep. He is advancing in life, and, on inquiry, you find that the stream of water has long lost the force with which it used to be expelled. If you examine the water, it may be feebly acid, neutral, or alkaline. It usually contains pus, and often some blood-globules; there is usually no pain at any time anywhere.

From such a history you may safely conclude that the bladder is not emptied; but that there is partial retention of urine; that the urine which remains behind in the bladder decomposes, and makes that which is passed neutral or ammoniacal, according to the degree of decomposition. Gradually the mucous membrane becomes more and more inflamed, and then the change in the reaction of the water is greater, it becomes more purulent, and ultimately, though it may be very gradually, the patient is worn out by the irritation and discharge of purulent matter from the bladder, or, if the inflammation spreads to the kidney, he dies comatose.

By the careful and constant use of the gum catheter these evils may be avoided; and I have known a medical man thus keep off diseases from himself for twenty years by the use of the catheter night and morning daily.

The frequency of the use of the catheter in retention of urine in the bladder, from this or other causes, must be determined by the reaction of the urine. The object to be obtained is to prevent the ammoniacal urine from increasing the inflammation of the bladder. If the urine can be kept neutral or acid by drawing off the water once daily the object will be attained; but if, after drawing off the water at night, that which is passed in the morning is found alkaline, it is evident that that which remains behind in the bladder must continue during the day to act injuriously on the bladder until it is drawn off again at night. The object of drawing off the water is not merely to empty the bladder, but to take away the alkaline urine which is irritating the mucous membrane; and the sooner it is taken away after it is formed the less will the membrane be irritated.

If, then, the morning or mid-day water be alkaline, the catheter should be used twice a-day. Without this, medicines are useless; with it, they often are scarcely required.

If the passing of the catheter also becomes a cause of irritation, then the least of the two evils must be endured.

A partial retention of urine giving rise to exactly the same results, sometimes is caused, not by obstruction at the neck of the bladder, but by chronic inflammation of the bladder itself, from gonorrhœa or other causes. The lymph effused in the submucous and muscular coats prevents contraction and consequent expulsion of all the urine, and thus the train of effects I have just described may be produced.

I have known inflammation of the peritoneum give rise to adhesions between the bladder and bowels, and thus also the contraction of the bladder has been stopped. Malignant disease, outside or inside the bladder, will sometimes also prevent the expulsion of the urine, and thus cause a partial retention, which will be speedily followed by putrefaction.

In addition to these causes of partial retention of urine in the bladder, there are still other causes which deserve notice.

A medical man came to me saying that something was wrong with his water, for he had only passed three or four ounces the day previously, and that he felt no desire to pass it. He was, he said, in good health, and never had anything the matter with his bladder. An instrument was passed after he had made what he could, and it was found that above a pint of water was retained in the bladder. It was alkaline, purulent, and contained blood-globules. No disease of the prostate could be found, and the inability to empty the bladder probably depended on a loss of contractile power of the muscular coat. This loss had slowly increased, and was unperceived until it had reached a most notable degree. In this case the use of the catheter was determined by the state of alkalescence of the urine. I have heard of other cases in which the gradual distension of the bladder by urine has caused a temporary loss of power of the muscular coat of the bladder, and, unperceived, a large fluctuating tumour of the abdomen has resulted, and when the catheter has been introduced, no urine has flowed until pressure has been made on the body; then two quarts of fluid have been drawn off.

[3. Dr. Jones next proceeds to stoppage in the ureter and pelvis of the kidney. It may occur in any part of this canal, though the more common is where the upper part of the ureter loses the funnel shape, which is a continuation of the pelvis of the kidney. The other most frequent point is at the entrance of the ureter into the bladder.]

There can be little doubt that a calculus may obstruct the ureter, and produce dilatation of the kidney and ulceration of the constricting portion of the ureter, and then passing into the bladder, the ulcer may contract and leave the ureter almost or altogether impervious. A patient was admitted into St. George's Hospital, with fracture of the lower articulating extremity of the tibia. He was able to walk until the ninth day after the accident, when shivering came on followed by much constitutional disturbance, and he rapidly sank. The day previous to his death, he, for the first time, felt inability to void his urine, which was drawn off by a catheter. On examination, after death, it was discovered that one of the kidneys was destroyed, by pressure, and was converted into a number of membranous dilatations and contained two quarts of urine. On attempting to pass a probe down the ureter, it was found to be almost impervious, so as hardly to admit a bristle. The opposite kidney and the bladder were sound.

The inability to pass the urine the day before his death was probably quite unconnected with the disease of the kidney, at least the same thing occurs in cases in which the kidneys are perfectly healthy. For example, a gentleman who was gradually worn out with attacks of obstruction of the bowels, consequent on stricture of the upper part of the descending colon, which produced fæcal abscess, was generally able to pass his water without difficulty. He was excessively wasted and enfeebled, but apparently not more ill than for some days previous, when, on trying to pass his water, he found himself unable to do so. The urine was drawn off in the evening. He was restless during the early part of the night; towards morning a change occurred, and he died in the afternoon. No urine was passed from the time it was drawn off on the day previous till the time of his death. The kidneys were both healthy.

4. **On Stoppage in the Secreting Structure of the Kidney.—Suppression of Urine.**—Many cases are called cases of suppression of urine which have no right to this title. I have frequently been told of cases of suppression of urine in which no catheter was ever passed to certify the absence of urine from the bladder. Nay, I have even been told that a patient laboured under suppression of urine because no water was passed, and none could be drawn off by the catheter, and yet, a few minutes afterwards, a more skilful surgeon has drawn off some pints of water.

In the treatment of retention and suppression of urine, our first thoughts must be directed to the cause of the disease. The obstruction must be overcome; the congestion must be diminished; the inflammation must be subdued. When nothing else remains to be done, we must endeavour, by acting strongly on the bowels by hydragogues, and on the skin by sudorifics, to relieve the poisoned system, and thus, at least, to obtain more time for the action of these antiphlogistic remedies which give the greatest hope of recovery.—*Med. Times and Gazette, June* 10, 1854, *p.* 587.

39.—*On Irritability of the Bladder.* By Henry Thompson, Esq., Honorary Surgeon to the Marylebone Infirmary.—[A sort of rheumatic affection sometimes follows gonorrhœa, which is accompanied by frequent micturition but not of an inflammatory character. The general health may be depreciated. It may be benefited by iodide of potassium and tonics, such as cinchona combined with Plummer's pill. Sometimes colchicum and blue pill, and at other times guaiacum will be very serviceable. Ten grains of guaiacum powdered with an equal dose of magnesia may be given once or twice a day.

In other cases an irritable state of the bladder may be associated with diseased prostate gland. On this Mr. Thompson says:]

Frequent micturition is sometimes associated with an affection of the prostate, or more properly speaking of the urethra as it passes through that gland, when the symptom occurs in youth, or at an early adult age. Thus, after an habitual inordinate indulgence of venereal appetites, sometimes after repeated attacks of gonorrhœa, or quite as commonly after one attack only in certain constitutions which manifest undue excitability of the urinary apparatus, and in which catarrhal discharges generally are very obstinate and hard to cure, the patient complains of vesical irritability, attended or not with some gleety discharge. On passing a middle-sized catheter, no organic narrowing is discovered, but there is extreme sensibility of the canal, after the instrument has traversed some five or six inches of the anterior part. Acute cutting pain is then complained of as it is carried onwards to the bladder. The catheter may be tightly grasped, and is not to be removed without some pain. Perhaps it is not possible to say what is the exact condition of the urethra here; probably there is some vascular congestion, or subacute inflammatory action existing in the mucous membrane, analogous to that met with elsewhere. This, however, we know, that in four cases out of five, the passing of a catheter, repeated at two or three days' intervals, for a short time, almost entirely removes the hypersensibility. The muscular constituents of the canal are perhaps stimulated to contract by the presence of the foreign body, the enlarged or relaxed capillaries thus emptied of their contents gradually return to their normal size, and the congestion ceases to exist.

Thus it happens that on the first or second time of using the instrument, some slight bleeding may occur, which appears to have a beneficial effect rather than otherwise, and it rarely takes place afterwards. The actual presence of the pathological state described, is, in most instances, a matter of conjecture only, it must be confessed, but there is a strong probability that it may not unfrequently be affirmed. Now sometimes it seems to happen that this affection, when much neglected, or if marked by more than usual severity, may implicate the neck of the bladder also to a greater or less extent, and set up considerable irritability of this organ. Such a condition, as might naturally be expected, is aggravated by the stimulating diuretics frequently employed, which contain some volatile or acrid principle, such as buchu, copaiba, &c. I have met with several cases notably illustrating these remarks. In this complication, also, if there be no purulent discharge with the urine, which is sometimes the case, I have found the benefit of passing instruments into the blad-

der, beginning, say with No. 7 or 8, and passing on to 10, 11, or 12. But if these fail, or if there is some little purulent discharge, an application of the nitrate of silver to the prostatic portion of the urethra and neck of the bladder is exceedingly useful as a curative means. Sometimes the whole bladder is advantageously injected with solutions of the nitrate; it is, I believe, however, generally sufficient to cauterize the parts indicated. Nevertheless, there are conditions of the vesical mucous membrane in which such injection is highly beneficial. When the neck of the bladder has thus become affected, the urine may be decidedly albuminous, and yet no serious renal disease may co-exist. It would then seem to be a result of irritation, secondary to, and depending upon, the condition of the bladder; for on remedying this, the albumen disappears. In such a case, the absence of the constitutional signs of organic disease of the kidney, and the non-appearance of diseased epithelium or casts in the urine, will leave the observer to watch closely for the source of the albuminous product, and perhaps to connect it with some functional, and not with any organic derangement.

We meet with vesical irritability, accompanied by more or less of mucus in the urine, or muco-purulent deposit, in which, no evidence of inflammatory action being present, we cannot feel justified in regarding it as a condition of cystitis, although it is often, but not necessarily always, a sequel of it. It seems to be due rather to an atonic or relaxed condition of the mucous membrane, in which the phenomena described occur, and in which much distress from the frequent calls to pass urine is experienced, but which will not be combated successfully by any antiphlogistic treatment. It is for this class of cases especially that stimulating injections for the bladder are so useful. Among them may be mentioned the nitrate of silver, in the proportion of half a grain to two grains to the ounce of water. The strength, however, may be greatly increased, even to ten or fifteen grains for some cases. Another useful formula is half a grain to a grain of the acetate of lead, with two grains of the extract of opium, to the ounce of water. The nitric acid is particularly indicated when the urine is charged with the earthy phosphates, and may be added to water in the proportion of one or two minims to the fluid ounce. Creasote and copaiba have both been employed by continental surgeons in these circumstances; the former in the proportion of one minim, the latter in that of thirty to sixty minims, to the ounce of fluid; but I have no experience of the effect of these. Plain warm water is often of great service in most cases where much mucus is present, and it may then be advantageously injected once or twice a day. The employment of a continuous stream by means of the double-current catheter with a free opening for the transit of mucus, is best adapted for the purpose. When there is much pain, as well as irritability, the extract of opium may be added to the water; but, generally speaking, a sedative effect is much more efficiently obtained by the use of a suppository. When the contents of the bladder are extremely offensive, a weak solution of chloride of soda is sometimes of considerable value as an injection. It may be repeated every day. The strength to be employed should range between six and ten minims to the ounce of tepid water. In the operation of injecting the bladder, instead of fixing the nozzle of the syringe directly upon the catheter itself, it conduces greatly to the comfort of the patient to connect the two by means of a pliable or elastic tube, so that no impetus be transferred to the sensitive organ from the hand employed in propelling the fluid. The vulcanized India-rubber bottle, fitted with pipes and valves, like those of the ordinary enema apparatus, is very preferable to the syringe for this purpose, because it can be worked by the operator with one hand only, leaving the other free to adjust the catheter or turn the stop-cock. With regard to internal remedies, the alkalies before alluded to, provided the urine is not already alkaline, as a general but not invariable rule, or the mineral acids if it be strongly so, combined with the infusions of diosma or buchu, or of the uva ursi, with full doses of hyoscyamus or of camphor, and sometimes with copaiba or chios turpentine, appear to be the most efficient in non-inflammatory conditions of the bladder. The tinctures of iron and hyoscyamus together, or of iron and hop, sometimes exercise marked benefit in weak and anæmiated subjects. From decoction of the pareira brava, in doses of the ordinary Pharmacopœia strength, and also when augmented greatly beyond that standard, I have been disappointed in obtaining the good results attributed to its use, and in cases which have been apparently adapted for it. We have the highest authority, however, for its value in some circumstances.—*Lancet, June* 3, 1854, *p.* 594.

40.—*Treatment of a Nephritic Attack from Calculus in the Kidney.* By Dr. H. Bence Jones.—[When a stone has formed in the kidney, four things may take place.—1. It may remain on the pelvis of the kidney, and cause but little irritation. 2. The symptoms may be generally slight, but occasionally severe. 3. It may pass out of the pelvis, and cause obstruction in the ureter. 4. It may pass downwards into the bladder.]

The treatment of a nephritic attack consists in relieving pain and preventing inflammation. While the calculus is quiet in the pelvis of the kidney, but little can or need be done. That little consists, 1st, in hindering the increase of the concretion in the kidney, by correcting the unhealthy state of the urine by diet and medicine, according as acid, alkali, or oxalate of lime, predominate; and, 2ndly, in hindering inflammation in the pelvis of the kidney by avoiding severe exercise. I have frequently known patients advised to take strong exercise, but it is not during exercise that the calculus usually quits the kidney for the ureter. Most commonly I am told by patients that, in turning in bed at night, they were suddenly seized with the nephritic symptoms.

The first object must be to allay the pain. The hottest hip-bath, at the temperature of 110° or 112° Fahr.; chloroform externally and internally; opium: the injection of warm water into the rectum,—by these the first indications of relieving pain may be fulfilled.

The second object is to guard against inflammation; this is effected by general or local bleeding, by aperients, sudorifics, and purgatives.

The complaints which may be mistaken for a nephritic attack are lumbago and colic. I know of no certain means of distinguishing these diseases except the microscopic examination of the urine. The absence of vomiting in lumbago, and the seat of pain in colic, may lead you to a correct opinion; but the presence of blood-globules when the urine is examined with the microscope, is the sure indication that the attack is caused by the descent of a renal calculus, and the affection of the testicle may be generally found confirmatory of this opinion. Do not think that in every case in which blood is found by the microscope in the urine a calculus necessarily exists in the kidney. Blood globules may appear in the urine from simple congestion of the kidney in many diseases,—from Bright's disease, from inflammation of the mucous membrane alone, from congestion or disease of the prostate, from scrofulous disease, or malignant disease; and in some of these diseases the difficulty of distinguishing between them and renal calculus is by no means easy.—*Med. Times and Gazette, May* 27, 1854, *p.* 533.

41.—ON MALIGNANT AND SCROFULOUS RENAL DISEASE.

By Dr. H. Bence Jones, F.R.S.

[Renal diseases are often very difficult of diagnosis. We ask ourselves first, perhaps, whether it be Bright's disease; next whether any concretion be present, or whether inflammatory action exist; and, lastly, we ask whether the case be one of malignant or scrofulous disease. Neither malignant nor scrofulous disease admits of easy relief, and, therefore, the medical attendant should be careful in giving his prognosis. In the following remarks we find some valuable information on two things; first, some of the difficulties of diagnosis in malignant and scrofulous affections: and, secondly, on the palliatives which are mostly beneficial.]

A medical man from the country applied to me in consequence of the occasional passage of blood in the urine after exercise. He complained of some pain in the loins; he was easily fatigued; spoke of his fear lest he should be obliged to give up his profession. He looked worn, and the urine contained oxalate of lime, crystals, and blood-globules. I was unable to detect any other disease. There was a very small quantity of albumen in the urine. The pulse was 100.

From the occurrence of blood after exercise, from the oxalate of lime crystals, and from the fact that, after the most careful examination, I could find no proof of any disease of the kidney, I conjectured that a renal calculus existed. I saw no more of this patient. The *post mortem*, which occurred months afterwards, was

thus reported:—Left kidney about the size of two fists; extremely hard, and infiltrated throughout with opaque, white, solid encephaloid matter. Right kidney healthy. Great enlargement of the lymphatic glands in front of the spine and at the upper part of the abdomen. Liver enlarged and studded throughout its whole extent with masses of encephaloid matter, varying in size from that of a pea to that of a pullet's egg. Lungs studded throughout with small, opaque, hard, white masses, from the size of a pea to that of an almond. Portions of the liver, lung, kidney, and lymphatic glands, when examined under the microscope, all showed the same encephaloid deposit.

In this case, then, no appearance indicating malignant disease was found during life in the urine; nor, though he was seen by many, was the true nature of the case known until after death.

A mechanic, aged 37, was sent to me with a statement that he was suffering from malignant disease of the kidney. The diagnosis was founded on the microscopic appearances of the urine. He first passed blood in quantity a year before I saw him; then a little blood was passed when he finished making water. He had some pain in the left lumbar region. On examination of the urine, I could find only a few blood globules, and very considerable quantity of oxalate of lime. I could find no proof of malignant disease. Six months after this, I again saw him. He had had another slight attack, but otherwise had been much better. I could not, on this occasion, find any proof of malignant disease. I again saw him a year afterwards, when he told me his medical man had changed his opinion regarding the existence of malignant disease in consequence of his improvement. For the last year I have had no account of this patient.

In this case, probably, the microscopic appearances misled the observer in the diagnosis; for the apparent recovery of the patient was altogether opposed to the assumption of malignant disease. The error, which I mention only to show the difficulties that exist, was of the opposite kind to that mentioned in the first and following case.

T. A., aged 45, was admitted into St. George's Hospital under my care, April 16, 1851. He stated, that in August, without any cause, blood first appeared in the urine. He had pain in the right side, under the ribs, and in the loins. He was leeched, and that relieved the pain; but it never entirely left him. Since then, the blood has passed at variable intervals; but while resting for three months he passed none. For the month before his admission he passed it more frequently, and for the last two days the urine has not been free from blood, the urethra being at times blocked up by the coagula. There has been no pain or retraction of the testicle; but he has had severe pain in the right groin, and in the inside of the thigh. The urine, when it was free from blood, was free, plentiful, and clear. The bowels were regular. In two days, with perfect rest and gallic acid, the blood disappeared. After some days it returned, and was increased after a fit of coughing. The gallic acid was increased, but the blood increased rather than diminished. An infusion of matico, after two days, again stopped the blood; it recurred after some days, and was again stopped by matico. He remained rather improving until the 4th of June, when he was made an out-patient. When the water was clearest and freest from blood, the microscope always detected some blood-globules, some oxalate of lime; no pus-globules, and no other remarkable appearance. There was always some albumen in the urine. I saw nothing of him until the 3rd of February, 1852, when he said, that since he went out of the hospital he had had blood in the urine occasionally, about once in six weeks at first, then at longer intervals; now the blood comes very rarely, but the pain in the loins has been increasing. It is a heavy, aching pain in the right loin, with numbness down the inside of the thigh not so far as the knee. There is no retraction of the testicle. Is obliged, on account of the pain, to keep from work. Standing, even for less than an hour, brings on the pain. He thinks himself very well in other ways. The urine still contained blood-globules and oxalate of lime.

In November, 1852, he came to me suffering with most intense neuralgia of the face and back of the head. With difficulty it was relieved by quinine. He had the same pain as before in the loins, but no blood that he knew of in the urine.

On the 19th of December, 1853, I was requested to go to him for most violent

hæmoptysis. He was breathing with the greatest difficulty, and bringing up florid blood in considerable quantity. He was pulseless, and with difficulty able to speak. The following day he died.

On examination by Mr. Prescott Hewitt, the right kidney was larger than natural, and was of a globular shape. It was converted into a mass of encephaloid disease, in which all traces of the structure of the kidney were lost. The pelvis of this organ, much dilated, was also filled with encephaloid deposit, which extended far down the ureter, and blocked up its cavity to within two or three inches of the bladder. This encephaloid deposit was, for the greater part, formed of a firm structure of a fawn colour; but in some parts it was of a reddish tinge, and broke down readily. The other kidney and the bladder were healthy. No cause for the hæmorrhage was found in the lungs, which were healthy. The heart was also healthy.

In this case, microscopic examination of the urine, repeated frequently, gave no indication of the malignant disease. The occurrence of the blood when no exercise was taken, made me suspect scrofulous disease rather than calculous; but in this also I was mistaken.

Sometimes no urinary symptoms whatever are observed, the malignant disease existing in another more vital part, causing the renal disease to be entirely overlooked.

Sometimes another urinary disease exists at the same time, and thus the symptoms are made obscure. Thus granular disease of the kidney may exist with encephaloid disease, and the secondary symptoms produced by the former may alone force themselves on the attention.

As far as I have hitherto observed, then, the microscope has not enabled me certainly to predict the presence of malignant disease of the kidney. I must say the same of incipient malignant disease of the bladder. The symptoms of this disease are more distressing, than when the kidney is affected; but even when I have felt confident of my diagnosis, I have sometimes been unable to confirm it by the microscopic discovery of malignant cells, in consequence of the highly alkaline urine acting on the cells. In other cases, in which small masses of malignant matter were passed, the microscope has confirmed the diagnosis which the general symptoms had made most probable. Nor has the microscope, as yet, enabled me to speak certainly regarding the commencement of scrofulous disease. The means of distinguishing between scrofulous disease and calculus of the bladder or kidneys, are very insufficient; but by careful attention to the progress of the symptoms, more certainty may be attained than by help of the microscope.

In conclusion, then, as no certain microscopic or chemical test exists for the early stage of scrofulous and malignant disease, it will be best, in doubtful cases, to refuse to give a positive opinion. Suspend your diagnosis until, by carefully watching the progress of the case, you find sufficient evidence of the nature of the disease. Thus, your diagnosis will remain uncorrected, and your prognosis will be sure.—*Med. Times and Gazette, June* 17, 1854, *p.* 613.

42.—*On the Development of Vibrio Ureola, Bodo Urinaris, &c., in Urine.* By Dr. Arthur H. Hassall.—[The following are the conclusions which Dr. Hassall arrives at in his interesting paper:]

1. That vibriones are not developed in strongly acid urines so long as these retain their decided acidity.

2. That so soon as such urines have lost a considerable part of their acidity, and have become but feebly acid, vibriones begin then to appear. It is thus that the occurrence of both torulæ and vibriones in the same urine is explained. While the urine is strongly acid the torulæ are formed, but as soon as the acidity becomes greatly reduced, the vibriones make their appearance.

3. That vibriones are invariably developed in different proportions in all those urines which are either feebly acid, neutral, or more particularly alkaline.

4. That the greatest development of vibriones takes place in all those urines which contain most animal matter, as mucus, epithelium, and albumen, and which, at the same time, are decidedly alkaline. Hence, although not an exact test of the

degree of alkalinity of any one sample of urine, or of the amount of animal matter (especially albumen) present, yet by their early appearance, and by the quantity developed, to a certain extent vibriones may be regarded as affording valuable information on both these heads.

5. The vibriones are freely developed when the urine is exposed to the air.

But vibriones are not the only kind of animalcula found in urine. A second species not unfrequently occurs in great abundance, entirely different in size, form, and structure; this is the bodo urinarius. The author gave a minute description of this animalcule, showing that they multiplied by *fissiparous* reproduction. Some carefully executed drawings illustrated the characters of this animalcule. The author's investigations had led him to the discovery of a third kind of fungus developed in alkaline urine, entirely different from the two which had been described in a former communication. Like them this recently discovered fungus presented three distinct stages of growth—sporules, thallus, and aerial or perfect fructification. These were severally described. The author abstained from giving this fungus a distinctive name, wishing, before doing so, to ascertain whether it had heretofore been described by any other observer.—*Lancet, July* 8, 1854, *p.* 10.

43.—*The Elimination of Urea.*—In the 13th volume of the 'Archives Gen. de Médecine,' 4th series, will be found an interesting communication by Bernhard and Barreswil, on the elimination of urea from the blood after extirpation of the kidneys. It appeared to them a singular circumstance that whenever the kidneys of an animal had been extirpated, a period of time, varying from twenty-four to forty-eight hours, always elapsed before the blood yielded on analysis any traces of urea; and it immediately became a question in what way the urea escaped from the system during this interval. To ascertain this point, a series of experiments on dogs was instituted. The kidneys of these animals were first extirpated, and the blood and intestinal contents were subsequently carefully tested. Two of the dogs perished quickly after the mutilation, one from peritonitis, and the other from suffocation. In neither of these could any urea be detected in the blood. The gastric fluids liberated, on the addition of caustic potash, a suffocating ammoniacal odour; the intestinal fluid and bile, when similarly treated, disengaged also large quantities of ammonia. In the other experiments, the animals survived longer, and died, finally, in a state of considerable exhaustion. It was observed, in these instances, that the gastric juice was much increased in quantity, and was secreted without intermission both fasting and during digestion. Large quantities of ammoniacal vapour were liberated from it by the addition of caustic potash. No urea could be detected in the blood till the animal had become weak and exhausted, and the quantity of gastric juice secreted had undergone a considerable diminution in consequence.

Bernard and Barreswil conclude, from these experiments, that after the kidneys of an animal have been extirpated, the urea is eliminated by the secretions of the intestinal tube, and chiefly by the gastric juice, in the form of an ammoniacal salt; and that no urea can be detected in the blood till, from progressive diminution of the vital powers, the intestinal fluids become more and more diminished in quantity, and thus the supplementary channels for the separation of urea are cut off. Retention of urea, they argue, is not a direct result of suppression of urine, but rather a consequence of the loss of vigour, which follows it; for if the vital powers remain active, the urea escapes from the blood by other portals, when those provided for its natural elimination are closed. True it is, that in no instance was urea, as such, detected in the intestinal contents; but this circumstance is accounted for by the authors, on the supposition that it is decomposed by the acid of the stomach, and transformed into lactate or phosphate of ammonia, and in proof of this, they state that urea, when introduced into the stomach, is always changed into a salt of ammonia before it can be absorbed.—*Dublin Hospital Gazette, July* 1, 1854, *p.* 171.

44.—ON A NEW AND SIMPLE METHOD OF DETERMINING THE AMOUNT OF UREA IN THE URINARY SECRETION.

By Dr. Edmund W. Davy, Lecturer on Chemistry, Dublin.

Urea has long been regarded with much interest by scientific men, on account of its physiological and chemical relations.

It represents one of the last stages of a series of metamorphoses or changes, which nitrogenous matter undergoes in the animal economy, and is the form under which the detritus of pre-existing nitrogenous tissues, which have become effete, principally passes from the system.

This interesting organic base, urea, is not only formed during the exercise of the vital functions in man, and some of the higher animals, but is also produced during the chemical decomposition of a number of substances containing nitrogen; and the chemist can now obtain it in any quantity by artificial means, and thus imitate one of the most important results of the chemistry of life.

In reference to medicine, urea is not without some practical interest, as it is well known that during various diseased conditions of the system the quantity of urea eliminated from the blood by the action of the kidneys, and excreted in the urine, is occasionally subject to great variation; and some ready means of ascertaining its quantity in that secretion might frequently aid the physician in forming his diagnosis of certain diseases.

Different means of effecting this object have, from time to time, been proposed; but all the methods hitherto recommended, with perhaps the exception of Baron Liebig's recent one, require for their execution much time and trouble, and in some cases complicated and expensive apparatus, and though capable, in experienced hands, of yielding tolerably accurate results, would, in the case of those not much practised in chemical manipulation, and perhaps unaware of the many sources of error to be guarded against, give anything but correct results; and, therefore, inapplicable to the greater number of those desirous of a quick and easy method of determining the amount of urea in urine.

The method I propose is one of extreme simplicity, and can be performed by almost any one in a very few minutes, and is capable of yielding results sufficiently accurate for all practical purposes.

It is founded on the fact I have recently observed, that urea is very readily decomposed by the chlorides or rather hypochlorites of soda, potash, or lime, and that its constituent nitrogen is evolved in the gaseous state, and from the quantity of gas evolved I estimate the amount of urea present.

After trying different means of carrying out this fact, with a view of making it available to determine the quantity of urea in urine, I found that the following very simple one seemed to answer the purpose completely.

I take a glass tube, about twelve or fifteen inches long, closed at one end, and its open extremity ground smooth, and having the bore not larger than the thumb can conveniently cover. This I fill more than a third full of mercury, and afterwards pour in carefully a measured quantity of urine to be examined, which may be from a quarter of a drachm to a drachm, or upwards, according to the capacity of the tube; then, holding the tube in one hand, near its open extremity, and having the thumb in readiness to cover the aperture, I quickly fill it completely full with a solution of the hypochlorite of soda (taking care not to overflow the tube), and then instantly cover the opening with the thumb tightly, and having rapidly inverted the tube once or twice to mix the urine with the hypochlorite, I finally open the tube under a saturated solution of common salt in water, contained in a steady cup or small mortar. The mercury then flows out, and the solution of salt takes its place, and the mixture of urine and hypochlorite being lighter than the solution of salt, will remain in the upper part of the tube, and will therefore be prevented from descending and mixing with the fluid in the cup. A rapid disengagement of minute globules of gas soon takes place in the mixture in the upper part of the tube, and the gas is there retained and collected.

The tube is then left in the upright position, till there is no further appearance of minute globules of gas being formed; the time being dependent on the strength of the hypochlorite and the quantity of urea present; but the decomposition is gene-

rally completed in from three to four hours. It may, however, be left much longer, even for a day, if convenient, and having set the experiment going, it requires no further attention; and when the decomposition is completed, it is only necessary to measure the quantity of gas produced, by transferring it into a graduated tube or measure.

I have generally used a graduated tube in the first instance, as it saves the trouble of transferring the gas, and incurring the risk of losing some of it in the process.

That which I would recommend as being convenient for this purpose, is a stout tube having a bore of half an inch in diameter, and capable of holding from two to three cubic inches. A tube having this bore, and about fourteen inches in length, will hold two-and-a-half cubic inches, which will be quite large enough. Each cubic inch of it should be divided into tenths and hundredths of a part of a cubic inch. It is scarcely necessary to remark, that in cases where great accuracy is required, due attention must be paid to the temperature and atmospheric pressure, and certain corrections made if these should deviate from the usual standards at the time of reading off the volume of the gas; but in most cases, sufficiently near approximations to accuracy may be obtained without reference to these particulars.

From a number of experiments I have ascertained that the quantity of gas evolved from different amounts of urea (treated in the way I have described) very closely approximates to the quantity of nitrogen gas which should be furnished from the urea by calculation.

This will be seen from the following experiments taken from many:—

The fifth part of a grain of urea should furnish by calculation, 0.3098 parts of a cubic inch of nitrogen gas, at 60° Fahr. and 30′ Bar.; the same quantity of urea, treated as described, furnished in one experiment, 0.3001, and in another, 0.3069 parts of a cubic inch of gas, at the same temperature and pressure, which shows that the calculated quantity of nitrogen differs from the amount of nitrogen gas obtained by only a few thousandths of a part of a cubic inch. I may observe, that I was obliged to operate on such small quantities of urea, on account of the graduated tube I had at the time being only of one cubic inch capacity. Seeing, then, that the quantity of gas evolved agrees so very closely with the calculated amount of nitrogen present in a certain quantity of urea, I take the calculated amount as being the more correct, and knowing the relation that exists between a certain quantity of urea and nitrogen, I can, from the quantity of gas evolved in any case, easily calculate the amount of urea present, by the simple process of rule of three.

Thus, the fifth part of a grain, or 0.2 of a grain of urea gives, by calculation, 9.3098 parts of a cubic inch of gas. Then, 0.3098 : the volume of gas found :: 0.2 : to the required quantity of urea; or, multiplying the first and third terms by five, we have 1-549 cubic inch of gas representing 1. grain of urea, which is a simpler proportion. Using this data, I made several comparative experiments on different samples of urine, with my method and that of Baron Liebig's, which I believe is considered one of the most accurate of the methods of determining urea in urine at present known.

The following are the results of three comparative experiments on different samples of urine, using the same with each method:—

	Amount of urea in grains and parts of a grain in one fluid ounce of urine.	
	Liebig's.	*New Method.*
First	3.680	3.712
Second	5.328	5.472
Third	4.976	4.976

In the first and second experiments the quantity of common salt present in the urine was taken into account, as it is found to increase to a slight extent the apparent quantity of urea in the urine by Liebig's method; in the third this was not taken into consideration, and the quantity of urea was compared with the mean of two results obtained by my method.

These experiments show how very closely the results obtained by the new method agree with those of Baron Liebig's, and therefore show the correctness of the former method. I prefer the hypochlorite of soda to that of potash as a decomposing agent, because the soda salt is an article of our pharmacopœia, under the name of

"Sodæ Chlorinatæ Liquor," and therefore can be so easily procured, whereas the potash salt, not being used in medicine, would require to be specially made for this purpose. As to the hypochlorite of lime, I do not think it so effectual, and it has the disadvantage of soiling the sides of the graduated tube by the carbonate of lime formed in the reaction. In reference to the quantity of hypochlorite of soda to be used, it should always be employed in excess, and I think that about five or six times the volume of urine employed would be found generally to be quite sufficient, and ensure there being an excess.

The amount to be used may be easily determined by direct experiment, by adding to a certain quantity of urine to be examined, in a small glass, a measured quantity of the hypochlorite, and leaving it for a short time till the evolution of gas is nearly over; then if, on the addition of more of the hypochlorite, the effervescence is renewed, it shows that there was not enough of the decomposing liquor first employed, and more must be added till there is no further evolution of gas produced, and the quantity of hypochlorite which is required to arrive at this point, indicates the amount necessary. I found by experiment that one grain of urea requires somewhere about half a fluid ounce of the ordinary sodæ chlorinatæ liquor for its complete decomposition.

The amount of mercury employed requires some little attention; it should, as a general rule, be never less than the volume of gas evolved, for if the volume of gas produced is more than that of the mercury employed, it will be more than that of the solution of salt, and therefore some of the mixture of urine and the hypochlorite will be forced out of the tube before it is completely decomposed, and consequently some of the gas will be lost; so that if this occurs we must repeat the experiment, using either a larger quantity of mercury, if our tube will allow, or diminishing the quantity of urine employed. It might be supposed on first sight, that this method would be liable to the following source of error, viz.:—that some of the gas would be evolved and lost during the pouring in of the hypochlorite; but this is not the case, as several seconds elapse before there is any apparent reaction or evolution of gas on mixing the hypochlorite with the urine, and therefore giving full time to perform the experiment without loss of any gas.

I have, also, ascertained that the ordinary acid reaction of the urine does not affect my method.

I should observe, that this new method, like all the others known, is not perfectly free from some slight sources of error; the principal one being, that ammonia, if it exists in the urine, gives rise to nitrogen, and therefore increases the apparent amount of urea. But the same objection holds equally in Liebig's and Ragsky's methods, which are, perhaps, the two most accurate at present known. Uric acid, also, is similarly affected by the hypochlorite, but it and ammonia ordinarily occur in such small proportion in urine, that the error produced would be but trifling, and is partly corrected by taking the calculated quantity of nitrogen, which is, as I have shown, something more than that derived from a certain quantity of urea by direct experiment.

In cases where ammonia or uric acid occurs in more than ordinary quantity, these substances must be separated by the usual means employed, before having recourse to my method.

I should think that gently heating the urine with a certain quantity of baryta water, as long as the odour of ammonia is disengaged, and then filtering the solution, as recommended by Liebig, for the separation of ammonia before applying his method (see the 'Journal of the Chemical Society,' vol. vi. p. 30), would effect the object very easily, and separate not only the ammonia, but, also, the greater part if not all of the uric acid present.

There is one other source of error which may arise, and which can easily be avoided; it is the following: that if a solution of hypochlorite of soda, alone, or standing over mercury, be exposed to the light for several days, it will very gradually evolve a very minute quantity of oxygen. So that the experiment in determining urea should not be carried on for too long a time, but a day, or even two, will scarcely make any appreciable effect on the quantity of gas evolved in testing for urea.

The reaction which appears to take place in the process seems to be the follow-

ing: the hypochlorite of soda reacting on the urea gives rise to nitrogen, water, carbonic acid, and chloride of sodium :—

$$\text{Thus, } \underset{\text{Urea.}}{C_2 H_4 N_2 O_2} + \underset{\text{3 Hypochlorite Soda.}}{3\,(ClO\cdot NaO)} = N_2 + 2\,CO_2 + 4\,HO + 3\,Cl.\,Na.$$

The nitrogen is evolved and the carbonic acid is absorbed by some of the hypochlorite of soda in excess, for I find that this salt absorbs carbonic acid very quickly, without evolving any other gas, and I have failed in several experiments to detect the smallest portion of carbonic acid gas in the gas produced by acting on urea, though I have always noticed the presence of a very minute quantity of oxygen in the nitrogen.

These appear from my experiments to be the changes produced; but this part of the subject I have not as yet minutely examined, and my experiments have hitherto been made on healthy urine; but I have ascertained that several of the substances found in urine during disease, as, for example, sugar, albumen, bile, and excess of urinary colouring matter, produce scarcely any effect on the results obtained by this new method of determining the quantity of urea in the urinary secretion.—*Dub. Hosp. Gazette, June* 1, 1854, *p.* 134.

SURGERY.

AFFECTIONS OF BONES AND JOINTS, AMPUTATIONS, &c.

45.—SYMPTOMS AND TREATMENT OF SOME PRINCIPAL DISEASES OF JOINTS.

By Wm. Coulson, Esq., Surgeon to St. Mary's Hospital.

[Mr. Coulson having described the general morbid anatomy of articular disease, proceeds to the symptoms and treatment. Amongst the occasional causes of joint disease, he mentions one which is not sufficiently attended to, and which is particularly pointed out by M. Tessier, of the Hôtel Dieu, of Lyons, viz., that prolonged immobility of a joint may not only aggravate existing disease, but produce various lesions in a joint which had been previously healthy.]

This fact was ascertained by examining the joints of persons who had been confined to bed in a fixed position for a great length of time for fracture. In such cases, M. Tessier very frequently observed the following morbid appearances:—In all the joints of the affected limb, the synovia was replaced by an effusion of bloody serum, and sometimes of fluid blood. The same effusion existed in the soft parts of the joints, and in the peri-articular tissues. There was passive congestion of the synovial membrane, most visible in the folds or fringes which it forms in some joints, or in the more advanced stages, actual formation of false membranes, injected with vessels adherent to the cartilages. These latter were often involved; their surfaces presented a partial or uniform injection, with more or less destruction of substance, which varied considerably in different cases. Sometimes the articular surface of the cartilage was merely rough, and had lost its polished appearance; in others it was absorbed superficially, in others again it was softened, deeply eroded, or evidently ulcerated, with bright injection of the cartilage around the ulcerated spots. These are examples of the well-known fact that mere suspension of a function for any considerable time may give rise to alterations of structure, and they lead to the practical inference that the immobility recommended in the treatment of diseases of the joints cannot be prolonged beyond a certain period without danger, for perfect rest alone during five or six months is sufficient to cause absorption or ulceration of the cartilages.

[Mr. Coulson adopts the ordinary classification of diseases of joints, viz., those attacking the *membranes*, the *cartilages*, and the *bones*. He then proceeds to say:]

Inflammation of the proper capsule of the joints is termed synovitis, and the synovial membrane may, like other tissues, be the seat of acute or chronic inflammation. In addition to this, the inflammation may be common or specific; it may present the ordinary characters of inflammation in analogous tissues, or it may be modified by the gouty, the rheumatic, and the scrofulous diathesis. Finally, it may be connected with gonorrhœa, or be one of the symptoms of the general and dangerous affection depending on purulent infection of the blood. Here, then, are no less than seven or eight varieties of the same disease in a single elementary tissue—viz., the synovial membrane.

Common inflammation of this tissue, as I have said, may be acute or chronic. The acute form is generally attended with well-marked symptoms which render its diagnosis easy. It occurs more frequently amongst adults than at any other period of life, and can often be traced to the influence of the gouty or rheumatic diathesis. Rheumatism, I believe, is the predisposing cause in a great majority of cases, and exposure to cold the exciting cause. The characteristic signs of acute synovitis are generally fever, severe pain, and uniform swelling of the joint, with a sense of fluctuation, occurring in such quick succession that they seem to be almost simultaneous. If very acute, the inflammation is ushered in by rigors and high inflammatory fever. There is, at the same time, more or less severe pain in the joint, which, if superficial,

feels hot, and is evidently enlarged. The pain will vary according to the severity of the attack; it soon becomes acute, even intolerable, when the capsule is much distended by fluid. Hence pressure on the joint excites great suffering, which is moreover aggravated by the slightest movement of the affected parts. The swelling arises from distension of the synovial membrane by fluid effused within its cavity. It often occurs within a few hours after the commencement of the inflammation, but its appearances may be modified by several circumstances. Thus the swelling cannot be perfectly distinguished, unless the joint be superficial, and the quantity of effused fluid considerable. In such cases the swelling assumes the form of the synovial membrane, and fluctuation can easily be detected. The natural structure of the joint and surrounding parts will, however, tend to alter the uniform appearance which the enlargement would otherwise assume; it has a tendency to manifest itself over those parts of the joint where less resistance is offered; but on this point I need not repeat what I have already said when speaking of abscess and the examination of joints during disease. The sense of fluctuation, generally distinct at first, may soon be rendered obscure by effusion of lymph into the cavity of the joint, thickening of the synovial membrane, or effusion into the peri-articular tissues with œdematous swelling of the more superficial parts. During the progress of the disease some symptoms worthy of notice may occur. The synovial bursæ in the neighbourhood of the affected joints may become implicated. This is often observed during acute inflammation of the shoulder-joint, and gives rise to a sense of crepitation. In some other cases the crepitation seems to depend on change of quality in the synovial fluid secreted. But the symptoms which require closest attention are those which indicate that the inflammatory action has given rise to secretion of pus within the joint. This is usually indicated by a well-marked aggravation of the inflammatory symptoms—severe rigors, high fever, increase of pain, throbbing, starting of the limb, &c. The swelling also increases, but it is impossible to distinguish that caused by purulent effusion from other forms by the sense of touch alone; we must infer the presence of suppuration from the constitutional symptoms just mentioned.

Acute synovitis may run the usual course of common inflammation in other parts of the body, and terminate in resolution. Some weeks generally elapse before this favourable termination takes place. The chance of its occurrence will be modified by the nature of the fluids effused into the joint. Serum is readily absorbed, but if coagulable lymph has been deposited in the synovial membrane and peri-articular tissues, the joint will remain stiff for a long time; the disease is liable to recur, and chronic inflammation with ultimate destruction of the joint may be the consequence. Secondary luxations of the femur and tibia have sometimes been observed during this stage of the complaint, in consequence of relaxation or destruction of the ligaments, together with muscular action, and an unfavourable position of the limb. If pus has been secreted the prognosis is still more unfavourable. Purulent collections, the result of acute synovitis, have little or no tendency to find their way into intermuscular tissue. The reason of this is, that acute articular inflammation is generally attended by an effusion of lymph in the cellular substance and other tissues around the joint, by which the pus is prevented from forming sinuses; it works its way directly from the joint to the surface, but the cartilages become softened and ulcerated. Anchylosis is the most favourable result that can be expected, but amputation often becomes necessary.

Chronic synovitis may be regarded as an effect or rather continuation of the acute disease just described, for it is highly probable that the low forms of inflammation which constitute primary chronic synovitis (hydrops articuli excepted) are connected with scrofula or some other constitutional affection. Under this view of the subject it is unnecessary to say much about chronic synovitis. The physical signs are the same as those already described, but diminished in degree. The joint is stiff and swollen; its motions are limited and painful; there is always more or less swelling, but this is firm, not attended by a sense of fluctuation, and never presenting the smooth, round appearance of the tumour in acute synovitis. There are little or no febrile symptoms, but every now and then an exacerbation is liable to take place, and the disease to assume more of the acute form.

We should have, however, a very imperfect and limited notion of chronic synovitis if we confined ourselves to the species just alluded to. Inflammation of the synovial membrane, assuming from the outset a chronic form, and depending on some

constitutional derangement, presents itself under very different characters, and is of a more serious nature.

Gouty and rheumatic inflammations of the joints are sometimes acute, often chronic. To describe them in a complete manner it would be necessary to pass in review the general history of the two diseases with which they are connected. I shall therefore omit them here, and pass to a description of chronic synovitis in scrofulous patients.

Considerable discussion has taken place between writers on diseases of the joints, relative to the particular tissue affected in scrofulous affections of these parts. Some, as Sir B. Brodie, locate the disease in the cancellous tissue of the ends of the bones; others are of opinion that it begins in, and is for a long time confined to, the synovial membrane. From my own experience, I am inclined to think that scrofulous disease may commence in any one of the elementary tissues composing a joint; that it may remain confined for some time to the tissue in which it originated, but that, generally speaking, it has a tendency to pass in a gradual manner from one tissue to another until the whole joint, membranes, bones, and cartilages become involved. Scrofulous synovitis is sometimes described under the name of gelatinous degeneration of the synovial membrane. It is of frequent occurrence in some joints —the knee, for example, where it assumes a slow and very insidious form from the commencement. In the earliest stage the appearance of the membrane differs little from that of one attacked by a slight degree of common inflammation; it is partially injected and somewhat thickened at the inflamed points; soon afterwards it begins to soften from the deposit of scrofulous lymph, and is then gradually converted, as the disease progresses, into a pulpy, gelatinous layer of a pale-yellow colour, somewhat resembling that of infiltrated tubercle. According to some authors, the colour is often whitish or light grey; it is certainly more or less mixed with red in many cases, and it is probable that the appearances and consistency of the membrane will depend on the degree of organization which may have taken place in the imperfect lymph effused on the synovial membrane and into its sub-synovial tissue.

The symptoms of chronic scrofulous synovitis are at first sight obscure, for the disease generally commences in a very insidious manner. It attacks persons under the age of puberty more frequently than adults, and is in a great majority of cases accompanied by the general signs of the scrofulous diathesis. Some slight impediment to the free motion of the joint is often the first symptom observed, and this may continue for weeks or even months, occasionally accompanied by uneasiness or slight pain in the joint before the next symptom—viz., swelling—attracts any attention. This is always indolent, and progresses in a gradual manner. It does not appear to depend on any considerable accumulation of fluid in the joint, for it is not accompanied by any sense of true fluctuation; it has a doughy, elastic feel, and at first assumes the form and limits of the synovial membrane, giving a rounded appearance to the knee-joint, where the progress of the swelling can be observed with facility. When the synovial membrane has undergone pulpy degeneration, and any quantity of the imperfect plastic matter peculiar to scrofula has been effused into the joint, the tumour projects on either side of the articulation, and has a soft doughy feel, which may be mistaken for fluctuation. The skin over the swelling is pale, not much distended, and preserves its natural temperature. It is only at a later stage, when abscess has formed within the joints, that the integuments become implicated. The patient is free from general disturbance beyond what may be attributed to his constitutional state; there is no symptom of fever in a word; the local inflammation is betrayed by no other symptoms than obscure pain and gradual swelling. Scrofulous synovitis, however, has a great tendency to pass into suppuration, or more frequently still into the formation of those soft imperfect masses which some call "scrofulous false membranes," &c., and which compose a great portion of the tumours denominated white swellings.

In a few cases the inflammation assumes a more active character, and some coagulable lymph is effused on the surface of the membrane, but even then we do not find adhesion taking place between the opposite surfaces. After the disease has continued for an uncertain period, some attempt at reaction may be set up, and suppuration ensue, or pus may be effused into the cavity of the joint with very slight aggravation of the general symptoms. In other cases ulceration of the

cartilages takes place, either under the influence of the altered membrane, or of the suppurative inflammation, and the distinctive characters of the disease become merged in those of a general affection of the joint.—*Lancet, April* 29, 1854, *p.* 465.

[In inflammation of joints, the particular tissue involved can only be known in the superficial joints. When in the synovial membrane the swelling occurs very early, and is more considerable than when in the extremity of the bone. Ulceration of the cartilages and suppuration take place much sooner when the synovial membrane is affected than when the disease has commenced in the bone. Gonorrhœal rheumatism often rather confounds our diagnosis.]

The indications of treatment, in inflammation of the synovial membrane, will be chiefly regulated by the acute or chronic nature of the disease, and by a consideration of the constitutional influence under which it may have been developed. The synovial is a delicate membrane, its functions are quickly impaired by disease, and its close connexion with the cartilaginous portions of the joint favours the extension of inflammatory action from the membranous to the solid tissues of the articulation; hence acute inflammation of a common kind must be combated from the outset with active and decided measures. If the patient be strong, and the general inflammatory symptoms run high, blood should be taken from the system. Copious venesection at an early stage will have a beneficial effect in checking the tendency to the effusion of organizable lymph, on the presence of which many of the secondary changes depend. In less severe cases, the local abstraction of the blood by leeches or cupping, in the vicinity of the affected joint, will suffice. This must be repeated according to the circumstances of each case, which will also regulate the number of leeches to be applied; but, as a general rule, it may be laid down that the number should be considerable. The leeches are commonly applied immediately over the affected joint; the severity of the pain may render it necessary to apply the cupping-glasses rather above the inflamed articulation than on it. Warm fomentations should be frequently employed; they relieve distension and assuage pain, being usually composed of a decoction of poppy-heads. When, however, the pain is excessive, I can recommend a remedy, the use of which is attended by no danger, while its effects are really striking. This is the chlorinated hydrochloric ether, a compound somewhat resembling chloroform. A piece of lint, moistened with twenty to forty drops of this ether, may be placed on the joint, and its influence in dissipating pain is scarcely less than that of chloroform itself. I have seen cases in which the severest pains of acute rheumatism have been alleviated by it in a few minutes.

After sufficient extraction of blood, the means most to be relied on, is the administration of calomel and opium, so as to bring the system rapidly under the influence of mercury. This method of treatment has been adopted in consequence of the well-known influence which mercury exercises on serous inflammations, and when properly applied in cases of healthy synovitis it is highly efficacious. Still you must be perfectly satisfied that the inflammation is not developed under the influence of any diathesis, and, above all, is unconnected with a scrofulous habit. In such cases the general antiphlogistic treatment must be modified according to the constitutional disposition of the patient. Should any doubt exist as to the propriety of employing mercury, tartarized antimony may be had recourse to; but generally speaking the acute inflammatory symptoms will be sufficiently subdued by copious local bleeding, fomentations, and saline antimonial remedies; in a word, by the ordinary antiphlogistic treatment, with the principles of which you are all familiar.

Absolute repose of the affected joint is an indispensable condition of treatment, without which the most active remedies will fail to produce a beneficial effect. You must therefore pay early attention to this point, endeavouring to place the articulation in the most favourable position, and there maintain it as immovably as possible. This will require experience, practical skill, and a perfect acquaintance, not only with the anatomy of the joint, but with the effect produced by various positions of the pelvis, and by the action of muscles attached near the articular extremities of the bones. Hence it is evident that each joint will require a particular position, and special artificial means for securing it in a state of repose. I cannot, however, now lay down anything beyond a few general rules. It is commonly

said that the patient instinctively assumes the most favourable position for the limb. This is an error. Instinct takes nothing into consideration beyond pain, but the surgeon has to look further, and regard the possible terminations of the disease, together with the influence which different positions of the articular extremities of the bones may exercise on the inflamed tissues within the joint. If you examine the positions naturally assumed by patients labouring under articular disease, you will invariably find that they have a tendency to keep the inflamed joint in a flexed position. This may be the easiest one, but in the knee-joint, for example, it is evident that strong flexion promotes in a remarkable manner the tendency of the tibia to be luxated backwards.

A good position in articular disease should comprehend several circumstances. It should not give rise to distension of any part of the capsule; it should not cause one part of the articular surface to press against another; it should not favour the occurrence of secondary luxation; and, finally, it should be one most likely to render the limb useful, in case of anchylosis. Attention to these several points will enable you to select the best position for the limb in each case, after which various mechanical means may be employed to ensure complete repose. During the early stages of acute synovitis the pain is so severe that no artificial restraint can be tolerated. You must therefore gently place the limb in what you consider to be a favourable position, and endeavour to keep it motionless by securing it on pillows carefully arranged.

With rest and antiphlogistic remedies the violence of the disease will soon be subdued; it becomes more or less chronic, and our main object is now directed towards promoting the absorption of any extraneous matters which may have been effused into the cavity of the joint. In fulfilling this indication great care must be taken not to induce a recurrence of the inflammatory action by too hasty efforts to dissipate its effects. The most useful remedies are gentle friction, moderate and careful compression, stimulating applications, and blisters. During this period of the treatment perfect immobility of the affected joint may be ensured by the employment of mechanical means. Almost every surgeon has his favourite method. Some trust to pillows and bandages; but these are never sufficient to ensure perfect repose, and should therefore be rejected; others employ the common fracture-splints; others, again, recommend special apparatus manufactured by our instrument-makers. In general practice you cannot always procure these; but you can always get some pieces of thick leather: soften these in warm water, mould them when pliable on the limb, remove them when hard, line them with wash-leather, and you will have a very serviceable splint, which, with proper bandages, will keep the joint immovable. The use of the splints may be continued as long as you have reason to think that any trace of inflammatory action remains in the joint, but no longer. After five or six weeks it would be dangerous to employ them. I have already pointed out to you the changes which result from prolonged immobility of a joint, and hence gentle motion must be resumed as soon as the articulation appears in a condition to bear it. At first this may be effected by the hands of the surgeon, who will combine it with friction and what has been called shampooing; afterwards the patient will be encouraged to make such gentle efforts as he can bear without pain. Especial attention must be given during this part of the treatment; for, as I have observed before, injudicious and hasty attempts to accelerate the cure may have an opposite effect, and cause a return of the complaint.

Notwithstanding all our efforts, acute synovitis will sometimes terminate in suppuration. The symptoms of this state have been described, and, I may add, that it is generally much more easy to recognise the occurrence of acute articular abscess than of those chronic collections which take place in unhealthy subjects, and are accompanied by considerable tumefaction or thickening of the peri-articular tissues. Should any doubt exist, a preliminary exploration may be made with a grooved needle; and if it be found that the fluid contained in the joint is pus, the question immediately arises—should we leave the case to nature, or should we evacuate the contents of the joint by incision? Surgeons are not agreed upon this point, and it must be confessed that much may be said upon both sides of the question. For my own part, I am in favour of a free and early evacuation of the purulent collection, after which the treatment must be continued according to the general principles of surgery.

Chronic synovitis, as I have said, may be of a simple kind, or it may be connected with some constitutional peculiarity or diathesis. The treatment must be modified accordingly. Simple chronic inflammation of the synovial membrane is generally a continuation or sequence of some acute attack, and is to be combated by the usual antiphlogistic remedies, due attention being paid to the stage of the disease and the condition of the patient. The previous treatment will probably have dissipated any prominent inflammatory symptoms, and the local abstraction of blood is seldom indicated, unless the recurrence of pain, swelling, &c., shows that an exacerbation of the chronic state has taken place. In such case we must have recourse to moderate local depletion. The main object, however, is to promote the absorption of such materials as may have been deposited in the tissues of the joint during the stage of active inflammation, while at the same time we disperse any remnant of inflammatory congestion that may be lurking in the parts. The persevering use of counter-irritation is the means on which our chief reliance is to be placed. This is a point on which all surgeons are agreed, and it is also admitted that blisters are the best form in which the counter-irritation can be employed in the first instance. The blister may be of such a size as to cover the whole surface of a superficial joint, but many prefer applying it close to rather than immediately on the affected articulation. By healing the blister immediately, and employing a second or third one, if judged necessary, we obtain a much better effect than if the blister were kept open by an irritating ointment.

The blisters should be followed by the use of stimulating applications to the surface of the joint. These are of various kinds. Some surgeons recommend us to paint the joint over with tincture of iodine, but it has seemed to me that this application rather exercises a peculiar influence on the tissue to which it is immediately applied, than a remote action as a counter-irritant. Frictions with tartar emetic ointment have also been recommended. This remedy, however, is not very manageable; it sometimes produces troublesome sores; and for pustular stimulation I would prefer the croton-oil embrocation, with which any desired amount of counter-irritation can be produced. It is almost unnecessary to repeat that, during the whole of this time, the strictest repose must be ensured by the means already mentioned.

When the employment of local bleeding, followed by counter-irritation, and seconded by absolute rest of the limb, has had the effect of completely subduing inflammatory action, we have recourse to another order of remedies. The object now is to stimulate the absorbent vessels, and promote the removal of any adventitious material which may have been deposited in the tissues of the joint or around it. Various means are employed for this purpose. When the swelling is not very firm, and we have reason to conclude that the effused products of inflammation have not been considerably organized, common stimulant embrocations, or a discutient plaster, composed of equal parts of galbanum and mercurial plaster, may be used with advantage. In more advanced cases, pressure either by means of a bandage or strips of adhesive plaster may prove useful, but the most efficacious means, I believe, of restoring to the joint the greatest degree of motion of which it is capable, are methodical friction and cold douche. The friction should be exercised with the hand over the whole surface of the joint, which is at the same time to be cautiously moved and pressed in different directions somewhat after the manner employed in shampooing. This may be repeated for half an hour daily, or at certain intervals according to circumstances.

During the use of local remedies, your attention will not be diverted from the general condition of the patient, which must be carefully studied, and any indications arising from it promptly fulfilled. This is in accordance with the remark which I have already made, that articular diseases are very often dependent on some general derangement of the health, or as Mr. Abernethy would have expressed it, that the local complaint has a constitutional origin. Each constitutional taint, whether it be syphilitic, rheumatic, scrofulous, &c., will require its peculiar general treatment to be directed according to the principles established by medical experience.

But as I have only described in the present lecture, gonorrhœal and scrofulous synovitis, I shall confine what I have to say on this point, to these two varieties of synovial inflammation. Gonorrhœal rheumatism is a very peculiar disease, its precise nature is not yet clearly established, nor have we yet arrived at any very satis

factory method of treating it, for all surgeons who have had the opportunity of observing this form of synovitis, are agreed that it is remarkably intractable. The local inflammatory symptoms must of course be met by appropriate antiphlogistic treatment, but the constitutional remedies which should be employed, have not been so clearly determined. Sir Benjamin Brodie says that in the great majority of cases the disease must run its course, and that all that can be done, either by the surgeon or the patient, is to guard against the operation of those causes which might tend to aggravate or renew the symptoms. He has found colchicum combined with saline medicines, in the early stage, and the iodide of potassium in more advanced stages, the remedies from which the greatest benefit is derived. Some surgeons advocate the use of anti-gonorrhœal remedies after leeching, &c., and, even give large doses of copaiba, while M. Velpeau and others prefer a mixture of cubebs and copaiba. A few rapid cures have been obtained by this treatment, but generally speaking, it does not seem to have been very successful. Many of the German surgeons, influenced by the old theory of the identity between gonorrhœal and syphilitic poisons, employ Zittman's decoction, which is a solution of bichloride of mercury in decoction of sarsaparilla.

In the treatment of chronic scrofulous synovitis, great judgment and perseverance are required. The local remedies are the same as those already recommended for ordinary cases. The abstraction of blood by leeches, &c. is not often required, and its effect on the general health must be carefully watched. When the inflammatory symptoms have been completely subdued, recourse will be had to counter-irritants, friction, and the combination of pressure with moderate irritation, popularly known under the term of "Scott's dressing." From the nature of the disease, and the constitutional peculiarities of those affected by it, there is always some danger of the joint being more or less injured by organic changes; hence great attention should be paid to the position in which it is placed during the period of absolute repose, and if you have reason to fear that anchylosis must ultimately ensue, you will take special care to give the limb such a position as shall render it serviceable after the recovery of the patient. In all scrofulous affections of the joints, local remedies would produce little effect, without constitutional treatment.—*Lancet, May* 6, 1854, *p.* 487.

In some cases, the whole or certain portions of a cartilage are hypertrophied, without any obvious change in its texture. This is a rare affection; it seldom exists alone, but is generally connected with, and in all probability dependent on another change—namely, softening of the cartilaginous tissue. When this occurs, the texture of the cartilage is pulpy, the cells are enlarged or have burst, and the matrix is of a dark colour, containing a number of cells or nuclei.

In old people, especially in those who have led laborious lives, or been subject to rheumatism, we sometimes find an opposite condition of the cartilage. It is atrophied, or more or less removed by interstitial absorption, without breach of surface. The cartilage may be removed in patches, or the change may involve its whole surface, gradually thinning it until the entire cartilaginous end of the bone has disappeared.

Ulceration of the cartilage is, however, the disease which most interests us as surgeons, and to this frequent form of articular affection I shall now direct your attention. When disease, no matter of what nature, has continued for any length of time in a joint, the cartilages are extremely liable to become involved, but the affection of which I now speak, is a primary one, commencing in the tissue of the cartilage itself, or at all events commencing at a very early period of co-existing diseases, and for some time confined to the tissue in which it has had its origin. Primary ulceration of cartilage may be acute or chronic; it may also be common or specific, that is to say, connected with some constitutional taint, as scrofula. The appearances of the ulcerated parts are extremely various, and this may be explained if we admit, what is probably the fact, that ulceration of cartilaginous textures is not an ulcerative process in the true sense of the word, but a destruction, and disintegration of the tissues of a peculiar kind, and mainly depending on altered states of nutrition. However this may be, the ulcerated surface—for I must continue to employ the generally-received term—is sometimes a limited depression, giving the appearance as if a piece of the cartilage had been scooped out with some sharp instrument. In other cases we have small, round, and irregular excavated spots of

ulceration, either on the surface, or in the substance of the cartilage; while in still more severe forms, the whole cartilage seems to be rapidly removed without any further changes than those described at the commencement of the lecture.

Primary ulceration of the cartilage most frequently commences on the free surface; it may also begin in the substance of the cartilage, or according to Sir B. Brodie, on the surface by which the cartilage is in contact with the bone, but it may admit of doubt, whether in these last cases, the ulceration is not sometimes a secondary effect of some disease in the bone itself. Acute ulceration of the articular cartilages is a formidable disease. It is difficult to say how far it may be a primary affection, for it is always attended by synovial inflammation, or disease of the bones and soft parts. The most rapid forms are those seen to accompany purulent effusions into the joints from infection of the blood, when the whole of the cartilaginous surfaces of a large joint are sometimes destroyed in three or four days.

Simple chronic destruction of the articular cartilage is the form under which this affection usually presents itself to the surgeon. According to Sir B. Brodie, it occurs more frequently at the middle and later periods of life than in children. Its causes are obscure. Sir B. Brodie, having observed that primary ulceration of the cartilages is often preceded by pains like those of rheumatism in other parts of the body, is inclined to connect it with the rheumatic diathesis. It is sometimes connected with the scrofulous diathesis, and it may possibly be excited in some cases by mechanical injury. Numerous joints are sometimes affected in the same individual, while at other times the disease may be confined to a single joint, or even to a particular portion of a single cartilage. It is a curious fact, but one well established by numerous post-mortem examinations, that ulcerative destruction, both on the surface and in the centre of cartilage, may have been going on for a considerable time, and to a great extent, without giving rise to any symptom whatever. I am, indeed, inclined to think that the symptoms of primary destruction of cartilage are generally obscure, and that many of those which we are in the habit of attributing to it rather depend on disease of some other tissue, commencing at the same time with, or very soon after, the mischief in the cartilage.

Pain is one of the earliest symptoms which announce ulceration of cartilage. At first, it is of a dull kind, and deep seated, giving rise to a disinclination to motion; and this dull, deep pain may continue for weeks without any other change, the joint retaining its natural shape, and no swelling or effusion being perceptible. This presence of pain, unaccompanied by any signs of change in the joint, has been considered as diagnostic of primary ulceration of the cartilages; but I should not be disposed to place much reliance on these two points, inasmuch as I have often observed their coincidence in cases where the disease was confined to the osseous tissue of the heads of bones. As the disease advances, the pain becomes more acute; at times intolerable; worse at night; commonly referred by the patient to a particular spot, and often compared to the sensation as if an animal were gnawing the part. Although the pain is referred principally to the affected joint, yet it is often accompanied by sympathetic pain in some distant part, which may be so severe as to mask the original affection. Thus, when the hip is the primary seat of the disease, the most prominent symptom may be pain in the knee; again, when the knee is attacked, the pain often extends along the leg; and disease of the elbow-joint may give rise to more uneasiness in the forearm and wrist, than in the affected joint itself. In all cases, whenever the pain is at all severe, it is aggravated by any motion of the joint, and particularly by pressing the articular surfaces against each other. The patient, indeed, seems to avoid motion more carefully than the amount of pain would seem to account for, and hence the muscles connected with the joint are often more or less wasted at an early period of the disease. The wasting of the glutei muscles is a well-known symptom of hip-disease, and the same atrophy from forced repose is observed in the deltoid, in cases of ulceration involving the cartilages of the shoulder-joint. Unless the synovial membrane become accidentally implicated, there is never, as I have told you, any swelling about the joint at an early period of the disease. Many weeks may elapse before any trace of swelling can be discovered; and when this does occur, it is at first peri-articular, being produced by effusion of serum into the cellular tissue outside the joint.

So long as the disease is confined to the articular cartilages, we have few symptoms beyond those which I have just enumerated—namely, severe pain augmented

on pressure, disinclination for motion of the joint, sympathetic pains in distant parts, atrophy of muscles, and some peri-articular effusion, which steadily follows the progress of the disease. These symptoms may continue, with little change, for a considerable time; but unless the disease be arrested, it sooner or later involves the synovial membrane and bones, inflammation sets in, pus is secreted, and the symptoms of the primary affection become confounded with those of general destruction of the joint.

The terminations of chronic ulceration of the cartilages are various. As this disease has no tendency to the formation of pus, unless other tissues become involved, it may terminate favourably at any period prior to the occurrence of synovitis, or the exposure of the bone; but where pus is effused into the cavity of the joint, the ligamentous tissues destroyed, the articular surfaces extensively injured, and dislocation of the bone has perhaps taken place, there can be no hope of saving the limb.

Ulceration of cartilage may be confounded with synovitis, or with scrofulous disease of the heads of bones. From acute inflammation of the synovial membrane it can always be distinguished, if seen at an early stage, the former disease being always marked by rapid effusion of fluid, which takes place immediately after the onset of pain, and produces an uniform swelling of the interior of the joint, whereas in disease of the cartilage the swelling is slow in its formation, external to the joint in its seat, and never makes its appearance until a considerable period has elapsed from the first sensations of uneasiness and pain.

It will require somewhat more care to distinguish chronic degeneration of the synovial membrane from chronic ulceration of cartilage. The former is much more indolent, and although the swelling of the joint is slow in formation, it commences at a much earlier period than that which accompanies destruction of the cartilage. The sensation which the swelling communicates to the hand on pressure is also different; it is doughy, elastic, and superficial, not accompanied with obscure fluctuation, as we find in ulceration.

According to Sir Benjamin Brodie, ulceration of cartilage is more likely to be confounded with scrofulous disease of the heads of bones than with any other articular affection; but he thinks that attention to the following points will generally enable us to arrive at a satisfactory diagnosis. Ulceration belongs chiefly to the middle and subsequent periods of life, whereas scrofulous disease occurs in children, or in those little advanced beyond the age of puberty. Ulceration of the cartilages, again, commonly attacks dyspeptic persons free from any constitutional taint, or those who have been subject to rheumatism, while the scrofulous articular affection is almost always attended by the general signs of scrofulous diathesis. Besides this, the pain in scrofulous disease of the bones is slight at first, and does not appear to be developed until suppuration has set in; whereas, in ulceration there is always severe pain at an early period, and the disease may continue for a great length of time without causing any effusion of pus into the cavity of the joint. Ulceration of the cartilages may, however, manifest itself as a primary disease in scrofulous persons, and if, as Sir Benjamin Brodie affirms, the ulceration may commence on that side of the cartilage which is next to the bone, it is difficult to discover any assemblage of symptoms by which the one disease can be distinguished from the other. In a practical point of view it should never be forgotten, that joints, especially the large ones, are of complex structure; that the different tissues composing them are intimately united to each other, and concur in the performance of a common function; hence, disease commencing in any single tissue, is often quickly propagated to the neighbouring structures, thus soon losing its peculiar characters, and assuming those of general lesion of the joint.

In the treatment of ulceration of cartilage great discrimination on the part of the surgeon is required, and the means employed must vary according to the views which he may have formed of the nature of the disease, whether it be simple or scrofulous, whether it be primary or secondary, whether it be confined to the cartilage or have extended to other tissues and given rise to effusion of pus within the cavity of the joint. In this, as in most other articular diseases, the surgeon's first care will be to ensure perfect repose of the joint. If this be necessary in cases of synovial inflammation, you can readily understand how it is still more expedient to keep the joint perfectly motionless when its cartilaginous surfaces are the seat of disease, and

undergoing a process of destruction to which slight accidental circumstances may give a highly acute character, and render the preservation of the limb hopeless. All friction, then, between the cartilaginous surfaces must be avoided as completely as possible, and this object is attained either by Mr. Scott's method, or by the use of splints and bandages in the manner which I have already described. These are to be continued until all pain has disappeared, and until the subsidence of swelling shows that the active stage of the disease has been completely mastered.

At an early period of primary ulceration it does not seem probable that local abstraction of blood will be attended with much benefit. We have no proof that this form of the complaint is of an inflammatory nature, to be subdued by blood-letting, and experience confirms this view. On the other hand, when ulcerative destruction of the cartilage is secondary, depending on inflammatory action in the neighbouring soft tissues, abstraction of blood is manifestly indicated, and must be repeated according to the intensity of the inflammation with which the ulcerative process is secondarily connected. The same remark applies to cases which are undergoing treatment, and in which, without any assignable cause, or perhaps from the premature use of the joint, increase of pain and swelling indicate the accession of an inflammatory state. Here, however, we must be careful to ascertain that the swelling of the joint does not depend on the effusion of purulent matter within its cavity, for abstraction of blood under such circumstances would not only be useless, but positively injurious, by enfeebling the patient and rendering him less capable of sustaining the confinement which he must unavoidably undergo. At the early period of ulceration of cartilage mercury should be administered in all cases where the general state of the patient does not contraindicate the use of this powerful remedy. In my own practice I am in the habit of giving calomel and opium until the mouth is affected, and I find that if the mercurial action can be borne, and is early set up, it generally arrests the complaint. Some patients, however, are unable to bear the administration of mercury, while in others the disease returns the moment the mercurial action is discontinued. For the latter cases we may try an alterative course of Plummer's pill with sarsaparilla. It seems almost unnecessary to add that for the scrofulous form of ulceration mercury should never be employed. It should be replaced in such cases by cod-liver oil, and other remedies which I shall more fully notice in a lecture on scrofulous affections of the joints. In those cases unconnected with scrofula, where, from the general state of the patient, or from some peculiar idiosyncrasy, mercury cannnot be borne under any form, I would recommend you to try the effects of colchicum, combined with an alkaline aperient. When the inflammatory action in the joint has been subdued, our next reliance is to be placed in counter-irritation. Various methods have been had recourse to. Some surgeons place most reliance on blisters, which are either applied in quick succession, or kept open by means of savine ointment; others, again, insist on the necessity of our having recourse to a more severe and permanent mode of counter-irritation, and establish issues with caustic potass; lastly, we are advised by some writers to employ moxæ or the actual cautery. To me it seems that in the recommendation of these various forms of counter-irritation sufficient care has not been taken to discriminate the different stages of the articular disease. When this is far advanced, permanent counter-irritation by means of the seton may be useful, but then the disease has ceased to be ulceration of cartilage; at all events it has involved other parts of the joint. But in the earlier stages, when we have reason to believe that the ulceration is still confined to the tissue in which it originated, I am inclined to prefer placing a blister over the affected joint, and, if necessary, employing several blisters in succession. I have always adopted this plan in preference to keeping the blister open. I do not mean to deny the utility of setons or issues at certain stages of this disease; what I wish to convey is, that, according to my own experience, they should be reserved for the second or more advanced periods of the complaint, when the development of certain symptoms shows that it has not been arrested in the cartilage, but is extending to the neighbouring structures and threatening more or less extensive destruction of the joint. When this occurs, the treatment peculiar to ulceration of the cartilage ceases, and must be replaced by measures varying according to the different indications of the case. Amputation of the limb may be the only resource left, or it may be probable that anchylosis is taking place, when the precautions which I have already described to you are to be

insisted on. Should abscess have formed in the cavity of the joint its treatment must be conducted on the same general principles as those which guide us in the treatment of this occurrence under other circumstances. The matter should be freely evacuated, and the limb retained in a proper position so long as there appears any chance of obtaining an imperfect cure through anchylosis; but it must be confessed that extensive destruction of cartilage, followed by the changes which always attend this form of articular disease, often entails, sooner or later, the loss of the limb.—*Lancet, June* 3, 1854, *p.* 589.

[Mr. Coulson, with most other surgeons, places more reliance on constitutional than on local treatment in the diseases of joints. In the following remarks we find some good observations on the subject.]

Scrofulous disease of the heads of bones—and the same may be said of this affection without reference to its peculiar seat in the bone—never, I believe, admits of radical cure, except in the very earliest stages when it is limited to a small extent of tissue, consists in mere expansion of the cancelli with hyperæmia, and manifests itself by some tumefaction of the head of the bone with, perhaps, increase of its temperature. These symptoms, together with pain, indicate a commencement of inflammatory action, and the young practitioner might be disposed to have recourse at once to active antiphlogistic measures. Experience, however, demonstrates that local bloodletting is scarcely ever admissible, and on no account to be accepted as a general rule of treatment. In some cases where the constitutional signs of scrofula are equivocal, and the symptoms are severe at the beginning, a single application of leeches to the joint may be indicated.

This rule applies to scrofulous disease of the articular extremities of bone, but I would not extend it to certain cases of disease in the shafts of bones accompanied by periostitis; for this, though of a scrofulous nature, may manifest itself in so acute a form, as to require active controlling measures. Cataplasms or warm fomentations have no effect on the deep-seated inflammation, and can only afford some relief when an abscess is making its way to the surface, giving rise to pain and inflammation of the superficial tissues.

The propriety of having recourse to counter-irritation at an early stage of the disease, has given rise to much difference of opinion amongst the best authorities. Sir Benjamin Brodie says, "all kinds of counter-irritation, such as blisters, issues, setons, and tartarized antimonial ointment, are not only not useful, but actually mischievous." The only exception which he admits to this rule, is in cases where the disease, being situated in the hip-joint, is productive of more than an usual amount of pain. Here the use of several small blisters in succession, or the insertion of a seton in the groin, immediately in front of the hip-joint, afford much relief to the patient. Many of the continental surgeons, on the other hand, advocate the use of revulsive or discutient applications to the skin in scrofulous joint diseases. Those most commonly employed are the iodine ointments of Lugol, and the lunar caustic ointment of M. Jobert de Lamballe. When iodine ointment is employed, it should be strong enough to cause desquamation of the cuticle in a few days, and contain one part of iodine, two parts of ioduret of potassium, and about thirty of lard. The joint should be rubbed with this ointment twice a day. M. Jobert's ointment contains, according to its strength, four, eight, or twelve drachms of the nitrate of silver, to thirty drachms of lard. This remedy soon excites a pustular inflammation of the skin, and, according to M. Jobert, is one of the best means we can employ for combating the pain and tumefaction which attend white swellings of joints.

My own experience is against the use of counter-irritants, or discutient applications in the early stages of this affection; their efficacy is more apparent than real; they merely excite absorption of the fluids effused round the joint, and exercise no influence whatever on the progress of the disease which is going on within it; in fact, they simply act as does compression over a cancerous tumour, removing the innocuous products of inflammation, but leaving the original disease and the cause untouched. The great inconvenience attending the use of counter-irritant and revulsive measures is the danger of carrying the external irritation too far, and thus augmenting instead of diminishing the internal malady.

This objection does not apply to the use of douches and topical warm bathing, which have been found advantageous in many cases of scrofulous diseases affecting the smaller articulations. M. Lebert, who has had considerable experience in the

use of douches at the mineral baths of Lavey, regards them as best suited for cases of inflammation of the heads of bones, when the disease has lasted for some time, and when the surface of the bone remains intact, or the fistulous ulcerations in it are superficial. They are still more efficacious at a later period to promote absorption of the plastic materials, which produce stiffness of the joints. Besides, local applications of every kind interfere with a condition which is essentially necessary to a cure; I mean keeping the joint perfectly at rest, in such a position that the opposite surfaces of the articulation shall exercise the least pressure on each other.

I have already explained so fully the necessity of paying attention to these two points, that I shall not dwell on them here, but an additional remark or two in reference to scrofulous affections must be added. The structure of the hip-joint renders it impossible to keep that joint in a state of perfect repose, unless the patient be confined to the recumbent posture. Hence in scrofulous disease of the hip, you must place the patient on a bed or couch, and keep him there; but in all other cases I would content myself with securing the joint firmly in a proper position by means of splints, as I have already described, and allowing the patient to enjoy as much exercise in the open air as possible, taking due care that it is modified in such a manner as not to excite pain or uneasy sensations in the affected limb. It is almost unnecessary to remind you that free exposure to pure air exercises a more beneficial influence on the health of scrofulous persons than any medicine whatever, and experience has abundantly convinced me that any local effects of an injurious nature which may arise from exercise, (and, with proper care, we can generally avoid them,) are greatly counterbalanced by the genial influence of sun and air on the constitution of the patient.

The general treatment of scrofulous affections of the joints requires to be conducted with much discrimination, and pursued with untiring perseverance, for months, or even years, may pass over before a cure can be obtained. It is, however, a consolation to know that the disease is less dangerous than it appears to be, and that numerous patients, who formerly would have been condemned to undergo amputation of the limb, now recover under judicious treatment, with no other inconvenience than the deformity produced by anchylosis.

If the general condition of a patient labouring under scrofulous disease of the joints at an early stage be carefully examined, we shall often find some derangement of one of the grand secreting organs. The one most frequently in fault is the liver, and hence mild alteratives should be administered for a few days, care being taken at the same time to obviate a torpid condition of the colon, which frequently exists. Of medicines which act on the constitutional derangement which is supposed to characterize scrofula, the best are cod-liver oil, iodine, and iron, and of these cod-liver oil is unquestionably the most efficacious. The value of this remedy is also enhanced by the fact that it appears to exercise a more beneficial influence over scrofulous disease of the bones than any other,—a form of the malady which has hitherto been most inaccessible to the effects of remedies. It is important to keep in mind that this remedy, to prove efficacious, must be continued for a long time, perhaps for months, its use being interrupted from time to time, to prevent derangement of the digestive organs. The dose for a child is about a teaspoonful thrice a day; adults may take a tablespoonful. It is seldom advisable to go beyond an ounce a day, though many practitioners have carried the dose as far as three or four ounces. Cod-liver oil is not a specific for scrofula, and you must be prepared to meet with disappointments during its use; but a vast number of cases have established its efficacy, and I am inclined to believe that it possesses as much power over scrofulous affections of the bones as iodide of potassium does over secondary syphilitic diseases of the same organs.

The preparations of iodine have likewise an anti-scrofulous effect. They may be administered according to any of the well-known formulæ, or alternated with the cod-liver oil. Their efficacy is inferior to that of the oil, and, unless continued for a great length of time, they seldom produce anything beyond a temporary alleviation of the symptoms; still, experience has proved that, when properly administered, and assisted in its action by other means, iodine has effected a cure in many cases of severe and long standing scrofulous affections of the bones.

When the patient exhibits that peculiar cachectic state which often accompanies scrofula in large towns, being feeble and emaciated, with dirty skin, and an anemic

appearance, the preparations of iron may be useful, and one of our great authorities on diseases of joints seems to prefer them even to iodine or the oil. He recommends for children the iron wine or the syrup of the citrate, and sometimes of the iodide of iron, for three or four weeks, when it is discontinued for eight or ten days, to be resumed again, and continued for a long period.

The functions of the skin are always deranged during the progress of scrofula, and hence perhaps the well-known efficacy of warm or tepid bathing, not only at the commencement, but in the more advanced stages of scrofulous joint-disease. Cold baths likewise produce excellent effects from their tonic properties. Neither hot nor cold bathing should be recommended indiscriminately at an early period of the disease. If the joint be painful, and any signs of active inflammation exist, it will be better to wait until the symptoms subside, when tepid baths, at a temperature of 90°, may be tried. If they agree, the patient may continue in the bath for a quarter of an hour at a time, and they may be repeated every second day for a month or six weeks. After this period, or if the treatment has not commenced until all signs of active inflammatory process have disappeared, tepid salt-water bathing should be employed, and after a short time the temperature of the bath must be cold. The use of cold sea-water baths must, of course, be restricted to the fine season, from May to the end of September.

When circumstances preclude the patient from removing to the sea-side, we must endeavour to imitate the sea-water bath in the best manner we can. The most simple way is to dissolve from three to six pounds of common salt in twenty-five to fifty gallons of water, and this bath is perhaps as efficacious as any other. The principal point to which you must attend is that the bath, whether natural or artificial, does not produce over-stimulation, and hence its effects should be carefully observed. Cold sea-bathing is undoubtedly more efficacious when convalescence has commenced than at any other time. At this period also the alkaline or sulphuretted mineral baths have been employed with great advantage, and there can be no doubt that iodine baths have also proved useful, though it is difficult to separate the action of the bath, in such cases, from that of the remedy which is rapidly absorbed by the skin.

I have already said, that as a general rule, I would not advise you to be lavish of revulsive or stimulant applications in the treatment of scrofulous diseases of the joints. Still, I would not say that they should be absolutely rejected. A great deal will depend on the form of the disease, and the general condition of the patient. When the disease is confined to the articular extremities of the heads of the bones, it would not appear that external applications produce any marked effect in arresting it, and when the patient labours under that form of scrofula which is attended with general debility, the use of setons, issues, or any revulsive, accompanied by a drain, is positively injurious. On the other hand, when the disease is seated in the synovial membrane or cartilages, has become manifestly chronic, and is attended with effusion into the peri-articular tissues or ligaments, giving rise to stiffness of the joint and indolent swelling, various local stimulants may be employed with advantage. Frictions with iodine ointment are, perhaps, the best that we can have recourse to, and great benefit has been obtained from the judicious employment of douches, whether simple or medicated, hot or cold, in cases of chronic scrofulous arthritis, whenever the effused matters are of a kind susceptible of being removed by absorption. Warm douches seem preferable to cold ones, or to the vapour douche, though in some cases this is a valuable auxiliary. The general and local means now recommended would, however, produce but a limited effect, unless aided by proper hygienic measures—a fact which you can readily understand by remembering that scrofulous disease of the joints can only be treated successfully through the constitution, for the local malady is but a manifestation of the general or constitutional cachexy.—*Lancet, July* 1, 1854, *p.* 685.

[Mr. Coulson describes diseases of joints as existing in three conditions, viz., in the synovial membrane, in the cartilages, and in the cancellous structure of the articular extremities of the bones. It is only in the early periods of disease, however, that these distinctions can be made. When the disease has existed for some time the same general appearance will exist, whatever be the structure diseased, and the great tendency is to the formation of matter in the joint.]

The formation of matter, during the course of articular disease, may take place

under two circumstances, which you must be careful to distinguish from each other. The abscess may depend on inflammatory action in the soft parts external to the joint, to which it is confined, having no communication with the articular cavity: this I would denominate peri-articular abscess; or the secretion of matter may be an effect of inflammation within the joint, the pus is effused in the cavity of the joint, and is either confined to that cavity, or has made its way into the soft parts by perforation of the capsule.

The treatment of abscess of joints involves many points of great practical importance, yet we must admit that it has not arrived at anything approaching perfection, if we reflect on the very different opinions entertained by the best surgeons in this and other countries. The principal questions on which practitioners are divided are, the propriety or expediency of opening abscesses connected with joints, and also the manner in which such opening should be made. Some prefer leaving the abscess entirely to nature; others recommend small and repeated punctures, aided by compression: while other surgeons, again, with whose opinions I agree, advocate free and early incisions. That this is the best method we can adopt for the treatment of peri-articular abscesses which do not communicate with the cavity of the joint, I entertain little doubt, and I am inclined to believe that it may be extended also with advantage to many cases of perforating abscesses of the articulations. It is a necessary condition of this practice, that the joint should be freely opened, for there is then less danger of that general inflammation of the cavity which surgeons so much dread.

The changes which take place in a joint during the existence of abscesses, and generally speaking in the latter stages of articular disease, are extremely various. During a certain time they will depend, in some measure, on the primary form of the disease, and on the tissue in which it has commenced. They may, as I have said, gradually proceed to complete destruction of the joint, or they may become arrested, a reparative process setting in, and a cure effected by anchylosis. Let us now consider this mode of termination. Anchylosis is the name applied to the greater or less immobility of the joint which succeeds disease, and depends on a fibrous or osseous union between the parts entering into its composition. It is, in a word, a mode of reparation like that resulting from the formation of a cicatrix or the deposit of callus; it may follow various lesions of the joint, and have its seat in different tissues; it may be external to the joint or internal; it may be entirely osseous or fibrous, or a mixture of both. All these differences will depend on the nature and extent of the previous disease, and probably on the age and constitution of the patient. After extensive destruction of the articular cartilages of a joint, the new osseous matter may be thrown out in such quantity as to produce complete adhesion between the opposite denuded surfaces; in other cases, one of the bones seems lodged in a kind of cavity formed in the other, or the osseous matter may proceed from the outer shell of the bone, forming a bridge or splint, and sometimes a complete circle, which locks the bones together in a kind of case. Soft anchylosis is produced by adhesions or bands of unyielding fibro-cellular tissue, which pass from one bone to another, or are deposited in the ligamentous structures external to the joint. Finally, we may have a false anchylosis, in which the bones are unchanged and the ligaments but little altered, the stiffness and immobility of the joint depending on fibrinous deposit exterior to the capsule, but chiefly on a rigid and contracted state of the muscles connected with the joint.

The diagnosis of anchylosis presents no difficulty; for it is easy to see that the joint is rigid or immoveable, but this teaches us nothing in a practical point of view. The surgeon's main object is to afford relief; and in order to direct his efforts in a rational manner, he must endeavour to ascertain, with some degree of precision, the nature of the lesions which impede the motions of the limb. It is evident that nothing but disappointment could be the result if we were to apply to a complete osseous anchylosis the treatment suited for immobility depending on fibrinous deposit or altered muscular action. The first question, then, which you will have to determine, is the degree of immobility caused by the anchylosis; and this point alone, if properly investigated, will throw much light on the whole nature of the case. The patient should be encouraged to move the limb; and, if quite unable to do so, the surgeon must make the experiment himself, care being taken that one of the bones is firmly fixed, while an effort is made to move the other with the hand. The

extent, however, to which the heads of the bones are moveable can seldom be ascertained without the assistance of mechanical means, in the use of which the utmost caution must be observed. The impediment to motion, as I have said, may depend on contraction of the muscles, on fibrinous deposits in the soft parts or bones, or osseous union; hence your attention will be directed to these three points. To ascertain how far the muscles are involved, you must test their condition with the fingers, while attempts are being made to move the joint. If they are not hard and contracted, and if their relaxation has no influence on the mobility of the joint, you may reasonably conclude that they offer no impediment to its motion. The slightest motion in the joint is sufficient to show the absence of osseous anchylosis; but we cannot safely reverse the proposition, and affirm that bony union exists because the joint is completely immovable. This latter condition may be the result of fibrinous anchylosis, combined with irregularities of the osseous surfaces, which permit one bone to become encased as it were within the other. Fibrinous anchylosis is an ordinary result of scrofulous disease, and we may be pretty certain that we have to deal with this form when the anchylosis has supervened after the healing of fistulous abscesses and subsidence of scrofulous tumefactions of the joint.

The treatment of anchylosis will vary according to its nature, whether osseous or fibrinous. In the former case, little or nothing can be done, for I cannot advise you to imitate the practice of M. Louvrier, nor has experience yet pronounced on the method proposed by Mr. Barton, an American surgeon, who made an artificial joint by dividing the shaft of the bone below the anchylosed joint. In cases of fibrinous anchylosis, our first object is manifestly to restore, as soon as possible, the natural motions of the joint; and it is consoling to know that a great deal may be done in this way by judicious management. As long as any trace of active disease exists within the joint, we must abstain from artificial motion; but when this has disappeared, gentle motion should be commenced, sometimes with the hand, sometimes with instruments, and the movements gradually prolonged, care being taken to interrupt them whenever they excite any pain. Stimulant frictions, shampooing, douches, and the vapour-bath, may also be employed with great advantage. When the limb has become anchylosed in a false position, special contrivances must be had recourse to for the purpose of bringing it back to an useful, if not normal direction. This also must be effected in a slow and cautious manner; and when we have obtained the object, I am of opinion that all has been effected which it is prudent to undertake. In many old cases of anchylosis, especially when the knee-joint is the seat of deformity, the flexor muscles are so rigid and contracted, that gradual extension by instrument either fails to straighten the limb, or must be abandoned from the excessive pain it produces. Here great benefit has been derived from division of the tendons of the retracted muscles and aponeurotic bands; and the great success which the late Professor Dieffenbach obtained by this method would certainly appear to claim for it more attention than it has received in this country.

The extensive destruction of cartilages, bony structures, and ligamentous tissues, which sometimes takes place during the progress of articular disease, may so far diminish the bonds of union between the opposite surfaces of a joint, that the bone escapes from the articular cavity, and secondary luxation is the consequence. I have already described to you the circumstances under which these luxations are produced, and the various kinds of displacement that may occur in each joint; I shall now, therefore, merely add a few remarks relative to their treatment. This is either preventive or curative; and I need hardly remind you that the former is of infinitely greater importance than the latter, from which little can be expected. During the treatment of chronic articular disease, you must always bear in mind the possibility of its giving rise to secondary displacement of the joint; and you must also reflect on the principal circumstances which favour such displacement, for it is by avoiding or removing the causes that we have the best chance of obviating their results. Now, when the connecting parts of a joint have been destroyed by disease to a certain extent, the principal causes which may subsequently produce secondary dislocations, are muscular contractions and faulty positions of the affected limb. Indeed, both these causes are generally combined in effecting the displacement, and the means proper for avoiding the one will be applicable to the other likewise.

I have already described what I conceive to be the best position for each articulation, and I shall now briefly allude to the general rule—viz., that in determining this you have to consider what position is least likely to cause undue pressure on the ligaments or capsule of the joint so as to favour separation of the articular surfaces from each other; and also what position is best calculated to ensure, in case of anchylosis, an useful direction of the extremity. It fortunately happens that, generally speaking, the same position which is the best in cases of anchylosis, is also the best calculated to obviate the effects of muscular contraction and the tendency to displacement. Thus, for example, when the lower extremity is kept nearly straight, with its axis in the same direction as that of the trunk, when any undue degree of rotation is avoided, and the foot sustained in such a manner as to form a right angle with the leg, then is the danger of secondary luxation diminished as much as possible, and if anchylosis should take place, the limb is in the most favourable position. When luxation has once taken place, it becomes a matter of serious consideration whether it admits of any relief from the hands of the surgeon. For my own part, I believe that in the great majority of cases, if large joints be implicated, little or nothing can be done to remedy the deformity. The elbow-joint is perhaps the only one to which attempts at reduction of secondary dislocations can be applied with much prospect of success. Here the head of the radius may be dislocated outwards and backwards, and no very great difficulty may be experienced in restoring it to its place, by bringing the arm to a middle state between pronation and supination, while firm pressure is at the same time exercised on the head of the bone. A proper apparatus must then be applied, and its use continued for a considerable time. When you reflect on the great destruction of the various parts of a joint which must have taken place before its articular surfaces can be separated from each other, on the diseased condition of the bones, and on the contracted state of the muscles or ligamentous fibres which exists whenever the dislocation is of any standing, you can easily perceive both the difficulty of effecting a reduction, and the probable inutility of such a proceeding, even if it can be carried into execution. In the knee-joint, for example, the resistance of the fibrous bands can seldom be overcome; and the irregularities of the bony surfaces, from extensive destruction, are such that little or no advantage would be gained by bringing them again into juxtaposition after displacement. Secondary luxation of the head of the femur usually takes place on the dorsum ilii; occasionally, also, into the obturator foramen. I shall presently, in speaking of excision, notice the changes by which such displacements are accompanied, and shall endeavour to appreciate their influence on any mode of treatment employed; for the present we may ask—Is it possible to reduce a secondary luxation on the dorsum ilii, and if so, is it advisable that such an attempt should be made under any circumstances? These are questions not easily decided in the actual state of our knowledge, for little has been done towards solving them, in this country at least, where the attention of surgeons has been exclusively directed to the subject of excision. I have, however, related in my work 'On the Hip Joint,' a very remarkable case, which seems to prove that even spontaneous reduction may take place. The head of the femur could be felt on the dorsum ilii, and the limb was shortened by about three inches. In order to relieve the pain excited by irregular muscular action, but without any idea of reducing the dislocation, counter-extension was employed uninterruptedly for four days, and then given up from the irritation it produced. Seven weeks afterwards, while the nurse was assisting the patient to turn in bed, she felt 'the bone rushing past her hand,' and on the following day it was discovered that the limb was restored to within half an inch of its natural length, presenting no appearance of inversion or eversion, and quite free from pain. This case seems to place the occurrence of spontaneous reduction beyond doubt; while the excellent work of M. Humbert proves that secondary luxations of the femur on the dorsum ilii may, under certain favourable conditions, be not only reduced, but permanently cured. In one of the cases related by M. Humbert, the limb was shortened to the extent of four inches and three-quarters; in another nearly two inches. To effect the reduction, a special apparatus somewhat similar to those employed in cases of congenital luxation, must be used, and the extension is to be effected in a gradual manner. The time required will depend on the readiness with which the displacement yields. I fear that the number of cases to which this method is applicable will be considerably restricted

in practice. To afford any chance of permanent benefit, the patient's health must be in a more favourable condition than we usually find towards the close of a chronic hip disease; the head of the femur must have undergone little alteration, and the cavity of the joint must be in a much better state than experience would lead us to infer after the destruction of parts which precedes spontaneous dislocation. The cases related by M. Humbert deserve, however, more attention than they have received; for in eight of them we find that the practice adopted was followed by complete and permanent success.

In this country, as I have already observed, surgeons are more inclined to adopt the method of excision, on which I shall now offer a few remarks. Whatever opinion we may form of excision when applied to joints, the articular surfaces of which are not separated from each other, I do not think that we are justified in having recourse to it in the cases now alluded to, namely, where the head of the femur has been dislocated on the dorsum ilii, in consequence of hip disease. In the first place, the disease under which the patient, in a great majority of cases, labours, is a constitutional one; and, in the next, it is almost always impossible to remove the whole of the diseased portions of bone, without which no permanent cure from the operation can be expected. There might, perhaps, be some strong ground for extirpating the head of the bone, if the complaint were confined to this part, but if we consider the general state of a patient during the last stage of hip-joint disease, we shall be at once convinced that any danger to life which may exist arises from the general condition of the subject; and that although the local malady may, to some extent, aggravate the general symptoms, yet it is not their cause, neither will its removal prove their remedy. Besides, let us consider a few of the conditions under which it is said the operation is justifiable. The head of the bone must be displaced, and be lodged on the dorsum of the ilium. But, in a great majority of cases, it will be found that the shortening of the limb does not depend on displacement of the head of the bone—a part which, in fact, has ceased to exist—but on destruction of the head from ulceration, and a drawing up of the trochanter to a level with the brim of the acetabulum. Another condition is that the acetabulum should be free from disease; but, I would ask, do we really possess any means of ascertaining whether the socket of the hip-joint may not be diseased, as well as the head of the femur—whether the acetabulum may not be extensively involved in the affection, ulcerated, carious, or necrosed? And if this were the case, what advantage can we expect from removing the head of the femur, when we leave other and more important parts behind untouched by the operation, though equally, if not more so, involved in the disease? You are to understand that what I have just said applies to dislocation of the head of the femur; and not to excision, when applied to the smaller joints, or in cases where the whole of the diseased bones can be removed, with some hope of the limb being tolerably useful afterwards.

The last point which we have to consider is amputation. This last resource in cases of articular disease is now more rarely necessary than it was considered some years ago, and before you think of employing so severe a measure, you must be well convinced that the only chance of saving the patient's life is to sacrifice his limb, or that the disease before you is absolutely incurable. To pass a decided opinion upon this point, much judgment and experience are required, for it is astonishing to what a degree articular disease may sometimes proceed, in young and scrofulous patients especially, without being necessarily incurable. I have seen many cases where amputation appeared to be the only resource, yet, by waiting a little, and by carefully attending to the general health, the patients have recovered with tolerably useful limbs. Before deciding on the question of amputation, you must carefully reflect on the general condition of the patient, and on the nature and degree of the lesions in the affected joint. You must have made up your mind that the local disease is incurable, and you must also have a reasonable hope that the removal of the limb will be followed by a restoration of health. With reference to this point, you will have to consider what effects the long-continued local disease may have produced on the general health and strength of the patient, but above all things you must assure yourselves that he is free from signs of pulmonary or abdominal tubercle. If these be present, amputation can only hasten death. On the other hand, patients often recover in a rapid manner, and from an apparently hopeless state, after the removal of the diseased limb. But you may ask, at what period of the dis-

ease, and under what conditions of the joint, should amputation be performed? This is a difficult question to answer in a general manner. I may, however, state that when complete disorganization of the joint is evidently going on, and cannot be arrested by the usual means,—when extensive abscesses exist around the joint, and the drain or irritation begins to tell on the patient's health,—when it is clear that the articular cartilages are more or less destroyed, the ligaments softened, the bones displaced, and when, after waiting for some time, you can perceive no tendency to anchylosis,—in all cases of this kind the propriety of having recourse to amputation is placed beyond doubt.—*Lancet, Sept.* 2, 1854, *p.* 185.

46.—ON AMPUTATION AT THE KNEE-JOINT.

By W. Fergusson, Esq. F.R.S., &c.

[There is a great difference between amputation at the *knee* and at the *knee-joint*. Sometimes it is called amputation at the knee, when it is either just above or just below this part. Amputation at the knee-joint has been performed but a few times in this country. The following case was admitted into King's College Hospital on January 25th, 1854. The joint was much diseased, the surfaces of the bones being rough and denuded, with a considerable collection of matter in the upper part of the leg. Mr. Fergusson amputated the limb at the joint, in the way described in his work, and then proceeded to make the following remarks:]

When I first performed the operation, to my knowledge it had not been previously done in England, for I cannot find any record of the fact, and such an operation performed for the first time would not likely be passed over without some notice being publicly made.

This operation has since been followed by others, among the first of whom I may name Dr. George Williamson, now in India, one of my former assistants. The first time I performed this operation was on the person of a full-grown man; (the case is mentioned in the third edition of my work on 'Practical Surgery;') and I have rarely seen or made a better stump. He has repeatedly walked forty miles a-day, and once walked one hundred and twenty miles in three days; and, what is more astonishing, his false leg was but indifferently made and padded, the spoke of an old wheel being considered by the man an excellent substitute for a more expensive contrivance.

In consequence of such great advantages arising from my first trial of the operation, I have since frequently performed it. Notwithstanding, several objections have been made. Mr. Syme, who had performed the operation in Scotland before I attempted it here, had taken a dislike to the proceeding from something that went wrong in his own cases. Mr. Syme imagines that greater danger is incurred by a larger surface of bone being exposed, by the removal only of the condyles, than if the bone be sawn higher up in the shaft. But mischief, I think, is more apt to occur when the bone is sawn in the shaft. Where the bone is vascular, I think there is little chance of necrosis, and much less of caries; and you have frequently seen how kindly the two cut surfaces of the spongy portions of bone heal in cases of excision of the elbow-joint.

Another objection made is, that the length of the stump is very awkward. This I do not admit; if the stump be short, an apparatus cannot conveniently be fitted, and the bone, when cut too high, is liable to be tilted forward by the psoas and iliacus muscles. Indeed, I cannot perceive any objection to a long stump. Objections have been made to a long stump of the leg, and amputation of the leg is often recommended to be done a short distance below the knee, but I am doubtful of the utility of such a step as a general rule.

Again a long stump in the thigh can never hinder in any way; besides, the leverage is much greater than if it were only half the length. In addition, the great breadth of bone, when well covered, is better able to support the weight required to be borne.

This objection might be raised by some,—that this operation is not truly an amputation at the joint, as the condyles were taken away. If such be allowed, then one might say that Mr. Syme's operation at the ankle is not an amputation at the

joint, for he always removes the malleoli; but such an objection could not be held reasonable, and the operation now under consideration I deem a great addition to the history of amputation, and have taught it as such for the last ten years.

I now perceive that for nearly the first time mention is made of this operation in the surgical journals of the day, which, I make no doubt, will greatly tend to the advantage of this department of surgery.

When I first commenced my profession, it was an understood rule, with but few exceptions, that the coverings of the bone in an amputation should be taken from the sound parts of the region where the operation was required; as, for instance, in amputation of the thigh, the soft parts were always taken from the substance of the thigh; so also in amputations of the leg. But in this case, and in amputation at the knee, the soft parts covering the end of the femur are actually the tissues that originally constituted the calf of the leg.

In the history of amputation it has always been the aim of the surgeon to make a good stump, its quality depending greatly upon the proper covering of the bone. If the soft parts be scanty, a bad stump must result; if, on the other hand, the covering be too large, the result will likewise be unsatisfactory. A remarkable instance of this latter kind was under notice last summer in this hospital. But there is more danger of the covering being scanty than profuse. The fleshy condition of the covering, as you know, is ultimately converted more or less into a fibrous texture.

Though amputation cannot be said to be the opprobrium of surgery, an axiom I laid down in my first paper on Conservative Surgery—"For the greater proportion of sound material that we can save in any operation on the body, the nearer we come to the perfection of good surgery"—yet I think amputation at the knee-joint may fairly have at least a footing in the province of conservative surgery.

This operation has lain for some time in abeyance. I now find my name associated with it in the journals, in papers which have recently appeared on the subject from the abler pens of my friends Mr. Potter, of Newcastle, and Mr. Jones, of Jersey—men who, with myself, I would fain hope, have no desire to have their names connected with novelties, unless they be for the good of our fellow-creatures and the advancement of surgery

In cases of injury of the joint, including great contusion of soft parts, I am doubtful whether the operation should not be effected above the seat of injury. As to the mode of performing the operation, I first make a small anterior flap, drawing the knife across the front of the joint, and then, inserting the point of the blade behind the femur, thrust it through to the other side, close to the condyles; then, carrying it downwards, cut the posterior flap from the calf of the leg. The saw is then applied a little above the condyles, and the flaps brought together as in an ordinary amputation.

My friend Mr. Potter, of Newcastle, saws through the bone before making the posterior flap; but I prefer the method I have described, although the great aim is to obtain sufficient material to cover the bone.

In some instances I first effect the separation of the leg at the articular ends, and thereafter cut away as much of the femur as seems needful. In all cases it is requisite to take the full length of the calf for the posterior flap, as the soft parts of the back of the thigh contract very much in the course of time. The patella might be saved in some examples, but in general I think it would be best to remove it.—*Med. Times and Gazette, July* 8, 1854, *p.* 26.

47.—ON AMPUTATION AT THE KNEE-JOINT.

By G. M. Jones, Esq., Jersey.

[In an interesting case of comminuted fracture of both bones of the right leg Mr. Jones proceeded to amputate at the knee-joint. There was a considerable lacerated wound about three inches above the internal malleolus, through which near two inches of the tibia protruded.]

Operation, Nov. 10.—As the greater portion of the integument covering the

patella had suffered materially from erysipelatous inflammation, it was thought advisable to remove as much of it as possible; consequently an incision, which commenced just over the internal condyle, was carried, in a semilunar form, over to the opposite one, where it joined, in a transverse direction, a part of the sinus laid open three weeks before, and, for the reason just mentioned, its concavity was made to touch the superior edge of the patella, the upper attachment of which was next divided while the knee was bent. The knee was again straightened, and the knife, introduced at the point where the first incision commenced, was carried downwards a considerable way along the edge of the tibia, then brought across the calf in a lunated form, and carried upwards along the side of the fibula until it met the extreme point already mentioned; the knee again forcibly bent permitted the different ligaments of the joint to be easily divided. The fore-finger and thumb of the left hand were then used as retractors for the already divided integuments, while the palm, thrust through the soft textures of the under part of the joint, followed them, and thus completed the flap. The cartilaginous surface was next removed, and other portions pared off. But two vessels required ligature. The parts were now brought together, and kept so by means of sutures; and pledgets of lint moistened with cold water were applied to the stump.

The hemorrhage during the operation was very trifling. The patient was, however, removed from the operating table in an alarmingly weak state, and continued so for several hours after, so that powerful stimuli had frequently to be given, and hot-water bottles applied to the extremities.

During the recovery, which was both tedious and protracted, the same plan of treatment was, with slight deviations, followed out. The local treatment consisted principally of the water-dressing and moderate pressure applied either by adhesive plaster or bandage. The system was supported throughout by the most nutritious diet. For two months the patient was allowed one bottle of port wine and two pints of porter per day, and had quinine, &c., given in large doses. For upwards of a fortnight after the operation, the patient's extreme debility gave rise to well-grounded fears that death would soon terminate his sufferings, and, as may well be supposed, the appearance of the stump during this anxious period was anything but satisfactory; the stomach, however, never once rejected the food given, even when most disinclined to take any. This one happy feature in the case was naturally soon followed by an improvement in his health, so that at the expiration of six weeks all fear with regard to an unfavourable termination was at an end. On the 20th Feb. the stump was healed (it might almost have been considered so some time before), and the patient was able to move about on crutches.

Remarks.—Although amputation at the knee-joint has been occasionally performed on the Continent, and recommended by Velpeau, and other eminent surgeons, it does not appear to have met with the approval of British practitioners. In proof that such was, at all events, the case very recently, Mr. Fergusson, in his second edition of "Practical Surgery," published that year, while speaking of this operation, states, "Amputation at the knee-joint has never, to my knowledge, been performed in this country." And on referring to the works of Skey, Miller, Druitt, and others, I find no mention of the operation, either as approved of or condemned; although Mr. Fergusson, in a later edition (his last, 1852), speaks of cases which have since come under his own practice, in which the results have been so satisfactory that he is disposed to consider the operation worth the attention of surgeons. I have not been able to find that the recommendation of this celebrated surgeon to carry out the views of Mr. Syme, to whom the profession is certainly indebted for that which may almost be regarded as a revival of Hoin's operation, has been attended to by practitioners in general.

The entering so fully into the details of this case prior to the operation, may at first sight appear unnecessary. My motive is to show, that the low, debilitated state the patient was in would in all probability have retarded recovery quite as much had amputation been performed through the continuity of the thigh as at the knee-joint. The time taken in effecting a cure must not, therefore, be considered as prejudicial to the operation.

The principal aim of surgeons of the present day is to avoid amputation if possible; and, failing this, to retain as large a portion of the limb as may prove of the

greatest use and comfort to the patient. My endeavours to effect the first were, unfortunately, unsuccessful; not so the second, as the present state of the patient will, I think, satisfactorily prove.

The man has now a most serviceable stump, upon which he can bear the weight of his trunk in progression. A good firm cushion of muscular and integumental structures covers the extremity of the femur; it can be manipulated without the least pain. The posterior flap, formed out of the muscles of the calf, and which was brought up to cover the condyles, has united to the anterior one, and the line of their union is two inches above the extremity of the bone. The internal condyle is abundantly covered with soft structures, the external not so completely, the greater prominence of this part being in some degree apparent through the cicatrix; with this exception, the stump may be considered a perfect one; practically, I have no doubt it will, in time, prove such.

This case proves that great advantages result, as far as concerns progression in his mode of amputation; there is not such an amount of lateral and outward motion required; the limb does not in walking describe the semicircular curve which one so commonly remarks in amputations higher up; the gait is more that which follows on anchylosis of the knee-joint; the limb is brought more directly forwards and backwards by the rectus and posterior extensors; the walk is consequently more firm and assured, and, as far as the trunk is concerned, there is not the same demand for compensating muscular power in steadying and balancing the body, such as one finds in a greater or less degree in different persons. The deduction, therefore, to be drawn from this case is, that a patient is evidently benefited by preserving as great a length of femur as possible.—*Med. Times and Gazette, June* 3, 1854, p. 570.

48.—ON EXCISION OF THE KNEE-JOINT.

By W. Fergusson, Esq., F.R.S., &c.

A few years back excision of the head of the femur was viewed with much distrust by a great many surgeons, and warm discussions took place respecting the operation. The opposition became, however, gradually weaker in proportion as the results were more and more favourable, and at the present time the great majority of hospital surgeons of this metropolis are reconciled to this procedure. The great point with the resection of the head of the femur, as well as with the resection of other processes of bone, is to make a proper choice of cases, the object being to save life before the disease has exhausted the patient, and to favour the formation of a new joint, likely to be endowed with sufficient mobility to render the limb valuable and useful. This end has been attained in several instances, and it is to be hoped that the operation will be the means of restoring to health and usefulness many a sufferer from hip-joint disease.

But it should not be forgotten that there is such a thing as a spontaneous formation of a new joint on the dorsum ilii, and that the possibility of such a termination should be kept in view when operative measures are contemplated. No inconsiderable number of young subjects may be noticed in the streets of London, with whom it is evident that the disease has ended in dislocation of a more or less perfect head of the femur on the dorsum ilii, the new joint enjoying a very fair amount of motion, and the limb being of course considerably shortened.

Whether excision of the knee-joint will ever gain as much favour as the operation just alluded to is doubtful, because the incisions and removal of osseous parts are much more extensive than about the hip, and the risk of inordinate inflammation and constitutional irritation much greater. Anchylosis of the knee-joint is also a more frequent occurrence than the same change in the hip, and there is less prospect of efficient support from an excised and partly anchylosed knee than from a new joint formed on the broad and resisting surface of the ilium.

But the parallel we have just drawn is not of such immediate practical importance as the juxta-position of excision of the knee-joint and amputation of the thigh, for these two operations are frequently the only alternatives left. It is, of course, a great advantage to arrest the disease of the knee-joint and preserve the limb, and surgeons would perhaps be inclined always to give a trial to resection before proceeding to amputation, if it were not plain that the work of repair, after excision of

the joint, is extremely slow, and may tell injuriously upon an already impaired constitution and weakened frame. Nor can it be forgotten that the interference with osseous texture is much more extensive in resection than amputation, and that a stump is much sooner formed than an imperfect union between osseous surfaces laid bare by the saw. But Mr. Jones, of Jersey, silences objections by pointing to his successful cases, and we gladly submit to such peremptory arguments.

In the case before us, Mr. Fergusson was inclined to allow the knee-joint of his young patient to anchylose, the boy, who is about ten years of age, having suffered from chronic inflammation and disorganization of the articulation for several years. He was admitted into the hospital about six months before the present operation, and his health and the aspect of the joint improved so much that Mr. Fergusson sent him into the country, with the hope that the disease would become arrested, and anchylosis take place. But the poor little boy came back to the hospital with a renewed inflammatory attack of the joint, and no alternative was now left but amputation or resection.

Mr. Fergusson decided in favour of the latter operation, and on the 29th of July the child was brought into the theatre, and narcotized with chloroform. The right lower extremity was much emaciated, the knee-joint presented the usual globular shape, but no fistulous apertures were observable. Mr. Fergusson made the usual H-shaped incision in front of the joint, and having reflected the upper and lower flaps, the condyles of the femur and the articular surfaces of the tibia were removed by the transverse section of a broad and short saw, after the soft parts had been carefully cleared away. Mr. Fergusson had, in previous operations, removed the patella also; but in this instance he left that bone, after having gouged the carious surface it presented. It was also found, when the operation was nearly completed, that the lower part of the shaft of the femur, close to the condyles just removed, was bare of periosteum, and Mr. Fergusson judged it prudent to remove the unprotected portion of bone to the extent of about an inch. When the leg was attempted to be brought on a line with the thigh, it was found that the rigidity of the hamstrings prevented apposition. Mr. Fergusson therefore proceeded to divide the inner hamstring subcutaneously, and no obstacle was then left to the approximation of the osseous surfaces. The margins formed by the incisions into the soft parts were then brought together by sutures, after a small quantity of pus had been pressed out from the side of the head of the tibia.

When the little patient had been removed, Mr. Fergusson stated that he had hesitated between amputation and excision, but that he thought the present a fair case for the latter operation, which, as was well known, had originated in this hospital. Much discussion had occurred respecting excision of this joint; but he (Mr. Fergusson) thought himself quite justified in having recourse to it in cases like the present. He had not been as successful as could be wished in some of the cases in which he had performed that resection, but no better result would perhaps have followed amputation of the limb.

Mr. Jones, of Jersey, and Dr. Mackenzie, of Edinburgh, had had cases which ended very favourably, and in which scarcely any deformity was left. Indeed, Mr. Jones performed the operation on a patient with whom the hip-joint of the previously sound limb was attacked, after excision of the knee-joint had been performed on the opposite side. Had the limb been removed the patient would have then been thoroughly disabled.

Mr. Fergusson went on to say, that further experience was necessary in order to come to some definite conclusions respecting the operation; but that it might even now be gathered from the twenty-three or twenty-four operations which had been recorded, that some reliance might be placed on this operative procedure.—*Lancet*, *Aug.* 12, 1854, *p.* 123.

49.—SUCCESSFUL CASE OF EXCISION OF THE KNEE-JOINT.

By Dr. Charles Cotton, Senior Surgeon to the West Norfolk Hospital.

[The patient was a boy nine years of age. The disease had existed three years. The knee was considerably enlarged, limb semiflexed, and foot everted. Chloroform having been given, the limb was forcibly straightened, and secured in this posi

tion by a splint. His health having suffered a good deal, he was made an out-patient for a time, and readmitted on Sept. 27th.]

General health much impaired; was pale and sickly. He had no appetite, got but little sleep at night, and suffered much from pain in the knee, which was tender to the touch. The foot, secured to the foot-piece, was with the thigh and knee twisted outwards, the patient having turned the splint above to the inner side, giving the whole limb a very awkward appearance. On removing the splint, the integuments were found sound, and the knee much enlarged; but there did not appear any material fulness from fluid in the joint. The patella was completely dislocated on the outer condyle; the tibia was displaced backwards and outwards; and the condyles of the femur projected forwards and inwards. The whole leg and foot were so greatly rotated outwards on the femur, that the inner ankle was placed directly forwards, while the external malleolus was inclined completely in an opposite direction. The least examination of the limb caused apparently agonizing pain. The knee was shortly after condemned in consultation; and the choice of amputation or excision being left to the discretion of the surgeon, the latter was determined upon.

Oct. 5th. *Operation.* The boy, under the complete influence of chloroform, having been placed upon the operating table, the usual letter H incisions were boldly and quickly made. The flaps having been reflected, the patella, being diseased, was at once removed, exposing the joint, and giving exit to a quantity of pus. The remaining fleshy and ligamentous connexions were further divided by cutting close around the bones; and the partially destroyed crucial ligaments were also detached. The ends of the bones, which were carious and destitute of cartilage, being made to project by flexing the limb, the saw was applied to the femur, and a thin slice of its diseased surface was removed. The head of the tibia, which was much damaged, was treated in a similar manner; whilst that of the fibula, also involved, was excised with the cutting forceps. On bringing the parts into contact, by extending the limb, and correcting the eversion of the foot, the ends of the bones were found somewhat separated in front, but tightly wedged together posteriorly, the femur projecting considerably forwards. An additional slice of that bone was consequently cut with the saw, at a bevel angle, in a posterior direction; the soft parts and vessels being protected by a copper spatula; and the projecting edge in front was further bevelled off previously to adjusting the limb upon a ready prepared M'Intyre's splint. One vessel at the bottom of the wound, the azygos articular, spouted at first freely, but no bleeding of importance occurred. The flaps were brought loosely together by six sutures; and the boy was conveyed at once to bed, where he commenced whining and crying, from smarting at the knee, on recovering from the effects of the chloroform.

July 26th. *Present condition of the patient.*—The boy has occasionally presented at the hospital up to this time. He attends regularly at the village school, a distance from home. He walks firmly, and with tolerable speed, though with a stooping gait, which he is trying to correct, and which he thinks is owing to his having been obliged to sit so long a time at the hospital. The tendency forwards of the femur is strikingly lessened. There exists considerable power of flexion at the knee. The limb is equally developed with its fellow, and he is able to project it forwards or backwards with ease. Indeed, up to this period, the case, without exception, has been deemed one of a highly satisfactory character.

Remarks.—Formidable resections in the regions of continuity of bones, and *forcible straightenings of the limbs*, in all vicious positions of the knee, *no matter what the stage of disease*, as directed by M. Bonnet, have for several years been neither novelties nor unsuccessful proceedings in my practice at the Lynn Hospital, as the neighbouring professional gentlemen can testify, who have embraced the opportunity of witnessing the operations. My excisions of importance in the regions of joints have been, however, confined to four, one of the hip, two of the elbow, and one of the knee. That of the hip—a desperate but extremely interesting case of a girl—led to several months of ease and comfort; and death, as shown at the *post-mortem* inspection, was in no way induced by the operation. The two elbow excisions, in male and female adults, ultimately ended unfavourably, as I was obliged to amputate the upper arm in both cases, several months afterwards, at the request of the parties; in the one, for recurring disease of the elbow, and necrosis of the humerus; in the other, owing to the limb having become wasted and useless

—in fact, an encumbrance. In the case of the knee (now abstracted from the rough notes of my case-book, upwards of nine months after the operation), I was glad to embrace the opportunity of attempting the saving of the condemned limb by cutting out the knee—a proceeding I was about to put in practice so long ago as 1845, in a case of scrofulous degeneration of the joint, but for the negative sanction of the physicians, and a deference to a surgical opinion in favour of amputation. The youth of the patient, and the anæsthetic value of chloroform, together with my experience, which justifies no extreme fear of interference when the larger joint ends of bones have become severely diseased, led me to give a trial to excision in the present instance; and, strengthened by the result, and the reported cases of Mr. Jones, of Jersey, and also by the very elaborate paper of Dr. Mackenzie in the 'Monthly Journal,' I see no reason to regret my disregard of the strongly expressed and somewhat hastily promulgated censures of the Edinburgh authorities, for whose opinions in general I have the highest respect. I am no exclusive reporter of wondrous cures, neither do I presume to lecture upon an individual case, for, by the aggregate of successes and reverses, operative surgery can alone be fairly tested. I think, nevertheless, that the "*settled inexpediency*" of the operation remains yet unproved; and leave, therefore, further experience to determine upon the propriety of attempting the saving of the limb by cutting out the joint in certain cases of incurable disease of the knee.

At the hazard of incurring the charge of "recklessly risking life," "unprofitably spending time," and of having taken a step in surgery "in the wrong direction," I publish the notes and these few crude remarks on a case, the progress and termination of which has afforded here considerable interest and pleasure.—*Association Med. Journal, Aug.* 4, 1854, *p.* 696.

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§ 50.—*Case of Excision of the Knee-joint.* By Dr. GURDON BUCK.—[This case of excision of the knee-joint, related in the 'New York Medical Times' for March, 1854, was successful.]

A transverse incision was made from one condyle to the other, across the lower margin of the patella. A longitudinal incision intersected the middle of this, and extended four inches above and below it. After the flaps had been dissected up, the joint was opened by an incision across the ligamentum patellæ, and also across the lateral ligaments. The adhesions of the articular surfaces were broken up by gradual forced flexion. A slice was then removed, with the common amputating saw, from the inferior surface of the condyles of the femur, including the pulley-like surface intervening between. Special care was taken to make this section on a plane parallel with the surfaces of support upon which the condyles rest, when the body is erect. The articular surface of the tibia was next removed on a level with the upper extremity of the fibula. These broad fresh-cut bony surfaces, which were very vascular and healthy, admitted of accurate coaptation without stretching the tendons and other parts in the ham. To secure them in close contact, a flexible iron wire was passed through both bones on each side, and the two ends twisted and left out between the flaps of the skin. The patella, being disorganized and softened, was removed, except the superior margin. The flaps of integument were brought together with sutures and adhesive straps, and the limb placed in a fracture-box. The constitutional fever was moderate and disappeared within a fortnight. At the expiration of five and a half weeks, the wires, having become loose, were removed. No exfoliation was produced by their presence. At the end of nine weeks, the wound had entirely healed, and the patient could raise the limb from the bed. Slight motion between the bones was perceptible in the direction of flexion and extension, but none laterally. At the expiration of about three months, the patient was allowed to leave his bed, and use his crutches. On Feb. 1st, 1854, he had been steadily improving up to the present time, and walked with a cane only. There was no mobility between the bones. The difference in length between the two limbs was one inch and a half, which permitted the foot to clear the surface of the ground without the body being thrown to the opposite side.—*Association Med. Journal, Aug.* 11, 1854, *p.* 721.

51.—REMARKS ON THE LINES OF INCISION SUITABLE FOR OPERATIONS AT THE ANKLE-JOINT.

By T. P. Teale, Esq., F.L.S., Surgeon to the Leeds Infirmary.

Operations at the tarsus having become frequent, and the modes of performing them various, it is desirable to determine generally upon some operative proceeding which would afford easy access to the tarsal bones, and at the same time be compatible with the removal of the entire foot at the ankle-joint, should the disease of the bones be found during the progress of the operation too extensive to allow of a reasonable hope of the preservation of the foot.

The following mode of operating appears to me to afford these advantages.

A transverse incision is made across the sole of the foot, commencing about three-quarters of an inch in front of one malleolus, and ending at a similar point in front of the other malleolus. A second incision is then made in the median line, beginning over the tendo-Achillis, on a level with the ankle-joint, and joining the former at right angles in the sole of the foot. The two lateral flaps thus marked out being next dissected upwards close to the bones, the calcaneum and astragalus are freely exposed. By division of their ligamentous and tendinous connexions, one or both of these bones may be easily removed; and should it be thought desirable to remove also other bones of the tarsus, they may be readily reached by extending the median incision a little forwards. If, from the great extent of disease, it is found necessary to remove the entire foot, it may be accomplished by uniting the two extremities of the transverse incision by a curved incision across the dorsum of the foot. Even where it is originally intended to amputate at the ankle-joint, the operation by two lateral flaps, as here described, affords much greater facilities, as I have experienced, than the ordinary mode of detaching the soft parts in one flap or bag from the calcaneum.

My present object being chiefly to describe a mode of operating, and not to discuss the general question of the propriety of removing one or more diseased bones of the tarsus, I will merely on the latter point state my conviction, that such operations are of doubtful propriety when the disease is of a strumous character, originating in the cancellous structure of the bones; since, in this affection, after the removal of one or more carious bones, the exposed articular surfaces of the neighbouring bones may appear healthy, even while their cancellous structure is in an early stage of the disease. Whereas, in those cases of caries which have originated in disease of the ligaments or synovial membrane, the full extent of the disease is more likely to be exposed to the observation of the operator.

This objection, founded upon the strumous character of the disease, applies with much less force to the excision of joints like the elbow, which allow of the removal of the articular ends of all the bones entering into their composition.

[Mr. Teale illustrates his opinion by an interesting case from notes by Mr. J. W. Greenwood, the resident assistant surgeon of the Leeds Infirmary. The man was twenty-four years old and had disease of the right tarsus; several sinuses communicated with the astragalus and calcaneum. The excision of the diseased bones became necessary.]

July 30.—The patient having been placed under the influence of chloroform, Mr. Teale commenced an incision by plunging the point of a French bistoury down to the bone in front of the outer malleolus, at a distance of three-quarters of an inch from it. This incision was carried across the sole of the foot, and ended at a point situated three-quarters of an inch in front of the inner malleolus. By this incision the soft parts were divided down to the bones, and the calcaneo-cuboid joint opened. A second incision was now made in the median line, beginning posteriorly at a level with the ankle-joint, and extending along the tendo-Achillis, and under surface of the calcaneum, until it met the former incision in the sole of the foot. The two lateral flaps thus formed being dissected upwards, and the ligamentous connexions of the calcaneum divided, this bone was quickly detached. The under surface of the astragalus being now ascertained to be carious, this bone was in like manner quickly removed. The median incision was now extended a little forwards, to give more easy access to the cuboid, which was also diseased. The removal of this bone occupied a little more time than that of the former, but it was accomplished without

difficulty. After a careful examination of the articular surfaces of the tibia and fibula, and the exposed tarsal bones, no further disease was observed. A few small vessels were secured. The flaps were placed in apposition, and covered by wet lint, supported by a light bandage.

Six hours after the operation the wound was cleansed from clotted blood, and, after being united by sutures, was covered with wet lint. The limb was laid on the outer side, in a flexed position upon a leg-splint.

On examining the bones which had been removed, the cartilages were found ulcerated in several places, and the whole cancellous structure softened, so as to admit of their being readily cut by a scalpel.

August 3.—He has proceeded favourably. Sutures removed; wound united at several points; no sloughing. The flaps supported by adhesive straps, covered with lint wet with myrrh lotion.

[The case seems to have been tedious but successful. On May 3, 1854, *i. e.*, nine months afterwards, the man had the power of freely flexing and extending the foot, and could bear firm pressure on the heel, but still a little matter was discharged from two sinuses.]—*Med. Times and Gazette, May* 27, 1854, *p.* 539.

52.—ON THE AFTER CONSEQUENCES OF AMPUTATION.

By John Hamilton, Esq., Surgeon to Richmond Hospital, Dublin.

The first and most pressing danger after amputation is hemorrhage. It may arise from half an hour after the operation to any number of hours or days, till union in the stump is effected. It is this that makes the surgeon careful to have his patient watched by an assistant always at hand. I cannot forget the anxious hours I have spent, when, a student, I have been left to watch a stump after amputation. The suspended terror with which I have raised the sheet, from time to time, to see if blood was trickling down, or whether the bloody oozing, which always flows from the lips of a stump, was hemorrhage or not. If the bleeding is only trifling, I would have you not interfere; compression on the face of the stump and the application of cold is often sufficient to check an inconsiderable bleeding, which your opening the stump would only tend to render a considerable one; but should the blood flow freely, the stump be swollen, and clots appear between the spaces of the sticking-plaster, you must open it forthwith, and search for the bleeding vessel. Before, however, the dressings are removed, firm pressure should be applied over the femoral artery, by a steady assistant. This is much better than the tourniquet; it causes the veins to become turgid and add to the bleeding, which sometimes even comes from the femoral vein itself. When you have no one to help you, there is no choice but to apply the tourniquet. Then carefully clean the face of the wound of the clots which you will nearly always see covering it. The blood will now be found to come in three ways:—first, in a general oozing from the stump. In this case, if there are one or two spots whence the blood flows with more rapidity, you may take up the vessels; but, in this general bleeding, it is best to cover the part with shreddy lint, and press steadily with your fingers on it for some time. The bleeding will usually stop, or at least that bleeding which comes from small branches, temporarily increased or excited by the inflammatory action which has begun in the end of the amputated limb. Those of larger size will then be more readily found and secured. Then put together the sides of the wound with sticking-plaster, or compress and bandage, giving up all hope of union by the first intention.

A more serious hemorrhage, though happily very rare, is from the femoral artery itself. This may occur early, from the slipping off of the ligature, from its not having been tied sufficiently tight;—or at a later period, when the ligature has been drawn off, but no proper clot or union exists at the end of the vessel.

The third kind of hemorrhage is from the femoral vein, and is sometimes very profuse. At the time of the operation there is occasionally troublesome bleeding from the femoral vein, which ceases on the slackening of the tourniquet and removal of the tourniquet bandage; or is readily restrained by putting a small compress of lint over the cut end of the vein. Most surgeons dislike applying a ligature round a vein, but if the other means fail this must be done. I removed the leg of a young

man below the knee, for disease of the tarsus and ankle-joint: on the twentieth day after the operation, when all the ligatures had come away for several days, he was seized in the middle of the night with violent hemorrhage, not *per saltum*, but in a free flow, and of a dark colour. Pressure on the femoral artery did not stop it. All the stump had united except a small portion in the ham, which went like a large fistula high up, and from which the hemorrhage came. Pressure over this part with lint saturated in a strong solution of alum and in turpentine, stopped the hemorrhage, which, however, returned again and again, but was finally completely arrested by laying open the unhealed cavity from whence the hemorrhage came, and laying the compresses over an ulcer in the popliteal vein, from which the blood was seen to flow.

The cut surface in amputation is almost always large; and in sudden accidents, in muscular men, or in fat women, the wound you have made, particularly in the thigh, is very extensive indeed. Sometimes the whole, except where the ligatures are, will unite by the first intention; this, however, you can expect to happen but seldom;—generally, the external or tegumentary lip unites, while the inside is still ununited, and discharges pus, which you should carefully and gently express.

The smaller ligatures come away about the tenth day; the ligatures on the larger arteries, as the femoral, humoral, or posterior tibial, after the fourteenth, or longer; I have known even a month elapse before the last ligature came away. You should at each dressing, after the eighth day, pull gently at the ligatures; some of those on the smaller muscular arteries come readily away; but I use the word *gently* emphatically, because I witnessed a rough tug at a ligature once bring on a very alarming hemorrhage.

If the wound does not unite by the first intention, its progress is pretty much as follows:—for the first two days all goes on well, but the third day you hear that the patient has passed a restless night; he complains of pain in the stump, and his face is flushed, skin hot, pulse quick, and the tongue loaded. After removing the lint dressing, the stump appears swollen and red, and the bandage looks too tight; you slit it up and remove one of the strips of plaster, when the wound at once gapes, no union having taken place, and a quantity of thin, bloody matter gushes out; the patient complains of much tenderness, and can scarcely bear the gentlest compression by the sponge. Two things should be observed: first, not to put too many strips of plaster on, or too tightly, as room should be left for the matter to escape; and second, not to put the bandage on too tightly, or it will cause much uneasiness: let it be applied merely firmly enough to give support.

The Bone.—The usual course is for a lymphy exudation to cover the surface of the bone, which becomes adherent to the soft parts—this is when the greater part of the stump heals by the first intention; when it does not, the surface of the bone, particularly the cancellous structure, throws up granulations, and union by the second intention is accomplished. Finally, the end of the bone is covered by a firm, fibrous material, its edges are absorbed, and it becomes conical. There is, however, trouble from the bone in two ways: first, the action of the saw sometimes seems to kill the sawn surface of the bone; and after the stump has refused to heal for a long time, the centre remaining open and discharging thin matter, the bone can be felt hard and distinct at the bottom of the wound. At length it is perceived to be loose, and can, with a forceps, be withdrawn through the unhealed opening, or if not, an incision should be made, and it should be withdrawn. It sometimes presents a curious appearance; a perfect flat ring of exfoliated bone, about one-eighth of an inch or less of the end of the bone having become dead or separated entire. Another way is this: you see that the end of the bone is pressing against the upper part of the stump, and that the integument there is strained and tense in consequence; the bone being drawn upwards and forwards by the action of the psoas and iliacus muscles, while the soft parts are drawn downwards and backwards, partly by their own weight and also by the powerful traction of the hamstring muscles. You will best counteract these forces by putting a splint of wood, either flat or scored, at the under and back surface of the limb, with a well applied bandage, and the tension against the end of the bone is at once relieved. If you do not do this or it does not answer, the part against which the end of the bone projects and presses becomes white and glossy, then red and painful and very tender, then fluctuates, and finally ulcerates, and the end of the bone can be felt with a probe; but though you would think the bone

would exfoliate, it generally does not, but unites by granulation with the neighbouring parts, and all goes on well. A stump that unites by the first intention, or, at least, that heals soon, is much more likely to be a good plump one; when the healing is tedious from unhealthy action or diseased bone, no matter how well the operation has been done, the stump will be conical.

. Now, with regard to the nerves I have not much to say—they rarely give any trouble. Nerves in a stump are, as was ascertained long ago by Mr. Langstaff, found, after the lapse of some time, to have bulbous extremities. This, I may observe, seems to be the case when nerves have been divided from other causes. In a case of caries of the petrous portion of the temporal bone, where the portio dura was destroyed and completely divided, as it passed through the carious spot, the divided end of the nerve next the brain was bulbous, the end of the distal portion not so.

If therefore, you find, as in these plates and preparations, the ends of the nerves in an old stump ending in bulbs, you are not to consider them as diseased; nor if any pain has been felt that they are the cause of that pain; nor can I say that cases where they have been removed by operation have been satisfactory. Stumps are usually not very sensitive; but occasionally, from a constitutional, I believe, rather than a local cause, the most violent pain is experienced,—a pain, apparently the most intolerable, and resembling tic-douloureux—absent for a time, but returning in paroxysms of dreadful severity.

The case of the Marquis of Anglesea, whose leg was amputated at Waterloo, will be familiar to you: nothing that was tried gave him any permanent relief. The great Nelson, also, suffered from this nervous affection, after removal of the arm above the elbow, and many years of his life were rendered miserable by it.—*Dublin Hos. Gazette, Aug.* 15, 1854, *p.* 210.

53.—CASES OF ARTICULAR DISEASE ILLUSTRATING THE ADVANTAGES OF THE ACTUAL CAUTERY.

Under the care of Prof. Syme. (Reported by Dr. Joseph Lister.)

[The following interesting cases occurred in the practice of Prof. Syme, of Edinburgh. The first case commenced after cold and fatigue, about four months before admission into the Edinburgh Royal Infirmary. The pain in the shoulder was at first intense.]

On admission she complained of constant gnawing pain in the left shoulder, and extending down the limb as far as the elbow, and sometimes to the fingers; when in the sitting posture she held the affected limb with the other hand, to ease the pain; the arm was also affected with a feeling of numbness and weakness; and although the shoulder was not very tender on pressure, and very gentle passive motion of the arm could be performed, through a considerable angle, without pain, yet any attempts on her own part to move it, produced great aggravation of her sufferings. As a result, no doubt, of habitual disuse, the muscles about the shoulder were much atrophied, and this caused a remarkable apparent prominence of the bony points, viz., the spine of the scapula, the acromion, the anterior border of the outer part of the clavicle, and the head of the humerus. The shoulder had an appearance that suggested at first sight the idea of dislocation.

On the 3rd November, the patient being under the influence of chloroform, Mr. Syme cauterized thoroughly the skin over the anterior and posterior aspects of the joint, rubbing a red-hot cautery iron freely backwards and forwards four or five times over each part. It had the effect of raising and rubbing off the cuticle, but did not char the skin. An hour afterwards the patient was suffering but little pain.

Nov. 4. Said, with a smiling countenance, that she slept well last night, the first time for four months, and feels now no pain save that of the burns.

Nov 5. A poultice was applied yesterday; the pain of the burn is now gone, and she feels *no pain at all.* Says that she has not only lost all pain, but also that the feeling of numbness is gone from the limb, and that she seems to have more power in it. The burned parts present a white sloughy appearance.

The poultice was continued till the sloughs separated, when simple cerate was substituted for it, with the view of retarding, rather than promoting cicatrization.

Nov. 12. To-day she has been trying to lift the arm, and felt none of the old pain in the attempt.

Jan. 31, 1854. She has to-day left the infirmary. She has for some time past been gradually acquiring more and more power in the limb; she can move the arm backwards and forwards for a considerable extent, and even raise it slightly. The movements of the forearm are free; there is no tenderness whatever about the shoulder. The return of the use of the limb has been accompanied with a restoration of the fulness of the muscles, so that there is now no difference between the contour of the two shoulders. She continues quite free from spontaneous pain.

I saw her again towards the end of May. She was still quite free from pain, and there remained only some stiffness about the joint that prevented her from raising the arm to the full extent.

Case 2. *Disease of Shoulder-Joint; Actual Cautery; Cure.*—Lilly Kay, aged 50, admitted March 23, 1854. Has generally enjoyed good health, except that for the last twelve years she has suffered inconvenience from what she supposed to be rheumatism in the right shoulder, characterized by shooting pain, occurring more especially when she attempted to lift anything. In January last the limb became completely disabled from increase of the pain, which now assumed a gnawing as well as a shooting character, and also began to be felt in the elbow-joint, and in the arm, forearm, and hand. At this time she first observed the existence of swelling about the shoulder-joint.

The pain continued to increase till the time of her admission into the infirmary, when it was exceedingly severe; not constant, but frequently keeping her awake at night. She was unable to raise the arm from the side, and had a sense of weakness in the limb, and some stiffness of the hand. There was considerable swelling about the shoulder-joint, which was tender on pressure, particularly at the anterior and posterior aspects. On the day of admission, Mr. Syme applied the actual cautery freely over the anterior and posterior parts of the joint, the patient being under chloroform. From this time she lost the old pain entirely, or at least was uncertain whether that which she still felt was not altogether that of the burn; and though the pain of the burn was considerable till the sloughs separated, yet it was much less distressing than the old pain, for which it was substituted, so that she slept much better than before the application of the cautery. The sloughs came away on the 1st of April, on which day she had a slight return of the old pain near the wrist, but it has not occurred again, and she is now (4th April) quite easy. The swelling about the shoulder has almost entirely disappeared, and there is little, if any, tenderness; the sores are granulating healthily.

April 14. Continues quite easy.

She was discharged on the 27th of April; I saw her about a month after, and she still continued free from pain.

Case 3. *Disease of Wrist-Joint; Actual Cautery; Cure.*—Janet Archibald, aged 32, admitted November 2, 1853. Rather a weakly subject. In October last she "took a shivering," without any particular exposure to cold, and a pricking pain came on in the left wrist, which increased for a time, and was accompanied with swelling. She applied poultices medicated with acetate of lead, and under their use a great improvement had taken place at the end of five weeks, when she got fresh cold in it, as she says, and it became excessively painful; the pain continued ever after till her admission, and although its extreme severity was then somewhat mitigated, yet it kept her awake a good deal at night; it was partly dull and heavy, and partly of a shooting character, and extended down through the hand and fingers. There was also an occasional tingling sensation in the fingers, and a sense of unnatural weight in the limb. A great degree of swelling existed about the wrist-joint, particularly on the dorsal aspect, and this part when manipulated gave a feeling very like that of fluctuation, so that her medical attendant had been desirous to open what he had supposed to be a collection of matter there.

Mr. Syme regarded the condition of the wrist as almost hopeless, but as he thought suppuration had not yet occurred, he determined to give the limb a chance with the actual cautery, which he accordingly applied on the dorsal aspect in two lines, crossing one another over the articulation. The pain and swelling both diminished greatly during the first four weeks after the cauterization; some aggravation of the symptoms then occurred for a time, but as the sore was still open, Mr. Syme thought it unnecessary to interfere further, and a gradual improvement after-

wards took place, till at the time of her leaving the infirmary (Feb. 14, 1854) there was scarcely any swelling and very little pain.

I saw her again on the 10th of June, there was then no swelling whatever about the wrist, and no uneasiness except a painful feeling of weakness when she exerted it much.

Case 4. *Disease between the Atlas and Axis; Actual Cautery applied with great benefit.*—Thomas Smith, aged 27, admitted June 20, 1854. Generally enjoyed good health till eighteen months ago, when a stiffness of the neck came on without any assignable cause, with pain when he turned round his head on the pillow; the pain increased greatly, and deprived him altogether of sleep for seven weeks, during which time he lost three stones in weight. There was severe pain in the head as well as in the neck, aggravated to an extreme degree by either nodding or turning of the head, particularly the latter, which, indeed, he at last never did without turning the rest of the body also. He applied to numerous medical men in Birmingham, where he lives; and blisters and caustic issues were repeatedly applied to the back of the neck, but never gave more than very slight and very transient relief, and he says that from the commencement of his complaint, he never had one minute's freedom from pain, except during sleep, till he came here.

At this time he was, according to his own account, about as bad as he had been at all. His countenance wore a peculiar expression of mingled suffering and apprehension, as Mr. Syme expressed it. He complained of severe pain in the neck and head, aggravated by any sudden movement, so that there was a great constraint about all his actions. He always kept his head bolt upright, except when in bed, and could neither lie down nor get up without supporting his head with his hands; he never turned his head without the rest of the body, but gentle nodding was not very painful. There was great swelling of the upper part of the neck, and he could only open his mouth a little way; deglutition was extremely difficult, and a remarkable prominence of the bodies of the upper cervical vertebræ was to be felt in the pharynx.

On the day after his admission, Mr. Syme applied the actual cautery over the spinous processes of the upper cervical vertebræ; the man was not under chloroform, and said he hardly knew whether the pain was greater even at the moment, than what he had experienced from caustic issues, and immediately afterwards he told us that he did not feel the pain of the burn at all. Next day he found less pain in moving the head, and in two or three days his countenance assumed a cheerful aspect. A steady daily improvement has since taken place in his symptoms, and at the present time (July 15) he has no pain whatever when he sits at rest, and can also use strong and active exertion without uneasiness, and no longer requires to support his head in lying down or rising; he can turn his head round pretty freely and look up to the ceiling, and it is only in sudden movements of the neck that he feels any pain at all. The swelling of the neck has greatly subsided, and he can open his jaws wide, and swallow with comparative facility. The sore on the neck is almost healed, and he talks of leaving the hospital in a few days as cured.

Remarks.—The above cases speak for themselves; and I might add several others, that exemplify in an equally striking manner the beneficial effects of the actual cautery in certain forms of articular disease. It will be observed that it is by no means so painful a remedy as is generally supposed, and also that its good effects are more than can be attributed to the mere discharge of pus from the sore which it produces, seeing that a great improvement commonly occurs within a few hours of its application, and long before suppuration is established.

It is now many years since the use of this means of counter-irritation was introduced into Great Britain by Mr. Syme; but although a constant series of successful cases have since continued to demonstrate its value to those who have witnessed his practice, yet I am satisfied that it has not hitherto been sufficiently generally appreciated. Case IV. is an example of its efficacy against a most formidable disease, where caustic issues had been long tried in vain. I believe many limbs and lives have been sacrificed that might have been saved by the actual cautery, and by it alone; and having been myself very strongly impressed with the importance of the subject, I should be truly glad if any surgeon who may have hitherto overlooked it, should be induced by the above report to inquire more closely into its merits.—*Monthly Journal of Medical Science, Aug.* 1854, *p.* 134.

54.—*Veratrine Ointment as a Local Application in Scrofulous Affections of the Joints.*—Dr. Klinger has employed, with success, an ointment, consisting of veratrine, gr. 5 to 10, dissolved in spirits of wine, and added to axunge ʒi. to promote absorption in scrofulous diseases of the joints. He considers its use inadmissible where heat, redness, or other signs of active disease exist; but when the affection has become chronic, he directs the bulk of a small bean of the ointment to be steadily and gently rubbed over the affected part, for a quarter of an hour daily. It produces a tingling sensation, with local increase of temperature, and promotes a restoration of the tissues to a state of health. In some instances, the itching which is caused proves distressing: he then omits the ointment, and uses friction with glycerine in its stead for a few days. The formication which attends the application of veratrine may extend over the entire body, and is often accompanied by an eruption resembling urticaria; there can be no doubt of its decided local stimulating powers, and that it is capable of materially aiding absorption in chronic disease. We ought also, possibly, to ascribe some part of the beneficial results in Dr. K.'s cases to the friction with which the ointment was applied.—*Glasgow Med. Jour.*—*Dublin Hosp. Gazette, Feb.* 15, 1854, *p.* 31.

55.—ON FRACTURE AND EXCISION OF THE ENDS OF BONES.

By J. Russell, Esq., Merthyr-Tydfil.

[Mr. Russell reports some interesting cases to show the practicability of confining separated and very moveable portions of bone by means of ligature applied immediately to the ends. This idea occurred to him in 1839, although he learned afterwards that Valentine Mott used the same in 1826. Dr. Mott thinks that the seton will be sufficient when the bones are in contact, or nearly so, and that when they are not so, nothing will answer better than resection of their fragments and their union by means of wire. Mr. Russell recites the following cases:]

Case 1. William Bennett, aged nine years, was brought to me about Christmas 1843. I was told that his injury had been received twenty-seven weeks previously. He had an ununited fracture of the humerus, at the point of junction of the middle and lower thirds. It was perfectly pliable; so that, when the upper arm was attempted to be raised to a horizontal position, the lower portion of the humerus, with the forearm, hung perpendicularly. Many fragments of bone had escaped during the progress of the case. The wounds had healed. A free incision was made on the outside of the arm, where the lower portion of bone was very prominent. I exposed the ends, and removed their points, endeavouring to do so at a point which would leave the denuded ends with a surface of the size of the natural circumference of the bone. The lower portion had penetrated and united to the triceps muscle. The ends of the bone were very pointed, and were connected by ligament; and a third detached portion, about half an inch long, was included in the ligament. I drilled a canal obliquely in each end, in the cylindrical portion of the bone, which terminated at about an inch from the ends. A piece of platina wire was passed through the canals, and twisted into a ligature. A slight degree of fever ensued, which lasted a few days. Very little suppuration took place, and the ligature separated on the thirteenth day, perfectly free from any exfoliated bone, and uncorroded. The bone united in the ordinary period, a month; no exfoliation followed, and the wound healed readily. The patient had in 1849 a good, firm, and useful arm, and was earning his living as a haulier in a colliery.

This case was reported in the Lancet, July 13th, 1850.

Case 2. Rees Jones, collier, aged 49, applied to me April 18th, 1851, with disease of the right elbow-joint; it was far advanced and extremely painful. There were wounds, which discharged freely. Accumulations of matter occurred from time to time, and free incisions were made for its escape, but no relief followed. He was very much emaciated; but he declared he was not much thinner than he used to be, and that he had been a tolerably healthy man through life. These two last statements, I was afterwards told, were not correct, he having been a sickly man for many years. He was anxious that some operation should be performed, if possible, without losing his arm. As it appeared probable that the emaciation proceeded from the continual suffering and drain from his arm, excision of the joint was

performed on May 30th. Canals were drilled in the cylindrical portion of the bones, and a piece of strong twine was passed through, and the bones were brought together by tying it tightly. The arm continued very painful after the operation, and, about the end of seventeen days, the ligature remaining firm, it was cut through and removed. No relief followed. But the pain continued severe and constant, and the arm was amputated at about the middle of the humerus in August. The ends of the bones were found covered by a gelatinous pulp, and rough and carious. The pain continued to affect the stump. He was also subject to violent attacks of pain, which he referred to the right side of the abdomen. He gradually grew worse, and died in May, ten months after the removal of the arm.

Examination of the body.—On opening the chest, the lungs presented one uniform black mass, which appeared to result from spurious melanosis. The intestines were amassed by cohesions. The liver was of the ordinary size, but, upon being cut into, presented a mottled nutmeg-like appearance.

Case 3. John Williams, aged 58, a railway haulier, fractured his leg Nov. 6th, 1852, from his foot becoming entangled in the wheel of a loaded waggon while endeavouring to unfix it. The tibia was fractured obliquely at the junction of the middle and lower third. No apparent injury had occurred to the skin. The limb was set, and put up in the ordinary way, with side splints.

Nov. 7th. He had suffered great pain. The leg was covered with vesicles; most of them were filled with a dark coloured fluid. The foot was of low temperature. There was but little constitutional disturbance. Warmth was ordered to be constantly applied.

Nov. 8th. The foot had recovered warmth; the vesicles were subsiding; and a distinctly marked slough, about two inches in diameter, was present over the seat of fracture.

Nov. 22nd. The slough was removed, and the prominent pointed extremity of the lower portion of the tibia could be seen in the centre of the wound, and much raised. It was denuded of periosteum, and blanched.

Nov. 23rd. After enlarging the wound, I removed by the saw the points of both upper and lower portions. Upon again examining the wound, an isolated large piece of bone was discovered lying at the back part of the wound, strongly held by the interosseous ligament, and required much force for its disengagement. Canals were then bored in the cylindrical part of the bone, about three-eighths of an inch long at the front aspect of the tibia; and a piece of copper wire (No. 16) was passed though them, and twisted tightly, till the ends of the bone came firmly in contact. The limb was placed on a pillow, and supported at the sides.

Nov. 28th. Everything had progressed favourably. The lower portion of bone had fallen slightly backwards, but was easily raised by the ligature. The ligature appeared slightly loosened.

Nov. 30th. The ligature was decidedly loose, and a small piece of exfoliated bone, about the size of a grain of rock powder, came away from the side of it. Exfoliation was still going on from the lower portion.

Dec. 2nd. The upper portion, as well as the medulla, was granulating healthily: the granulations surrounded the wire closely.

Dec. 3rd. The lower portion had again fallen back a fourth of an inch, and was apparently released from the ligature.

Dec. 4th. The ligature came away altogether by using very small force. It was free from any attachments, uncorroded, and not altered in any respect.

Dec. 5th. The bones were connected by a band of granulations, which could be stretched by extending the leg.

Dec. 7th. He was progressing favourably. A collection of matter had formed over the lower end of the fibula. The limb supported itself while lying on a pillow.

Dec. 13th. Firm union was advancing, and everything had progressed favourably since the last report.

Jan. 3rd, 1853. The wound was filling up. By grasping the leg above the seat of fracture, and raising it, it raised and supported the lower portion.

Jan. 13th. Union was quite firm. The limb was taken out of the cradle, and laid unsupported on a pillow. An abscess formed upon the heel during the severe weather, and the wounds healed slowly; but everything continued to progress favourably.

In August, 1853, the leg might be said to be perfectly sound, and the shape remarkably good. It was two inches shorter than its fellow.

Case 4. Lewis Evans, aged 22, complained of an affection of his elbow about Christmas 1851–52. His right foot had been previously affected, and at present there is an open sore on it. Counter-irritation and constitutional treatment were pursued; but suppuration took place, and wounds opened about the elbow-joint, about twelve months ago; and scrofulous disease of the joint was very evident. The wounds and discharge continued, and about last Christmas the idea of excision of the joint was entertained; but his constitution appeared so much impaired that it was not thought advisable to operate then. Many of his family are the subjects of scrofula in various forms. One has disease of the hip-joint, and one died of phthisis. Twelve months afterwards, the disease of the elbow and the constitutional symptoms appeared to be much the same, and to have been stationary for the last six months; and the wounds having put on a more healthy appearance, and the arm remaining useless and helpless, so much so as to require the assistance of its fellow for all movements, excision was determined on.

The operation was performed June 14th, 1853. The humerus was found to have lost its periosteum for a considerable distance, and a large portion of bone was required to be removed. After removing the olecranon, a deep rough notch could be felt on the anterior aspect of the ulna, about a quarter of an inch from the sawn end. This required the removal of a second piece from the ulna. The radius appeared sound. Holes were then bored through the cylindrical part of the bone, perpendicularly or nearly so, into the medullary cavity or centre of the cancellated structure. Common bottling wire was then passed double through the holes, and twisted till the ulna and humerus appeared to be in contact. Little constitutional disturbance followed the operation. On the second day, a very obtusely angular splint was applied to the inside of the arm.

June 15th. The splint was discontinued.

June 17th. The patient walked to my house a distance of five hundred yards.

July 14th. The ligature was quite firm. No pain was caused by pulling it. The wounds were granulating and healing.

July 15th. A large portion of the wound healed; the ligature remained as before. The thickening round the humerus was decreasing.

July 18th. He had felt considerable pain in his arm the day before, apparently caused by a small abscess over the former site of the internal condyle. I pulled very hard at the ligature, which was still firmly attached, which caused no pain. He had no return of pain.

July 21st. The wounds continued to heal fast. The ligature still endured a hard pull without yielding. The arm continued easy.

Aug. 3rd. The ligature continuing sound, it was pulled very hard and yielded about half an inch; very probably released from one bone. A quill was then passed through a loop in the wire, and a quantity of worsted was twisted round between the quill and arm.

Aug. 6th. The worsted had made a great depression. The wire was untwisted, and, while attempting to pull it out, it became released altogether. The arm was gradually acquiring strength, and the wounds were healing.

Aug. 14th. The upper arm was much inflamed, swollen, and hard; an abscess pointed in front of the head of the radius, and was opened.

Aug. 17th. A free discharge of pus had taken place, and the arm was altogether much improved.

Aug. 19th. The arm was much better.

Aug. 30th. The patient continued to improve. The thickening of the humerus and the enlargement of the arm, generally, were diminishing.

Sept. 26th. Several small wounds were still open. The patient could now raise his arm, and touch his upper lip with his thumb, unassisted.

Remarks.—I will now offer for consideration the most striking points, as they appear to me, of the foregoing cases, and my deductions from them. The object which Dr. Mott or M. Flaubert had in view when they first adopted the use of wire ligature in cases of ununited fracture, does not appear from the above account; nevertheless, whether it was used as a connecting agent only, or with the object of exciting action in the ends of the bones, is, I think, not material. The object at

which it is desirable to arrive is that which experience proves or renders probable. In the sixth case of Dr. Mott, the expression occurs, "joined by means of iron wire." In the ninth case (which he considers as unique, it being an example of intra-uterine non-union), we find the words, "and *connected them* with silver wire;" and part of the concluding sentence of the report is "resection of the fragments, and *their union* by means of wire." In all these cases, the wire appears prominent as an agent of connexion. My observations lead me to conclude that no action favourable to the union of bone is caused by the presence of the ligature. I view it as a perfect parallel to ligature and sutures in other living structures; and I believe that none think the presence of sutures in the integument or elsewhere favourable to healing otherwise than as an agent of approximation. In Case 3, the bones and wire were exposed to view, and the granulations from the bone closely surrounded the wire, which was copper: and no pain appeared to be caused by pulling the wire, which had remained in the bone long enough to have produced irritation, if calculated to do so. And in Case 4, the frequent and hard pulls produced no more sensation than to allow the patient to acknowledge that he felt it. In Case 1, the boy complained of a pricking pain for a part of the time of the presence of the wire in his arm, which I think, from its nature, may be justly referred to an angle in the wire. In Case 2, the patient suffered excessive pain, which had existed previous to the operation, and continued after the removal of the ligature, and even after the amputation of the limb; and the man's condition of body was, I think, sufficient to account for the failure.

My conclusions are, that wire ligature is a safe, useful, and certain agent for conducting points of severed bone into apposition, and confining them in that condition till either that union may have taken place, or that the soft parts immediately surrounding the seat of injury shall have adapted themselves to the position of the bones. Although ligature has hitherto been applied only to cases of ununited fracture (*i. e.*, permanently ununited), I advance the case of John Williams, No. 3, which will perhaps be admitted to show its utility in recent fractures, when the bones are very moveable; and I think it would be found very useful in military surgery or elsewhere, when the removal of the patient soon after the injury may be required, or such a necessity feared: and I would suggest the experiment of wire ligature in veterinary surgery. My present impression is, that the material of which the ligature consists does not signify. Dr. Mott has used silver and iron wires; I have used platina, copper, and iron wire, and common twine; and I have no reason to suppose that the texture of the last had any influence on the unfavourable result of the case. I did on one occasion contemplate the using of catgut; but I feared that the knot would yield when it became sodden by the discharge, and so rejected it. The applicability of ligature in recent cases of fracture would bring it into a much wider field of utility than that to which it has apparently been hitherto confined. And it appears that ligature is tolerated by bone with equal patience, both in recent cases of injury and chronic cases of injury and disease.—*Asso. Med. Jour.*, *June* 23, 1854, *p.* 547.

56.—ON COMPOUND FRACTURES.

By Dr. Samuel G. Wilmot, Surgeon to Steevens' Hospital, Dublin.

Compound fracture may be produced in two ways: either the violence may be so applied as at the same time to break the bone and tear the skin; or the force may at first merely break the bone; then the continuance of the same force, or another, applied in a different direction, may drive the upper fragment through the skin. In the case before us the upper fragment of the fibula protruded through the integuments. Now experience teaches us, that where the bone protrudes, making a small punctured wound, the case frequently takes a much more unfavourable course than where the skin has been extensively wounded; gangrene, fascial inflammation, and abscesses, are far more apt to occur. I have frequently observed this to be the case; and it is the result of Mr. Cusack's vast experience, that where the fibula, in particular, protrudes, making a small punctured wound, the case is most prone to run an unfavourable course. You see, then, that so far from measuring as you would be apt to do, the evil consequences likely to result from a compound fracture by the degree of injury done to the soft parts, on the contrary,

within certain limits, a small punctured wound will generally lead to worse results than where the skin is extensively lacerated.

The first step you have to take, when called to a case of compound fracture, is, of course, to endeavour to reduce the bones. When the upper fragment projects, the skin affords the first impediment to the reduction; the more you pull, the more you draw in the skin between the bones. There is but one means of overcoming this obstacle. Pass a probe-pointed bistoury beneath the lower lip of the wound in the skin, and divide it; this will at once liberate the bone, and allow it to slip back, and if perfect coaptation cannot be otherwise procured, a portion of the bone may afterwards be clipped off with a forceps. The bones being properly adjusted, the limb is to be placed on its side or back, according to the situation and the direction of the fracture; and now comes the question as to the advisability of applying bandages. There is some difference of opinion upon this point. Now, bandaging may be advantageous or injurious, just according to the particular period at which it may be done. If you see the case before swelling takes place, always apply bandages; they will not only keep down swelling, but will help to maintain the bones *in situ*, and will control the spasms of the muscles, which are so frequent an accompaniment both of simple and compound fractures. If much swelling, however, have taken place, bandaging must not be attempted; it will surely increase the tumefaction, excite vesication of the skin, cause pain and fever, and if done with any degree of tightness, may lead to gangrene. As to the treatment to be pursued in compound fracture, all you can do is to watch the symptoms as they arise, marking their character, and meeting them accordingly. During the early period of the case, you will have to deal with symptomatic inflammatory fever, which sometimes runs very high; then the type alters, and in cases attended with much suppuration, hectic fever becomes gradually established. One of the most troublesome symptoms, at this stage, is often diarrhœa, which sometimes proves quite uncontrollable by the most powerful astringents. The chief point, however, to attend to, in the course of compound fracture, is the formation of abscesses. The patient is going on satisfactorily; his pulse is tranquil, his skin cool; he sleeps well, neither does he experience any pain—when, suddenly, he is seized with a rigor, high fever is lit up, and he complains of severe pain. This change implies the formation of matter in some part of the limb; examine it carefully, and if you detect an abscess, open it at once. Sometimes the suppuration presents itself in the form of a succession of small abscesses; but, occasionally, a large collection of matter forms beneath the fascia, and unless this be let out by an early and free opening, it may burrow until it destroys the cellular tissue between the muscles to a serious extent.

Now, with respect to the performance of amputation in compound fracture, it may be had recourse to at three different periods; within twenty-four hours from the receipt of the injury—primary amputation; and after this period, principally for gangrene; and at a still more remote period, for hectic fever—secondary amputation.

As regards primary amputation, we should be very slow in having recourse to it. It is wonderful what powers of repair nature occasionally exhibits, particularly in young persons; so that unless the amount of injury be obviously irreparable, it is our duty to endeavour to preserve the limb. In those cases, however, in which primary amputation is unavoidable, we must take care to hit off one particular moment for its performance—when reaction is fairly established, but before it passes into fever. In cases of severe compound fractures of the limbs, particularly those inflicted by sudden and powerful violence, the individual remains in a state of collapse for several hours afterwards, and if amputation be performed under such circumstances, a fatal result may accrue. You must wait until reaction sets in; still, if you wait until it has glided into fever, you operate under a great disadvantage; your case will be very apt to have an unfavourable issue.

One of the greatest questions, however, in practical surgery, is the period at which amputation should be performed in compound fracture, when gangrene supervenes. For a long time there have existed two parties in the profession which entertain exactly opposite views upon this point. One contends that you should not operate until the gangrene has ceased to spread, and that the line of demarcation has been fairly established. This party is headed by the celebrated Pott, and we find among its ranks, besides many eminent surgeons, the late Mr. Colles, than whom there was no higher authority on a point of practice. He thus emphatically expressed himself

in his lectures;—"The patient has no chance of surviving amputation when the gangrene is spreading, and the fever high, and before the red line of demarcation has formed." The other party contend that amputation may be performed while the gangrene is spreading. It is headed by the distinguished Larrey, and we find Guthrie, and many other surgeons of repute, following on the same side. It is worthy of remark, however, that hitherto the advocates for amputating in spreading gangrene have been chiefly military surgeons; and I allude to this fact, because it is well known that conclusions drawn from the practice of military surgery are not always applicable to civil. Here, then, are two exactly dissimilar opinions on a point of the highest practical importance; and how are we to reconcile them? While every surgeon must admit the far greater probability of success attending amputation when performed after the gangrene has ceased to spread, there are few, I believe, at the present day, who would refrain from operating merely because the "red line" had not appeared; in many cases, were we to wait for this event, our expectations would never be realised. If, within a certain period, no disposition to an arrest of the gangrene appear, we ought to operate while the patient is strong enough to bear it. Should, however, the gangrene spread quickly, accompanied by great prostration of strength, delirium, and rapid pulse, the operation is inadmissible. If we are to afford a chance of recovery, in this disease, by amputation, we must be most particular in making a proper selection of the cases in which to operate. We do not operate indiscriminately in cases of diseased knee-joint, nor cancer of the breast; neither should we do so in spreading gangrene. Students are very apt to think, and even to argue, that in a desperate case, where death is inevitable if the disease should extend beyond a certain limit, it is justifiable to operate. This opinion, however, is most erroneous. We have no right to bring operative surgery into disrepute by having recourse to it, unless there be a fair chance of its being attended with success.—*Dublin Hos. Gaz., April* 1, 1854, *p.* 66.

57.—CASE OF UNUNITED FRACTURE SUCCESSFULLY TREATED BY PEGS.

By Dr. Edward Hutton, Surgeon to the Richmond Hospital, Dublin.

[This case was mismanaged for a long time by a bone-setter.]

On admission to the Richmond Hospital (Aug. 19th), his arm was almost quite useless to him; he was unable to raise it from his side, and the motions of the elbow were very limited; the fractured humerus made an angle salient anteriorly, and retreating posteriorly. On the fore and outer part of the arm there was a quantity of callus thrown out, but there was, nevertheless, between the fragments a considerable amount of motion, which did not produce the sensation of crepitus.

In order to get the fragments into a better position, extension was employed every day for three weeks, for the space of an hour, and while extension was kept up by the pulleys, splints were applied, and bandaged on as firmly as they could be done.

Nov. 2. The fracture still continuing ununited, and the patient's general health having so much improved as to place him in a favourable condition for the proposed plan of treatment, the following operation was performed:—The arm was forcibly extended, so as to remove deformity as much as possible, two incisions were made down to the bone on the outer and anterior aspect of the arm, just sufficient to admit of the entrance of a drill, by means of which two holes were made, two inches deep, each perforating both fragments, and, of course, the connecting medium between them; into these holes two ivory pegs, two and a-half inches long, and furnished with flattened heads, were driven; one of these passed readily through somewhat rarified osseous structure; the other, which entered about an inch above and to the inner side, could not be driven farther than to the extent of the hole made by the drill (*i. e.*, two inches), in consequence of the dense texture of this part of the bone at the time of operation; but a few days later it was easily pushed home, the bony structure having become soft and readily penetrated.

After the operation, water dressing was applied, and the whole limb placed in a cradle splint of open metal work, and kept at perfect rest; on the third day following there was a slight amount of inflammation, the dressings were renewed, and the splints re-applied.

On the seventh day after the operation he began to suffer slight pain at the back of the arm opposite the fracture, and on the ninth day, a small abscess which had been formed was opened; this discharged healthy matter, and soon healed.

At the end of the seventh week the pegs were removed. The upper one remained unaltered, but the end of the lower one was absorbed, having a rough spiculated extremity, resembling the fang of a tooth in process of absorption. There was still slight mobility at the seat of fracture; splints were applied to the upper arm and firmly bandaged, and the whole limb was still kept at rest by means of an angular splint: this was continued for three weeks, when the arm felt quite firm, and the angular splint was removed.

Jan. 12, 1854. The elbow is still a little stiff; there is no perceptible amount of shortening of the limb; on introducing a probe into the openings, the holes from which the pegs were removed seem to be filling up with bone; the swelling around the fracture has much subsided, and the arm is now very useful. He can lift heavy weights, and work with a shovel with strength and ease.

Observations.—In oblique fractures this mode of treatment seems to be particularly appropriate, as the ivory pegs being passed through the fragments bind them together closely, and keep them fixed. Being so inserted they excite vital action not only in the medium of connexion between the fragments, but in the bones themselves, as was manifested by the softening of the indurated osseous texture, owing to the absorption of the earthy particles, and by the absorption of the extremity of the ivory peg in the softer part of the bone.

It can scarcely be doubted, that the new bone which filled the trajet of the bored holes, passed through from fragment to fragment, and thus aided in cementing the union, which took place in an unusually short period, viz., seventy days from the date of insertion of the pegs; there was throughout little or no constitutional disturbance, nor was there any excessive local inflammation.—*Dublin Hospital Gazette, Feb.* 15, 1854, *p.* 20.

58.—*Treatment of Fractured Patella.* By Le Gros Clark, Esq.—[In cases of transverse fracture of the patella, it is sometimes very difficult to secure co-aptation of the fragments. Mr. Clark employed lately a novel expedient.]

It consisted of a cap of gutta percha moulded to the front aspect and sides of the joint, and with a hole cut in its centre large enough to receive the patella. The two halves of the broken bone having been by manipulation brought into apposition, the splint, lightly padded with lint, was placed over them and confined by means of bandage. Its influence was thus, by pressure from above and below, to keep the fragments in exact position. The case in which the plan was adopted did remarkably well, and after the first reduction no trouble with subsequent displacements occurred. It is of course advisable to adopt the ordinary measure of flexing the thigh as much as possible on the pelvis during the treatment, and with this view, in the present instance, the leg was swung by a broad suspensory bandage passed beneath the calf and fixed to the curtain-pole of the bedstead. The cap was not applied until the tumefaction following the receipt of the injury had subsided. The above accident occurred to a female. A similar case of transverse fracture of the patella in a man, is now under the same treatment in the Hospital, under Mr. Le Gros Clark, and promises an equally favourable result. The application of this simple apparatus has not been attended by any inconvenience in either case.—*Med. Times and Gazette, July* 1, 1854, *p.* 12.

59.—*Recipe for making Felt Splints.*—These splints were formerly sold at our instrument-makers. We were in the habit of using them with satisfaction, but latterly have not been able to procure them. Professor Frank H. Hamilton remarks—we think justly—that they are, in some respects, superior to gutta percha, and he gives ('Buffalo Med. Journal,' Dec. 1853) the following recipe for making them:—"Dissolve three pounds of gum shellac in two quarts of alcohol. It should be dissolved in a tin vessel, furnished with a tight cover, to prevent evaporation. Spread

a piece of old or new woollen cloth on a board, and, with a clean brush, saturate both sides of the cloth with the solution. Hang it up until it is thoroughly dried. Lay it again upon the board, and apply a second coat of the solution to one side only of the cloth. Dry again, and apply a third coat to the same side. There will be now three successive layers upon one side, and one on the opposite. While the last coat is yet fresh, fold the cloth so that the side having three coats shall be applied to itself. Now, with a hot flat-iron, smooth and press the surfaces together. When it is cold, a slight rubbing with sand-paper makes it fit for use. It becomes a flat, almost unyielding board; but exposure to a moderate heat will make it pliant, so that it can easily and accurately be adapted to any surface."—*Amer. Jour.—Med. Times and Gazette, June* 17, 1854, *p.* 638.

ORGANS OF CIRCULATION.

60.—CASE OF TRAUMATIC CAROTID ANEURISM.

By W. R. Beaumont, Esq., late Professor of Surgery, Toronto.

[About eight weeks before admission into the Toronto General Hospital, this patient had been stabbed in the neck. This wound seems to have penetrated the carotid artery, and was followed by excessive hemorrhage.]

On examination, a very prominent tumour was found on the left side of the neck, seated close to the clavicle, and extending transversely from the inner to the outer border of the sternocleido-mastoideus, having made its way through the cut fibres of the muscle. The tumour was distinctly circumscribed, somewhat conical in form, and circular at its base, which was about two inches in diameter. On the summit of the tumour was a stretched and thin cicatrix, about one inch and a quarter long, by half-an-inch broad. The direction of the long diameter of the cicatrix was oblique, and rather nearer to a horizontal than to a vertical line. We do not know the weapon with which the wound was made, nor do we know whether its point was directed horizontally, as in making a thrust, and so piercing the artery on the same horizontal plane as that on which the cicatrix is placed; or whether the weapon had been held as a knife or dagger usually is held in the act of stabbing, and its point directed downwards. In the latter case, the artery would be pierced considerably below the level of the cicatrix. The tumour pulsated strongly. To the touch, it gave a distinct vibratory sensation or thrill, and to the ear a loud *bruit de soufflet*. By compression, the sac could be emptied; and by firm pressure on the carotid distal of the tumour, the sac became much more tense and resisting to the touch, and, I believe, somewhat larger, which latter circumstances I consider the best evidence of the tumour being aneurismal, and of the aneurism being carotid. His pulse was 84, and moderate as to force and fulness; his appetite good. I ordered ten minims of tincture of digitalis three times a day, spoon diet, and perfect quietude.

On the ninth of January, a week after admission, the tumour and his pulse remained the same. He was now bled to ten ounces (the blood containing three ounces and a half of serum, and six ounces and a half of coagulum). His diet was restricted to a pound of bread and a pint of milk per diem, and the digitalis was continued as before. On this day I made a cast of the tumour, in order to ascertain with accuracy any increase or diminution in its size.

On January the 15th, thirteen days after admission, and six after bleeding, the tumour appeared smaller, pulsated less strongly, and its vibratory feel was less marked. Pulsation had occasionally been observed in the external jugular vein, which I conceive may have been caused either by a communication between the currents of blood in the common carotid and in the internal jugular, or by the aneurismal tumour striking at each pulsation the external jugular, with which vein the tumour was nearly in contact. The pulse had been 72 for the last three or four days, and the patient felt rather weaker than on admission.

[The treatment usually called Valsalva's continued to be pursued, and nine weeks after the first bleeding, and eighteen weeks after the wound, the tumour was no longer visible, although the bruit was as loud as ever, sounding like the roar of the sea.]—*Lancet, July* 29, 1854, *p.* 75.

61.—*Treatment of Aneurism by Peroxide of Iron.*—Dr. Pavesi, of Bergamo, has lately injected a strong solution of the above-named salt into an aneurism of the temporal artery affecting a young man. The sac was about one inch from above downwards, eight lines transversely, and might contain a drachm of fluid. A puncture was made into the tumour, and bright arterial blood escaped; the glass-pipe of one of Charrière's syringes was then introduced, and about sixteen drops of the solution thrown in. The tumour was ten minutes afterwards quite solid, and in one month no trace of the aneurismal tumour was left.—*Lancet, Aug. 5, 1854, p. 104.*

62.—*Is Traumatic Phlebitis a Contagious Disease?*—[In a conversation at St. Bartholomew's Hospital, this question was brought up by Dr. Simpson, of Edinburgh. It arose from the fact of the great frequency of phlebitis after operations.]

Dr. Simpson stated his conviction that phlebitis is essentially in the same category as puerperal fever, some forms of erysipelas, &c., and that it is capable of being conveyed from patient to patient by the surgeon, just as the accoucheur is well known to convey the latter diseases. He believed that the operator's hands, or even the knives used, might thus be made the means of transmitting the morbid poison. In support of the opinion, an instance was adduced in which a surgeon had, on account of the extreme prevalence of phlebitis among his patients, been obliged to desist for a time from operative practice.—*Med. Times and Gaz., Aug. 5, 1854, p. 137.*

ORGANS OF RESPIRATION.

63.—ŒDEMA OF THE GLOTTIS FROM A SCALD OF THE PHARYNX.

By Jolliffe Tufnell, Esq., Surgeon to the City of Dublin Hospital.

[This patient attempted to swallow a very hot potatoe, which stuck in his throat:]

He felt his condition becoming critical, and finding that he could not remove it by his fingers (introduced from the mouth), he grasped the throat with such force as not only dislodged the potato upwards, but broke it into two, and he shot the pieces out upon the floor. Conceiving from the sensation continuing, that a piece still remained in the throat, he came up to have it removed. From his general appearance it was evident that nothing was now there; but to assure him, he was given bread to swallow, and, lastly, had a probang passed. Still, however, he was not quite satisfied, but went away, and continued at his labour throughout the day, returning home at five, p.m. During the afternoon, he felt a "huskiness" in his throat, and some uneasiness about the top of the windpipe. He did not take much notice of it, however, until sitting down to his tea, at six o'clock, when, on attempting to swallow, spasm set in, and he nearly fell from off his chair; he recovered from the immediate suffocation, but felt great distress in breathing, and as if he was being choked; his brother, who was with him, became also greatly alarmed, and they ran to the hospital as quickly as they could.

On arriving there, his situation was urgent, the dyspnœa from spasm, œdema, and the act of running, combined, being extreme. Having been sent for, I arrived in a short time, but his condition was already much changed; the alarm had subsided, and his heart's action became much reduced, the breathing being regular, and (though somewhat obstructed) not such as to demand immediate interference. After removing all anxiety by assurance, I proceeded to examine the throat, and found the epiglottis erect, and the tumefaction extending all round. With a view to reducing the œdema and give vent to the serum effused, I passed down a guarded bistoury and scarified the epiglottis fully, which afforded relief. He was then placed in a well-warmed ward, his feet immersed in a bucket of hot water, and he was desired to inhale steam freely.

Up to nine o'clock he continued doing well, when, on attempting to swallow, spasm again set in, and dyspnœa suddenly returned. He now grew rapidly worse,

the countenance became anxious, and exhibited distress. The pulse, too, became intermittent, and there was no respiration audible beneath the clavicles.

Under these circumstances, I deemed it advisable to operate at once, and obtaining the sanction and approval of my colleague, Dr. Williams, performed laryngotomy by a vertical incision, inserting a full-sized trachea-tube through the crico-thyroid space. The operation presented nothing peculiar. It was performed by placing the patient on his back, and with rather a free incision *completely* exposing the crico-thyroid membrane, by dissection, before opening into it with the knife. This thorough division of the structure, and cleaning of the membrane, was done at Dr. Williams's suggestion, as he stated that he had often seen the branch of the thyroid, the inferior laryngeal artery, which penetrates this space, of such considerable size as, if wounded, to render it necessary that it should be tied, and in this instance his opinion proved correct, for on being divided it bled *very freely until secured.* The vertical opening into the crico-thyroid membrane was rendered sufficient for the introduction of the trachea-tube, by divaricating the edges of the incision by means of an aneurismal needle, inserted on either side. Speedy relief followed the operation; the respiration, previously inaudible with the stethoscope in either lung, was immediately free and forced, and the patient, in a few minutes, jumped from off the table and walked over to his bed.

[The after treatment was directed to prevent brochitis, and on January 16th the canula was withdrawn: on January 20th, the wound had healed, and the patient was discharged.]

This case, then, goes to establish the fact so strongly dwelt on by Mr. Prescott Hewitt, viz., that when we have to operate for the relief of dyspnœa arising from acute affections of the larynx in the adult, it is sufficient to make the opening in the crico-thyroid space.

Mr. Hewitt draws attention to the great practical fact of the wide difference which exists in the condition of the parts in acute affections of the larynx, terminating in effusion, in the adult and in the child: "the effusion in the adult taking place by far most frequently in the submucous tissue, whilst in the child it is usually poured out on the free surface of the mucous membrane. In the adult, then, the effusion is purely laryngeal; in the child it is not only laryngeal, but also tracheal."

This effusion in the submucous tissue of the larynx, we know to be almost invariably limited to the parts above the rima glottidis, and the explanation of this limitation, Mr. Hewitt says, "can easily be found in the anatomical structure of the larynx and trachea, by observing that above the inferior vocal cords the mucous membrane is connected to the subjacent parts by means of a loose cellular tissue, which is very abundant (especially in the region of the aryteno-epiglottic ligaments), whereas the connexions of the mucous membrane lining these cords and the trachea are very firm, the cellular tissue here being very short and very dense, forming so firm a bond of union, that it is difficult to separate the mucous membrane from the parts lying underneath it."

With effusion thus limited to the rima glottidis and parts around it, if dyspnœa should occur, what reason is there for resorting to tracheotomy, when laryngotomy will suffice? The delay, which is now so fatal in these cases, will not be permitted, and precious time will not be wasted in the administration of useless medicines, if experience proves that the comparatively simple operation of laryngotomy, *early performed*, will give relief. The proverb says, delay is dangerous; and it certainly is so here.—*Dublin Hosp. Gazette, Feb.* 15, 1854, *p.* 17.

64.—*Impromptu Tracheotomy.*—It occurs occasionally to the medical practitioner to be called on, unprepared and unfurnished with the proper instruments, to perform the operation of tracheotomy. It may not, therefore, be amiss to state what may be done on such an emergency. A pair of pointed scissors exists in every case of instruments, and in every house. With these, the integuments over the trachea may be divided, and, the points being brought together, the trachea pierced; the points being again duly separated, a horizontal, longitudinal opening is made. If the case be extremely urgent, the handle of a teaspoon may now be inserted, and turned one

quarter round; the opening is then patent, and the patient, if previously struggling for breath, is relieved, perhaps saved.

To render the tracheal orifice permanently patent, a portion of all the tissues surrounding and composing the orifice may be included in a loop of cotton, at four opposite points, and the cotton may be drawn tight, and fastened to a *ring* of wire, tin, whalebone, cane, or even card. If all these are absent, the pieces of cotton may be readily tightened by the operator and an assistant.

In this manner, life may be saved in the case of choking, and in other cases of laryngismus.—*Lancet, Aug.* 26, 1854, *p.* 172.

65.—FOREIGN BODY IN THE TRACHEA OF A CHILD.

Under the care of JOHN HILTON, Esq., F.R.S.

Mr. Liston remarks in the fourth edition of his 'Practical Surgery,' as follows:—"Only one case is, I believe, on record, in which a foreign body was actually discovered in, and removed from the bronchus during life." Since that time, several cases of great interest have been recorded, and, indeed, in the chapter from which the above sentence is quoted, Mr. Liston himself gives the narrative of two others. The instances in which the operation has been performed are still, however, very few, and all additions to our experience in respect to it are very valuable. Before proceeding to the details of the following case, which has just been successfully operated upon by Mr. Hilton, we will take a short glance at the results of a few other cases in which the same procedure was adopted. In the case operated upon by Mr. Liston himself, a large portion of mutton-bone was extracted, six months subsequent to its being swallowed, from the trachea of a middle-aged woman. Before the operation, she had repeatedly suffered from attacks of bronchitis; after it "all the symptoms ceased, and the patient was soon restored to perfect health." A second case was the well-known one of the celebrated engineer Mr. Brunel. The foreign body consisted of a half-sovereign, which had been accidentally allowed to slip into the windpipe. After repeated trials of the inverted posture and other means, without any success, tracheotomy was performed by Sir B. Brodie. The attempts to seize the coin, however, failed, both on the occasion of the operation, and on a second trial five days afterwards. Sixteen days after the operation, and before the wound had healed, the inversion of the body was again practised, and fortunately was successful. In a third case, in which Mr. Dickin was the operator, a bell-button was removed by tracheotomy from the right bronchus of a boy, aged 8, six days after the accident. After a rather sharp bronchitis, the patient recovered perfectly. A fourth case occurred under the care of Mr. Birkett, in Guy's Hospital, nearly two years ago, and may be found recorded in the 'Medical Times and Gazette' for Sept. 10, 1853, p. 268. The patient was an infant aged 1 year and a-half, and the offending body was a portion of a walnut, which had passed into the larynx. The bit of nut had become impacted in the larynx, and had never passed lower; and in this respect the case differed from the preceding, and from the one which is to follow. Mr. Birkett opened the trachea, and endeavoured to dislodge the foreign body by pushing it upwards; his attempts, however, failed at that time, but another trial made two days afterwards was successful. The infant recovered and the wound closed. Mr. Liston mentions a very instructive case, in which the inverting of the body, and at the same time subjecting it to smart shaking, was successful in dislodging a small bullet which had slipped into the trachea. It may be concluded, then, that in all cases in which the size of the body swallowed is not so large as to preclude the probability of its expulsion, the first measure tried should be the shaking of the patient's body with the head in a depending position. If, after repeated trials, this plan fail, the trachea should be opened, (not by a mere slit, but a valve-like aperture of considerable size,) and extraction attempted. Should the attempt at extraction fail, it may be well to try again inversion of the body, the wound in the trachea being kept freely patent meanwhile. The inversion plan, previous to tracheotomy, fails sometimes, on account of the suffocative dyspnœa caused whenever the foreign body falls into the glottis; but when a false opening has been made, this inconvenience is quite remedied.

[Is it justifiable to remove a foreign body from the trachea when no urgent symptoms exist,—the general opinion is in the affirmative. In this case, in a little girl 8 years old, a small glass-bead passed into the trachea on May 3rd.]

It was found that the expansion of the two sides of the chest was never equal, and that respiration was always much less distinct in one than in the other lung. The deficiency, which was almost complete, would affect one side for a time, and then after a coughing effort would change to the other. The side from which the sound was for a time wanting did not fill during inspiration, and its intercostal spaces, instead of bulging, became much depressed. It was judged probable from these signs, that there was some loose body lying about the bifurcation of the trachea, which alternately plugged its divisions.

Operation, 2 p. m.—The child having been placed under the influence of chloroform, and the head thrown back, Mr. Hilton made a longitudinal incision through the soft parts over the trachea, by which several large tortuous veins were of necessity divided. The bleeding which resulted from the division of these, was arrested by means of cold, but the edges of the divided isthmus of the thyroid gland required to be touched with lunar caustic. The bleeding having ceased, the next step consisted in seizing the right side of the trachea, by means of a tenaculum, with which Mr. Cock, who was assisting, held the tube forwards, and over to the right side. Mr. Hilton now introduced a second tenaculum into the left side of the trachea, and with a bistoury divided four or five of the rings of the latter in such a manner as to allow of a valve-like flap to be lifted upwards. The opening being thus kept widely patent, a short time was allowed to elapse, in the hope that the foreign body might be floated up. These expectations were not disappointed, for after a few rather violent attempts at coughing, a large mass of mucus, in which the bead was enveloped, floated out into the wound, and was safely removed. The breathing became quiet immediately after the expulsion of the offending body. The flap of the trachea was carefully replaced, but no attempt was made to close the external wound, which was simply covered with a fold of wet lint; a saline antimonial draught was ordered to be given three times daily.

[On the 29th May she was discharged cured. Mr. Hilton draws attention to the following points:]

1. The importance in such cases of forming a correct and exact diagnosis previous to undertaking an operation. In the present one, the history of the accident, and the symptoms which had followed it, pointed conclusively to the presence of a foreign body in the trachea. The size and shape of the body supposed to have been inspired had been pretty clearly ascertained by getting the patient, with the assistance of her father, to draw it on paper. That it was moveable, and had not become impacted in either bronchus, was shown by the attacks of dyspnœa, during which it had probably passed upwards into the glottis, and also by the alternation from lung to lung of the loss of respiratory sounds. (See above.) 2. The necessity for speedy resort to operation in such cases, without delaying, in the hope of the spontaneous expulsion of the offending body. The inexpediency of losing time by waiting was grounded on the knowledge that the aperture of the glottis is always much larger during inspiration than during expiration, so that there is no good reason for expecting, that because a body has found its way through the aperture inwards, it will also be able to do the same outwards. 3. The importance of making a large opening into the trachea. The size of the opening must, of course, be in proportion to that of the body to be removed, which it should always much exceed. The size, shape, and smoothness of the foreign body in the present case would, in all probability, have made it extremely difficult to extract it, had not the opening been sufficiently large to permit of its easy escape. To have used forceps would have involved the risk of breaking it, while from its polished exterior and rounded form, it would not have been easily seized.—*Med. Times and Gazette, Aug.* 5, 1854, *p.* 136.

ALIMENTARY CANAL.

66.—ON MALIGNANT STRICTURE OF THE ŒSOPHAGUS.

By Dr. Risdon Bennett.

[The following case is chiefly valuable on account of the diagnosis of this disease. Before the use of the microscope this case would probably have been classed among simple fibrous strictures.]

James Sessions, a wine-cooper, of intemperate habits, aged 51, was admitted on January 16. He complained of having for some months been losing flesh and strength, and stated, that two years ago he had spit up a little blood. He had no cough or difficulty of breathing. On auscultation, a little harshness was noticed to attend the respiratory sounds over the left apex, but there were no marked signs of disease. He looked ill, was pale and thin, and his eye had the peculiar clear appearance which so often denotes pulmonary phthisis. The cod-liver oil was accordingly ordered, and a stomachic mixture, to be taken together three times daily.

On February 2, he mentioned casually another symptom, namely, that for two months past he had not been able to swallow solids. On further questioning, it appeared that he had long felt a sense of obstruction in the upper part of the throat, though quite unattended by pain or by smarting in the act of swallowing. He had been losing his voice. No tumour could be felt externally. His breath was fetid, and there was a continual flow of saliva into the mouth, by which the teeth had become coated. As a concentrated form of nutriment, the cod-liver oil was still continued.

From this date he got rapidly worse. The loss of voice became more complete. He spoke in a very peculiar manner, always holding the head motionless when doing so, and enunciating very deliberately; the sound produced was a low, soft whisper, quite unattended by hoarseness or any laryngeal twang. Several possible causes were suggested, and the exact diagnosis of the disease was not easy. The fetor of the breath was quite enough to indicate the possible existence of a pharyngeal abscess connected with diseased bone. There might be an aneurism pressing on the œsophagus, or the source of obstruction might be a stricture of that canal. The last supposition was deemed the most probable. On March 16, Mr. Hilton, as Surgeon to the Hospital for Diseases of the Chest, saw the patient, in consultation with Dr. Bennett, and the question was raised as to the propriety of attempting mechanical dilatation. Having examined the case carefully, Mr. Hilton took the diagnosis a step further, and also expressed himself as decidedly averse to instrumental interference. He believed that the disease was stricture by a malignant growth, this opinion being founded on the circumstance that the voice was lost. Had it been an aneurism pressing on the trachea or nerve trunks supplying the larynx, the voice should have been stridulous, and paroxysmal attacks of suffocative dyspnœa should have occurred. Were it an abscess, there could be no reason why the voice should be permanently lost. Were it a simple contraction or stricture of the œsophagus, the voice, again, would not suffer; but were it supposed to be malignant growth around the latter tube, it would in all probability, without stretching them, involve in its growth the recurrent laryngeal nerves, and produce exactly the condition present (paralytic aphonia). It must be remembered, that this opinion was given although there was no thickening whatever perceptible on external examination. The impropriety of attempting to force a malignant stricture was, of course, apparent; and Mr. Hilton advised, that, on the contrary, the man should not even try to swallow solids, but content himself with taking large quantities of bland fluid nutriment. Blisters were also to be applied around the throat, in the hope of diminishing the vascularity and tumefaction of the growth.

[The patient was transferred to St. Thomas's Hospital, where he died.]

The *post-mortem* examination was made by Dr. Bristowe, from whose notes we condense the following account.—A tract of the œsophagus, about two inches long, and extending downwards from about an inch and a half below the commencement of the tube, was involved in stricture. The mucous membrane of this part was destroyed, and the internal surface was rugged, slightly nodulated, of a dusky, livid colour, and covered with a dirty secretion. Excepting at the commencement of the

diseased tract, the calibre of the tube did not appear to have been lessened, as a considerable amount of hollowing out seemed to have been effected by ulceration. On section, the muscular layer of the œsophagus might be distinctly seen, having on each side of it a band of greyish-white deposit, a line or two in thickness. The tissues were firmly incorporated by this deposit, but in no part were there any large masses of new growth. The diseased tract of the œsophagus adhered to the trachea, but neither the latter nor the larynx were in any way diseased. External to the trachea, on its posterior surface, there were numerous small whitish nodules, about the size of pins' heads. In the submucous tissue of the anterior wall of the œsophagus was a narrow band of deposit, which extended from the lower edge of the ulceration, about two inches down. It was nodulated, and very firm. The mucous membrane, which adhered to it, but was not ulcerated, was raised by it into a ridge of about the eighth of an inch in depth. Dr. Bristowe made careful examination with the microscope of the juice scraped from many parts of the cut surface of the diseased tract. Among many epithelial scales, constituents of the broken down tissue and of inflammatory deposit, there was observed a sufficient number of well-characterised laminated or encapsuled cells, to make it certain that the disease was epithelial cancer. No secondary deposits were found, but some of the neighbouring lymphatic glands were slightly enlarged. With the exception of the lungs, there was nothing worthy of note observed in any of the other viscera. The apex of the left lung was indurated and presented puckered patches, as if from the cicatrization of old disease; no remains of tubercle could, however, be found, nor were there any cavities. In the apex of the right lung, there were also some local indurations, and, in addition, a few small cavities, each about capable of holding a horse-bean. These cavities were free from secretion, and were lined by a smooth false membrane. There was no deposit either in their walls or in other parts of the lung. No cretaceous matters could be found in any part of the pulmonary tissue. The œsophagus and trachea were removed, to form a preparation; but a few days afterwards, Dr. Bristowe made a dissection of the parts, in order to determine the relations which the recurrent laryngeal nerves had sustained to the diseased growth. The left nerve was found atrophied, and, indeed, quite lost in the cancerous mass, but the ends of the right one had been cut away so closely that they could not be found; from the relative position of parts there could be little doubt, however, that its trunk was likewise destroyed. Dr. Bristowe examined microscopically the fibres of the muscles of the larynx supplied by these nerves, and found them universally in so advanced a stage of granular degeneration, that scarcely any trace of transverse striation was visible. In proof that this change was not *post-mortem*, some portions of neighbouring muscles were examined also, and in these the normal structure was found still to exist in great perfection.

The above case offers some very interesting points for consideration.

1st. *The Symptoms of Œsophageal Stricture.*—Under this head we would ask attention to one which has not been more than casually alluded to by authors, although exactly what might be, *à priori*, expected. We refer to the difficulty in swallowing the saliva. In this case it was a very prominent one; the man always appeared as if under mercurial influence, the fluid running over the corners of his mouth, the teeth coated and in a loathsome condition. With respect to the diagnosis of the exact nature of the stricture we have already said enough in the narrative of the case.

2d. *Character of Aphonia from Paralysis.*—A better example of simple and complete paralysis of the laryngeal muscles could perhaps scarcely be found. There was no disease whatever of the larynx itself, but, by destruction of the recurrent trunks, all supply of nervous influence to its muscles had been cut off. The symptoms, as we have seen, exactly correspond with what should be anticipated from this condition; there was simple loss of the laryngeal element in the voice. The nerve-trunks had not been stretched or irritated, but gradually involved and destroyed; there had been, consequently, no attacks of spasm or of stridor.

3d. *The Conjunction of Cancer with Retrograde Phthisis.*—An indisputable example of the coincidence of active tubercular disease with cancer in the same stage is probably yet wanting. Many have, however, been observed like the present, in which, in patients dead of cancer, the remains of old tubercular disease have been found. It is interesting to note how complete the arrest had been, not a single trace

of tubercle being discovered, although the cicatrices and small excavations proved that it had been there. The previous history of the case showed, that at the period when probably the phthisis was active, the man scarcely knew himself to be in ill health, although, on one occasion, he had spit a little blood. A patient recently left one of Dr. Bennett's wards, in St. Thomas's, who had evidences of a considerable cavity in one lung, and was believed also to suffer from cancer of the uterus. Whether the pulmonary mischief might not also be cancerous, is, of course, open to question, and is, perhaps, the most probable supposition. At page 319 of the 'Medical Times and Gazette,' for March 26, 1853, a very instructive case may be found, in which, during life, a woman was believed, by many competent observers, to suffer from a conjunction of active phthisis with scirrhus of the breast. The *post-mortem* appearances, to the naked eye, were also deceptive, but the microscope proved that what had been mistaken for tubercle in the lung was in reality cancerous deposit.—*Medical Times and Gazette, June* 10, 1854, *p.* 595.

67.—*Anodyne Cement for rendering Teeth insensible to Pain.*—Mr. J. Paterson Clark, of London, with the object of rendering diseased teeth insensible to pain, previous to the operation of stopping them permanently with metal, or filing away the carious parts, recommends the application of a paste, composed of Canada balsam and slaked lime, which is to be inserted into the hollow tooth like a pill. Mr. Clark states, that this preparation affords immediate relief in all but chronic cases of inflammation. He says:—"If fresh pills be inserted as often as the old ones wear out, or are removed on the return of pain, in order, like an abscess, to allow the escape of matter or blood, and this practice be continued, the teeth will become as insensible to touch as the soundest, and may then be permanently stopped, and otherwise treated in the usual way, without the infliction of pain."—*Dublin Hos. Gaz., June* 1, 1854, *p.* 141.

68.—*Emetics in Toothache.*—M. Cæsar Fredericq, of Ghent, has directed attention to a variety of toothache which is indicative of a disordered state of the stomach, and which he has succeeded in relieving by the administration of ipecacuanha in emetic doses. He has seen this treatment succeed where everything else had failed, and where even the removal of the diseased tooth produced no effect.—*Lancet, July* 15, 1854, *p.* 34.

69.—ON STRANGULATED HERNIA.

By Prescott Hewett, Esq., Assistant Surgeon to St. George's Hospital.

[It is too true that the desire to reduce cases of strangulated hernia has too often induced the surgeon to prolong his attempts till too late to benefit his patient by an operation.]

Amid the various improvements so strongly urged in the present day for the treatment of strangulated hernia, none, I feel confident, would be greater than that of resorting to an earlier operation. Every case of this nature must be necessarily judged of by itself; but it would be well were a general rule laid down, that, after a fair trial of the taxis, with all necessary appliances, the operation should at once be resorted to; and especially should this rule be applied to cases of femoral hernia, which, among our operations, are by far the most common. Were such a plan once adopted generally, the statistics of operations for strangulated hernia at our different hospitals would, without doubt, be very much more satisfactory than they are at present.

An operation having been decided upon, the next question of importance is,—Whether the sac is to be opened or not? To this subject great attention has of late been devoted, and admirable are the essays which have been written upon it. At some of the metropolitan hospitals the present rule is, *cæteris paribus*, not to open the sac. Such is the rule at the London Hospital, and most satisfactory are the results in those cases where this plan was carried out; but, taking the aggregate

number of operations and the deaths in Mr. Nathaniel Ward's memoir, the results on the whole, although very favourable, are certainly not more so than they are at St. George's Hospital, where the practice, as to this point, differs widely from that of the London Hospital. At St. George's the rule is, to open the sac freely. The numbers in the present analysis from the cases at St. George's are 78 operations, in 75 of which the sac was freely opened, and in the whole of these operations there were 19 deaths. The numbers from the London Hospital, given in Mr. Ward's memoir, are 69 operations, and 21 deaths, the sac not having been opened in a little less than one-half of the sixty-nine cases. I have no wish to make the returns from St. George's appear better than they really are; but still it is right, I think, to mention that in the nineteen cases of death, the fatal result may fairly be attributed to other causes, not necessarily connected with the operation, in at any rate six of them.

The following were the *post-mortem* appearances observed in these six cases:—1. Granular kidneys and pneumonia, peritonitis, intestine dark coloured, with patches of slate colour, and very soft. 2. A young woman who had been confined shortly before the operation, the uterus was large and soft, with some remnants of sloughy placenta at the fundus, peritonitis, intestine slightly discoloured. 3. Died comatose a few hours after the operation; disease of the brain, peritonitis, intestine slightly discoloured. 4. Acute bronchitis, which existed before the operation, every symptom connected with the hernia being favourable. 5. Just recovering from a severe attack of erysipelas; well marked symptoms of peritonitis previous to the operation. 6. Lived forty-one days after the operation, and appeared to sink ultimately from exhaustion, brought on by extensive sloughing connected with large bed sores.

The number of operations and of deaths from the two hospitals, although small, prove, at any rate, I think, that the fatal result usually observed in these cases does not depend, as has been said, so much upon the opening of the sac, as upon other causes which have not been duly considered. There is no doubt that if cases of this nature were seen at a much earlier period than they commonly are at our hospitals, and if protracted efforts at reduction were more generally given up, there is no doubt, I say, that then the rule of not opening the sac would find many more advocates.

The last point I shall touch upon, is that of purgatives after the operation. Not many years ago, it was very generally the practice, immediately after an operation of this nature, to give at once some aperient medicine; and that which I then saw most frequently used, was Epsom salts and infusion of roses, every three or four hours, until the bowels acted, a dose of calomel having sometimes been previously given. I must say, however, from all I have seen, that I cannot help thinking that repeated doses of salts are much more likely to do harm than good; in fact, so decided is my opinion, on this point, that I have, of late years, never prescribed this purgative in such cases. As far as my own experience is concerned, I am decidedly of opinion, that generally it is very much better not to be anxious as to the early action of the bowels, and to give as little medicine as possible. Repeatedly, of late years, I have seen in the wards of St. George's Hospital, cases of strangulated hernia in which the bowels were not open for four or five days after the operation. These cases were left to themselves; the bowels acted of their own accord, and the patients did well. Far from soliciting the action of the bowels at an early period, I have seen large doses of opium given in many instances immediately after the operation, and that with decided benefit. Should it become necessary to administer a purgative, castor oil, with or without a mercurial, according to circumstances, is, I think, on the whole, the best remedy.

The wise plan, however, in these as in all other cases, is for the surgeon not to act on any particular plan, but to treat each and every case of strangulated hernia in all its phases, according to its own particular symptoms.—*Med. Times and Gaz., Sep.* 23, 1854, *p.* 315.

70.—*Mr. Luke's Strangulation Test for Hernia.* If the body of the hernial tumour be compressed by the hand, an impulse is communicated to all its parts below the seat of stricture; but, if the neck of the hernia be grasped between the finger and thumb of the other hand, above the stricture, while such compression is

made, there will not be any impulse felt. When, in the commencement of the examination, the neck of the tumour is first grasped, we may be always assured, that if an impulse is felt on compression of the tumour itself, the seat of stricture is nearer the abdomen; and, by gradually drawing the finger and thumb in that direction, while renewed compression of the tumour is made, a point will soon be reached at which impulse ceases to be felt. The point at which impulse first ceases to be felt is the seat of stricture. In like manner, if an impulse is not felt when the neck of the tumour is first grasped, we may be equally assured that the stricture is situated nearer to the body of the hernia; and, by a like gradual approximation to it with the finger and thumb, an impulse shortly commences to be felt. The point where the impulse commences to be felt is the uppermost part of the strangulated contents, which implies that the stricture is immediately above it; and, on inquiry, it will be found to correspond with the indications of an examination commenced from below." The value of acting on the indications of this simple proceeding when its application is practicable is at once apparent, as unnecessary division of the soft parts is thus avoided.—*Med. Times and Gazette, July* 29, 1854, *p.* 106.

71.—*Opium as an Adjuvant in the Reduction of Hernia.* By RICHARD HEY, Esq., York.—[Mr. Hey thinks highly of opium in these cases, but differs from some previous writers as to the mode of using it. He says:]

It is of the last importance that a prompt and decided effect should be produced. You must, as it were, take the system by surprise. In cases especially where the hernia is small and the stricture very tight, adhesions form very quickly, particularly if roughly handled; or the bowel becomes œdematous and the reduction by the taxis is all but impossible. It is surprising what tolerance of opium the system seems to acquire in these cases, and I thing its efficacy in "relaxing fibre" is much enhanced by its combination with emetic tartar, without, as far as my experience goes, increasing the tendency to vomit. As I wish to trespass as little as possible on your valuable space, I will conclude with a brief record of a couple of cases, by way of illustration. One occurred in an old man of seventy; the other in a person about fifty years younger.

Case 1. J. B., aged about 70, an old mail-coachman, was admitted into the York County Hospital under my care. He had very large scrotal hernia, which was strangulated. The scrotum was very greatly distended and painful, particularly when handled. He had been once before relieved from the same condition at the hospital, by means of large depletion, &c., leaving him in a very weak state. As any attempt at reduction by taxis gave him great pain, and I wished to save his strength, I at once prescribed the following in a pill:—Morphiæ muriatis; antimon. potass.-tartrat., aa gr. i. M.—To be taken immediately; and a quarter of a grain of each every fifteen minutes for four doses. He was then put into a warm bath, and in ten minutes the hernia was reduced. This man was very slightly narcotised; and on the following day he was quite well.

Case 2. R. L., about 22 years of age, was under the care of Mr. Edward Allen, with small inguinal hernia, which had been strangulated several hours. He had vomiting, &c. The taxis had been tried, and also repeated with the accompaniment of chloroform, but without success. I saw him in consultation about 10 p. m. I again put him under the full influence of chloroform, and gave the taxis again a fair trial, but with no better success.

As it was now late, and the vomiting had in some degree subsided, I recommended the following medicine to be given, failing which, I proposed to operate at eight in the morning.—℞ Morphiæ muriatis; antimon. potass. tartrat. aa gr. ii; conf. rosæ q. s. M. Fiant pilulæ iv., capiat i. omni quaque semihora. The four pills were taken as directed; and in the morning when the patient was first visited, the gut was found to have spontaneously retired. The young man did not suffer any inconvenience whatever from the medicine.—*Association Med. Journal, July* 7, 1854, *p.* 608.

72.—*On Laceration of the External Abdominal Ring in the Reduction of*

Hernia.—A new method of reducing inguinal hernia has been invented by M. Seutin of Brussels. He proposes that the external abdominal ring should be enlarged by being lacerated under the skin by the index finger. At first sight, it may appear by no means easy to lacerate at pleasure the external pillar of the ring with the index finger, acting through the thickness of the inverted integuments. Secondly, it may be asked how could the finger be introduced between the border of the pillar and the knuckle of the intestine, when the latter was distended and constricted at its superior part? And, lastly, it cannot be forgotten, that according to the generally accepted doctrine, the external ring is by no means the most frequent agent of strangulation in inguinal hernia, but rather the neck of the sac, or the superior orifice of the canal (internal abdominal ring).

Excellent, however, in theory as these several objections may be, it is impossible to abstain from noticing the particular circumstances which have been collected respecting the practice in question, in addition to the published accounts, and confident declarations of its efficacy put forward by its discoverer.

It has been established beyond a doubt, by experiment on the dead subject, that the external abdominal ring may be easily enlarged without any operation, that is to say, without section of the skin, by means of simple traction by the finger.

Nor does the possibility of reducing strangulated hernia immediately after the rupture of the ring appear any longer doubtful. In ten cases in which the taxis had failed, M. Seutin ruptured the external pillar of the ring, and immediately the reduction of the protruding intestine was effected. We are unable to say whether the laceration is prolonged through the length of the canal. Certain, however, it is, that the resistance offered by the hernia did not prevent the insinuation of the extremity of the index finger into the canal, and the laceration effected was sufficient to render reduction of the intestine possible. In face of these incontestable facts, it is impossible to pass over the method in silence, although at present it may be premature to express any opinion in its favour. Evidently it cannot be applicable to all degrees and all periods of hernia, for it would expose an intestine highly inflamed, or on the brink of gangrene, to the risk of rupture; but, nevertheless, it may have its circle of application, and surgeons will do well perhaps not to condemn it prematurely.—*Gaz. Hebdom.—Med. Times and Gazette, Sept.* 2, 1854, *p.* 246.

73.—*On the Propriety of Opening the Bowels soon after Operation for Hernia.* By Joseph S. Gamgee, Esq.—[After a long and interesting paper on this subject, Mr. Gamgee comes to the following conclusions in cases where the operation of herniotomy is not followed by spontaneous action of the bowels.]

1. When the condition of the gut is good, and there is little or no peritonitis, an oleaginous enema should be given an hour or two after the operation, and repeated after three or four hours in case of failure, or a purgative exhibited by the mouth.

2. When the peritoneal inflammation is intense, even though the bowels be in a fair condition, antiphlogistics must be perseveringly employed; and though a simple enema may be given in the first six hours, it is advisable to excite the action of the bowels until the next day, either by more active enemata, or purgatives by the mouth.

3. In the case of mortification threatening the gut, the bowels should be kept quiet by opium, and purgatives and enemata abstained from until the danger of perforation has passed.

These rules promise to be faithful guides for practice, inasmuch as they are in conformity with sound doctrine, and so far as I am aware, opposed by no facts; but the great rule is, to observe rigorously the symptoms of each particular case, to study its indications and aid nature with the lights which doctrine and practice reflect, and not to pretend to act according to systematic rules based upon speculations, in cases which present infinite varieties, according to the age and constitution of the patient; the duration, size, and position of the hernia; the degree and duration of the strangulation; the condition of the intestine; the presence or absence of inflammation; and other more or less important conditions. In truth, just as there is reason for dissenting from those who systematically oppose purgatives after herniotomy, there is reason for not placing implicit faith in the teaching of those who

universally recommend them. If there be any class of cases in medicine and surgery—and there are many—in which systems are injurious, and in which each case requires to be studied of itself with the light of reason and experience, that class is preëminently the one which comprises cases of strangulated hernia.—*Asso. Journal, June* 30, 1854, *p.* 576.

74.—ON FISTULA IN ANO.

By JOHN HAMILTON, Esq., Surgeon to Richmond Hospital, Dublin.

[Mr. Hamilton relates a case of fistula in ano, on which he makes some remarks of a general nature that are truly practical and good. The symptoms of the patient, W. Fry, aged 38, were nearly of the usual character.]

A probe can be passed through the fistula into the rectum; its point is felt a little above the internal sphincter. The external opening is about one-third of an inch from the extremity of the rectum.

He is otherwise healthy, and has a healthy aspect; but he has been a hard drinker. The only cause he can assign for the present complaint is, a very costive state of the bowels.

He was ordered a little grey powder at night and a castor oil draught in the morning. The bowels were well moved.

Feb. 19th.—Mr. Hamilton passed a slightly curved director through the fistulous opening, and brought its point out through the anus. By a sharp bistoury, passed along the director, the parts between the two openings, including the sphincter, were divided; a piece of dry lint was placed between the lips of the incision.

The bowels were afterwards kept gently open by mild laxatives, and the wound was dressed from the bottom with dry lint.

March 1st.—It is now the twelfth day since the operation; the wound is so nearly well that he is discharged.

If an abscess by the side of the anus is left to itself, after more or less suffering, it breaks, discharges its contents, and the patient experiences much relief. But the abscess generally will not close; the repeated action of the sphincter, in passing fæces and wind, prevents it by breaking up the adhesions between its sides as fast as they are formed, and it becomes a *fistula in ano*, that is, a small narrow passage communicating with a suppurating cavity at the side of the gut. Perhaps even a more powerful cause is, the extent of the cavity compared to the small opening, giving such imperfect exit to its contents, and constantly keeping it more or less distended.

This is the simplest form of blind fistula. The ease of the patient is not long; the fistula, which at first discharged freely, gradually ceases to do so; then pain is felt, with a return of swelling and general disturbance; this is again relieved by the abscess bursting through the old fistula, or making a new opening. In short, the matter being secreted faster than the small fistula can give exit to it, accumulates, and seeking some other outlet, burrows in the loose cellular tissue under the skin at the side of the anus, sometimes backwards towards the coccyx, sometimes forwards to the perineum and scrotum, and sometimes outwards to the buttock, and downwards to the thigh; you will, in fact, in old cases, be surprised at the number and extent of these fistulæ, and their distance from the anus. When there is only one fistula, and in rather an early stage, you will perceive but little redness or fulness at the side of the anus, but at one spot, a little raised, red papilla; this is the opening of the fistula; and is sometimes so small as to be with difficulty discovered, and scarcely to admit the aid of the probe. In old cases you will see many of these with a puckered appearance of the surface, in some parts red, in others whitish and glossy, with induration or callosity. In these old and extensive fistulæ, when you introduce a probe into one of the leading sinuses, you will be almost alarmed at the depth it goes up along the side of the rectum, the walls of which are bared, so that when you pass your finger through the anus you can readily feel the probe through the thin wall of the bowel, which is sometimes nearly isolated all round; the very loose structure of the cellular tissue here leading to this.

Now, if the abscess, instead of breaking externally, does so internally, the opening

will in general be found only a short distance within the anus, just above the internal sphincter; and the patient, after the breaking of the abscess, will be relieved for a time from the disagreeable sensations he previously felt, but not for so long a period as in the former case; wind, the secretions of the rectum, or some liquid, or small portions of solid fæcal matter get through the opening into the cavity of the abscess, and keep up pain, irritation, and suppuration. There is in this case more irritation of the rectum, and the patient has more frequent calls to stool, and not perfect relief after a motion, which will be observed to contain pus, and often blood. These are troublesome cases, and when the abscess is small, often obscure.

A medical friend of mine consulted me for uneasiness and weight about the anus; this uneasiness almost amounting to pain when he passed a motion; and he had observed a slight moisture. He was out of health, irritable, and low-spirited. There was no redness, but at the right side of the anus there was deep-seated hardness, which was very tender, causing him to wince on pressure. When the gorget speculum was introduced, a small papilla was seen just within the anus, at the same side, from which matter oozed; through this the point of a hooked probe could be passed and felt under the skin externally.

In either of the forms now described, if the disease is left to itself, it rarely ever gets well; but, on the contrary, as I have already said, fresh abscesses form, the matter burrows in every direction, and opens into different places, leaving fresh fistulæ, for many of the openings do not close. If in the beginning you have a blind external fistula opening by the side of the anus, the sinus or cavity of the abscess, sooner or later, after running up the side of the rectum, opens into it, and thus you have a "complete *fistula in ano*." So when you already have a blind internal fistula, with an opening just above the internal sphincter, it is usually not long before this works its way out by the side of the anus, and you have a complete fistula formed. This is known by not only matter, but air, coming through the fistula, when the patient passes fæces or wind; fæcal matter, however, rarely comes through, and even in the most complete and direct fistula the patient is often quite unconscious of the passage of wind. It is not always easy, by examination, to satisfy yourself whether the fistula is complete. You pass your probe into the fistula up by the side of the rectum, and generally much above the internal fistula; you can feel the probe through the isolated wall of the rectum, but you can detect no opening; try just above the internal sphincter, about an inch from the anus, and you will probably at length hit off the opening, and the probe will pass into the bowel. As the discovery of the opening renders the operation more easy and more effectual, we should not hastily abandon our efforts to find it. M. Ribes has, perhaps, studied this subject with the greatest care. He examined seventy-five bodies, and found the internal orifice to be "most commonly a little above the place where the union is effected between the membrane lining the rectum and the external skin, sometimes also a little higher up: but the opening is never situated above five or six lines high," at least in any one of the seventy-five dead subjects in which he found the fistula. He says, that "the opening itself appeared as if ragged or torn; in the greater number it was soft, but hard and callous in a few."

The precise course run by the fistulæ was a little variable; in many cases he had seen them, after having taken their origin in the interior of the rectum, descend between its mucous membrane and the internal sphincter muscle. When they have arrived at the inferior part of this muscle, they take a detour and pass between the external sphincter and the skin, and proceed to form an opening in the neighbourhood of the margin of the anus. In many other cases the fistula passed through the fibres of the internal sphincter muscle, descended immediately between it and the longitudinal fleshy fibres of the rectum, bore down on the superior face of the external sphincter, passed through its fibres, and then proceeded to ulcerate its way through the skin, at a greater or less distance from the inferior extremity of the rectum. But in a sufficiently large number of the subjects the parts through which the fistulæ made their way were so much disorganized and confounded together, that it became impossible to determine their precise course. In all these subjects, the course of the fistula was lined by a membrane, extremely thin, smooth, and uniform in its texture, and seemingly destitute of any villosity. He adds an opinion, in which my experience will not allow me to coincide. "The result of these researches has convinced me, that in nearly all cases the fistula is effected in a pile,

and sometimes that its passage is determined by some vein of the hemorrhoidal plexus."

In making an examination, therefore, for the internal orifice, we might almost detect it with our eyes, by separating the sides of the anus sufficiently, but if not, while exploring with a probe we should not go too high. It should also be borne in mind, that from the looseness of the textures by the side of the rectum, a little force will drive the point of the probe readily on, and while following a false route we might think we were still in the fistulæ. You will find, too, that you will often have to curve the probe considerably, the straight probe not following so readily the tortuous course of the fistula. Cases present themselves where the external opening is at some distance from the anus, the fistulæ burrowing horizontally under the skin towards the rectum. Slitting up this fistulous passage materially assists in hitting off the internal opening, which we had previously failed in finding; the probe being thus allowed a greater freedom of movement.

In the treatment of fistula you will first have to consider whether the patient has any serious internal disease, because in the latter stages of phthisis abscess by the side of the anus ending in fistula is not uncommon. Now the existence of the fistula may be of use as a derivative in delaying the fatal progress of the disease, and, therefore, no good surgeon would think of operating, unless when the pain and annoyance are unusually great.

In other diseases of the lungs, heart, liver, or womb, or when there is caries of the sacrum, we must refrain from cutting the patient. In the last case, till the carious bone takes on a healthy action, or where there is a piece of loose dead bone, till it is removed, operative proceedings are useless. Where none of these contra-indications exist, the disease being a purely local affection, we resort to the operation, which is generally the only means of cure; it will vary according to the kind of fistula we have to deal with.

When the fistula is simple and complete, the internal opening is readily found, and not a great distance of parts between this and the external opening, the operation is very easy. Pass a director through the fistula into the rectum, and where the internal aperture is near the anus, it can be passed out through it, by gently curving the director; along the director a probe-pointed bistoury, which divides the parts between two openings, including the sphincters. A piece of lint introduced to the bottom of the wound soon stops the bleeding; it should not be removed till forty-eight hours. The wound may afterwards be dressed with lint, and when granulating be touched with a solution of the nitrate of silver, a drachm to the ounce of distilled water; and, if indolent, the lint may be wet with a lotion of the sulphate of zinc, a grain to the ounce of water, with the addition of a little spirit of wine. It usually takes three weeks or a month to heal completely. During this time the bowels should be kept open by the daily use of the sulphur and cream of tartar electuary, with an occasional mild aperient, infusion of senna with epsom salts and manna, the compound infusion of roses with the same, or castor oil.

I have already alluded to the case where, though the internal opening is near the anus, yet a cavity exists much higher up, denuding the walls of the rectum. I believe it is not necessary to alter the operation on this account; the case is slower in getting well after the operation, but subsequent to the division of the parts below the internal opening, the walls of the cavity above become altered, healthy granulations cover their surfaces, which coalesce, contract, and the cavity becomes obliterated. It is very important to be decided on this point, because the division of the rectum high up is so dangerous, the vessels, both veins and arteries, being large, and the bleeding from them is consequently copious and hard to stop. Larrey, Sabatier, Ribes, Brodie, and Syme, are agreed in this opinion.

The operation is much more serious where a large thickness of parts exists between the two openings. You must expect smart bleeding, the veins at the lower end of the rectum are naturally large, and from the long-continued diseased action the arteries become so too. When the finger is introduced into the rectum, the diseased side feels quite hot, and a large artery can often be perceived pulsating strongly there. Should this come across the edge of the knife, and be divided, it will of course bleed furiously, and, as the part is solid and condensed by inflammation, the bleeding vessel does not retract, nor can it usually be drawn out by the tenaculum and ligatured. No one should despise this danger of hemorrhage.

In blind internal fistula it is best to make it a complete fistula, by making an opening at the side of the anus, in the hard tender red part, which, in such cases, usually points out the seat of the abscess communicating with the internal opening, and then operates as for complete fistula. When the fistula has been very small, and any indications of its existence at the side of the anus not well marked, and the internal opening close to the anus, I have passed a probe, curved like a hook, through the opening, till its point could be felt through the skin, so that I could readily cut down in it, and, afterwards, rapidly completed the remaining steps of the operation.

In blind external fistula the operation is simple enough. A probe bistoury, with the front of the end a little sharp and projecting, is passed up the fistula till its point presses against the wall of the rectum, the nail of the fore-finger in the rectum scratches against the end of the bistoury till it pierces the bowel, and then is drawn downwards, with the finger on its end, dividing the sphincter and all the parts below it.

When there is double fistula, it is best to operate on one side first, and a week or ten days after on the other; in this way incontinence of fæces is avoided, which, though temporary, is a very distressing and disgusting affection. Where several fistulæ lead into one, after the division of the main fistula, the others should be laid open into it. This is most essential; many of the cases which come to the hospital, when operations have been already ineffectually performed, owe their want of success to its having been neglected.—*Dublin Hosp. Gazette, April* 1, 1854, *p.* 68.

75.—*Diagnosis of Fistula in Ano by means of Iodine.*—M. Limauge mentions, in the 'Archives Belges de Med. Milit.,' that in order to find out whether the fistula in ano of a patient of his had an internal opening, he injected pure tincture of iodine into the fistulous tract, at the external opening, whilst his finger was in the rectum. In withdrawing the former, he saw upon it the usual stain of iodine, and he could judge, by the portion of finger stained, how high the opening was situated. This means of diagnosis is certainly of some value when the ordinary modes of investigation fail to clear up the case, and it is preferable to allowing fluid to regurgitate from the anus by the same kind of injection, as the internal fistulous opening may be so small as not to let any fluid pass through it.—*Lancet, Aug.* 12, 1854, *p.* 122.

76.—ON THE TREATMENT OF HEMORRHOIDAL TUMOURS BY CAUTERIZATION.

By Dr. Alphonse Amussat, Jun.

[Whenever hemorrhoids are of long standing, voluminous, ulcerated, accompanied by prolapse of the mucous membrane, or with hemorrhage, Dr. Amussat does not hesitate to remove one or more, according to circumstances, by means of cauterization.]

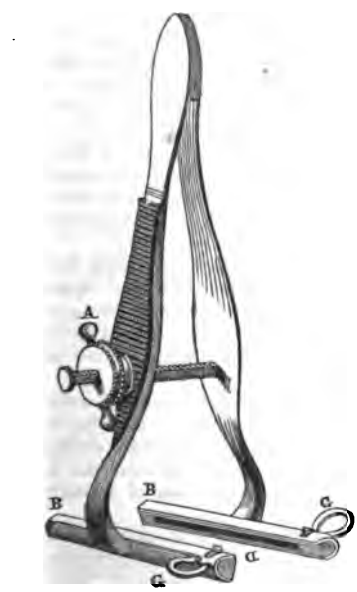
Since the month of April, 1844, the date of the first employment of the cauterization of the base of hemorrhoidal tumours with grooved forceps charged with Filhos' caustic, the idea of which originated with me, we had perceived that this new mode of operation enabled us to obtain the cure of hemorrhoids, and that the hemorrhage, ulcerations, &c., disappeared together with the tumours; but it remained to be shown whether, in cases in which these affections should be complicated with tolerably extensive prolapse of the rectal mucous membrane, cauterization by the same plan would remedy the principal affection and its complication. Experience has not been tardy in justifying the hope we had entertained in this respect. It is, moreover, easy to account for this result, which we verified in all our operations of this kind, in reading the description of our proceeding. The cases of the patients in whom this complication was sufficiently advanced, and the drawings taken from nature, which I have appended to them, will, I trust, enable practitioners to understand it.

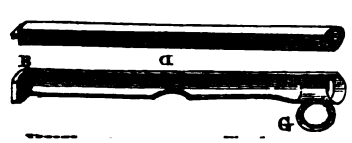
When we are about to examine a patient whom we suppose to be affected with internal hemorrhoids, we cause him to place himself on a vessel, and to make efforts

at defecation; the tumours then come down, and we easily demonstrate their presence; often they even permanently appear externally, or show themselves on the exercise of the least effort. But as it is necessary to judge precisely of the extent of the prolapse of the mucous membrane of the rectum, we advise the patient to take a lavement, and to pass it again in our presence some moments after, using efforts, and continuing the latter when he gets up. With the assistance of this mode of investigation, we arrive at an exact appreciation of the extent of the affection, and the operator is enabled to judge beforehand at what height he ought to place the *porte-caustique* forceps, in order to cure at once the hemorrhoidal affection, and the rectal prolapse which complicates it.

The introduction of the index finger into the rectum is a means of diagnosis on which we must not place too much reliance, for I have known several patients who, examined in this manner by the most distinguished practitioners, had been declared not to have hemorrhoids, and in whom I, nevertheless, found even voluminous ones. The surgeon accustomed to this mode of examination, indeed, experiences internally a peculiar sensation of softness in certain points, proves the existence of inequalities rather than of tumours disappearing under the finger, and gives pain to the patient when the tumours are ulcerated; but in general these signs are not sufficient to enable him to affirm whether a hemorrhoidal affection exists or not. The rectal *toucher* is especially useful in ascertaining whether any particular complication, such as a stricture, polypus, or cancer, coexists.

At the same time we examine the number of tumours which ought to be cauterized, as, for the following reasons, we never destroy them all:—In order not to suppress suddenly and completely a state to which the system has long been accustomed, and not to expose the patient to a disturbance which might not be free from inconvenience; besides, experience has shown that when we have destroyed the more voluminous tumour when there are two, or the most voluminous ones when there are more, those which remain diminish and give much less inconvenience. The cauterization of the largest hemorrhoid, when well performed, often prevents the others coming out, and the ulcerations, if such exist, being no longer subjected to a continual friction against one another, cicatrize, and the patients enjoy as good health as if they had been completely free from them. Consequently, a different mode of proceeding would be at least useless, and might sometimes be dangerous.





If, contrary to what usually occurs, the patient should subsequently suffer, recourse might be had to the operation, which would not then be more severe than before, and would remedy an inconvenience which we sometimes meet with, but which much more frequently does not present itself.

In ordinary cases I employ my forceps in the form of a compass with protecting blades,—more convenient when it is necessary to carry the cauterization to an elevated point of the rectum,—but in the case of which I am now speaking, I prefer the T-forceps of my father, or those which I have had made on the same plan, with very slight modifications. In fact, when there are voluminous hemorrhoids, with more or less extensive prolapse of the mucous membrane, the tumours expanding largely externally, it is very easy to cauterize their base.

Since the month of June, 1844, the period of the construction of my first grooved forceps for the cauterization of internal hemorrhoids, we have subjected both the instru-

ments and the operative manipulations to numerous modifications, which have made the operation so simple that a perusal of the description of it will render its application easy; and already distinguished surgeons, among whom I would mention MM., Maher, J. Roux, and Laurencin, Professors in the Schools of the Imperial Marine, MM. H. Larrey, Barthélemy, Martin Saint-Ange, Thirion (of Namur), Mascarel, &c., have employed this proceeding with success, using instruments made on the model of ours or having the greatest analogy to them.

The T-shaped porte-caustique forceps of my father, a drawing of which is given above, was made on the model of a little forceps he used for rupturing the internal membranes of arteries, in order to facilitate torsion. The forceps is drawn one-third less than its actual magnitude; the groove and blade, separately represented, are of the full size. This instrument is composed of two legs, united at one of their extremities by a spring; at the other they are furnished with two grooved semicircles of steel, D, perpendicular in their direction, and so arranged as to oppose one another when the forceps is closed, or when it embraces the base of the tumour which it is wished to cauterize. Two small sliding plates, B, having a movement of rotation around the grooves, which are cylindrical, have at one of their extremities a small handle, C, intended to be used for moving them, and consequently to cover or uncover the caustic when it is thought advisable to do so. A screw, A, is used for tightening the legs of the instrument. The two sliding plates, to which I have given the name of protectors, have partly replaced the preserving forceps which we originally used for completely isolating the tumours from the surrounding parts previously to cauterizing them. When the protectors are placed in front of the grooves, the caustic being entirely covered, we may seize the tumours, let them go again, take hold of them, or slide the grooved semicircles to a greater or less height without the patient experiencing the slightest painful sensation.

To fill the grooves of the instrument, I employ Filhos' caustic in paste, or solid. The caustic paste is made by throwing a sufficient quantity of pulverized Filhos' caustic into a saucer, adding some drops of alcohol or eau de Cologne, and triturating the mixture with a spatula. When the paste has the consistence of ordinary honey it is placed in the grooves, and covered with the protectors. To fill them with solid caustic, the latter is poured into them while in a state of fusion.

Internal hemorrhoids are often complicated with corresponding external hemorrhoids, or rather with swellings forming two rows of superposed tumours. Experience has proved that it is not necessary to destroy the external swellings—they die away when the internal hemorrhoids have been destroyed by caustic.

The operation having been decided on, I generally prepare the patient for several days previously with depuratives, baths, and mild regimen, even venesection, if it should appear necessary; and the evening before the operation I prescribe a purgative of cold-drawn castor-oil, in order to unload the bowels, and to prevent the occurrence of alvine evacuations for two or three days at the least.

The articles necessary for the operation are: one or two porte-caustique forceps; pulverised Filhos' caustic; alcohol or eau de Cologne; a sheet folded square; a piece of oilcloth; a syphon for continuous irrigation; a syringe, or a clyso-pump; two large vessels, one to contain cold water intended for the irrigation to be carried on during the operation; the other to receive the fluid when it shall have passed over the anus.

Before the operation the bed on which it is to be performed should be arranged. The folded sheet is placed across it, and is covered with an oilcloth reaching to the vessel placed under the edge of the bed, to receive the water projected during the operation; and in order that the latter may flow off easily, the two inferior angles of the oil-cloth should be pinned together, so as to form a channel to carry the water directly into the vessel intended to receive it.

The patient, having taken a lavement during these preparations, passes it again, at the moment of operation, into a vessel left near the bed, and then lies down, still keeping up the efforts at expulsion, which are indispensably necessary to allow the surgeon to place the forceps properly.

The position of the patient which I prefer is the same as that for the operation for fistula in ano. Doubtless, if the patient were on his knees, or placed as for the operation of lithotomy, he would be better circumstanced for keeping up the efforts; but notwithstanding the advantage the operator might have by employing one or

other of these positions, I have renounced them on account of the fatigue the patients experience—a degree of fatigue which once seemed to me to have occasioned syncope.

I am not in the habit of submitting my patients to the action of anæsthetic agents unless they earnestly request it; knowing by experience that the operation is not sufficiently painful to render their employment necessary, for under the influence of the very strong compression used, and of the douches of cold water, the pain is easily borne. During the operation the patient feels a pinching, which in a great measure masks the pain of cauterization, and consequently only complains of the latter at the moment of withdrawal of the instrument. This smarting, besides, is diminished rapidly enough by the means I employ immediately after the operation. Two assistants placed at the sides are engaged in holding back the neighbouring

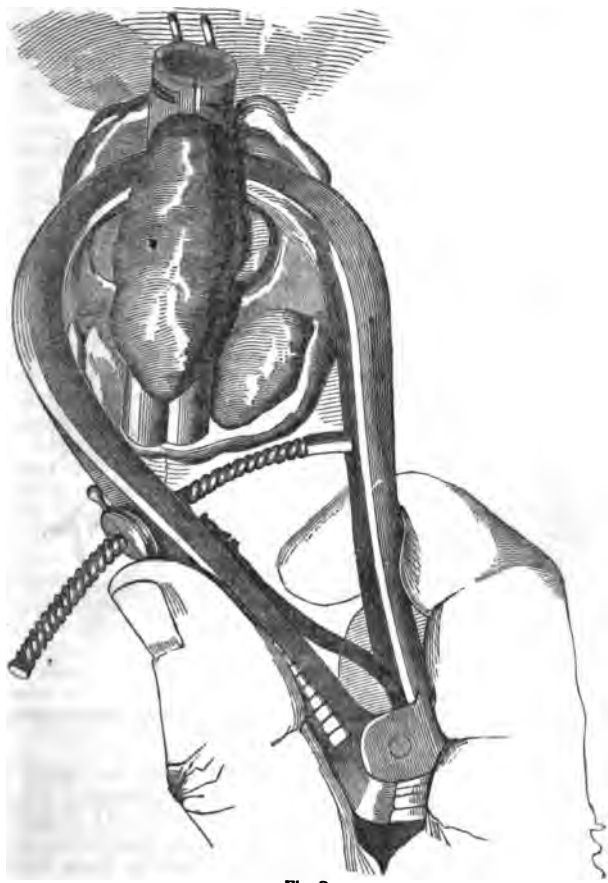


Fig. 2.

This figure represents a voluminous tumour caught with my forceps and vice, with the caustic acting.

parts; a third is occupied in directing a continued stream of cold water on the anal region. Strictly speaking, one assistant, and that even a non-medical person, might be sufficient, as he would only have to work the piston of the instrument, for the operator could direct the current of liquid with the left hand, while he held the forceps in the right, but it is much more convenient to be assisted by several persons.

The patient having been placed on a bed in the manner described, and continuing the efforts at defecation made in passing the lavement, the assistants carefully keep

back the surrounding parts. The operator holding in his right hand the porte-caustique forceps with its troughs covered, so as to be able to touch the tissues without causing the least painful sensation, seizes the tumour gently, without compressing it, and desires the patient to make efforts, if possible, still more energetic, in order to allow him to pass the instrument to the height he judges advisable. This period of the operation, very important in all cases, is especially so in this. When the forceps is well placed, he tightens the screw sufficiently to prevent the tumour escaping, but so as to be able, with ease, to turn the grooved pieces or the protectors. When the caustic is in contact with the tissues, the instrument is tightened as much as possible but gradually, in order to bring the compression to its maximum. By acting thus, the duration of the operation is shortened, and the pain is diminished, the patient most frequently complaining only of the pressure. The douche of cold water is immediately commenced, and continued while the operation lasts, which varies from two to four minutes. With strong forceps and fused caustic two minutes are longer than is necessary for cauterization. The time, however, as may be supposed, varies according to the bulk of the tumours to be destroyed.

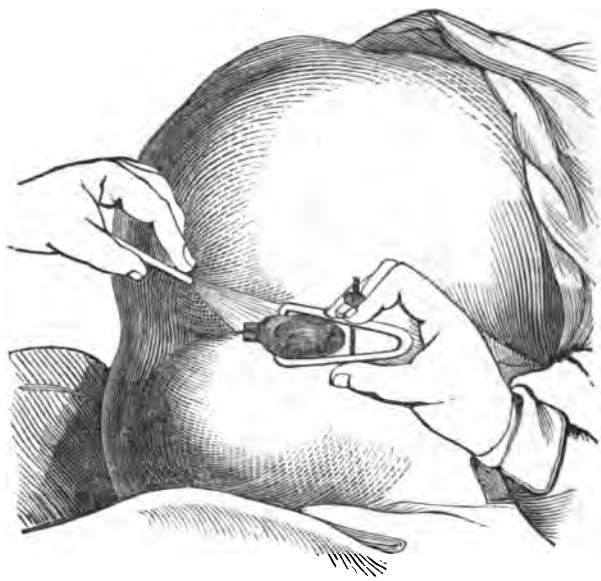


Fig. 3.

This figure represents the operator holding with his right hand the forceps placed upon the tumour, and directing the jet of cold water with his left.

The instrument is then gradually loosened and withdrawn, care being taken to re-cover the grooves to prevent the caustic touching the neighbouring tissues. The assistants should keep the parts separated, and the patient should be again desired to make efforts at expulsion, that the cauterized tissues, on which a little caustic the action of which is not yet over still remains, may not cauterize those which should be protected.

The operation being over, the douches of cold water both on the anus and internally should be continued so as to neutralize the last portions of caustic which may not have acted. I have often with advantage applied olive oil or vinegar and water to the cauterized parts.

Should a porte-caustique forceps not be at hand, the operation might be performed as follows:—The patient having been placed in the position I have described, and the same preparations having been made, the tumour it is wished to destroy should be seized, as high as possible, with an ordinary dressing or dissecting forceps, and cauterized directly, by placing in the centre a stick of Filhos' caustic cut into a conical

shape, to which a rotatory motion should be given to make it penetrate the hemorrhoid, so as to destroy it centrally and literally. It would be necessary, during the operation, to protect the surrounding parts with spatulas or paper-knives, and afterwards to wash the cauterized tumour with slightly acidulated water, so as to neutralize all the caustic which might not have combined with the tissues. The subsequent treatment should be precisely the same as in the ordinary proceeding.

Immediately after the operation the patient is placed in a large bath, and when the tumours have been bathed for some moments, they are returned. He remains in the bath for an hour, and sometimes longer, and always experiences great relief from it; for it is then he feels the action of the caustic, which had, during the operation, been masked by the pressure.

On coming out of the bath, cataplasms of linseed meal enclosed in muslin bags are applied to the anal region, or irrigations of tepid water are directed to the part if smarting is still felt. The pain which patients experience after the operation is very variable; I have seen some who felt very acute smarting for twenty-four hours; but with the majority the painful sensation is, after some hours, but slight, if the precautions already mentioned have been observed. Some of my patients do not go to bed even the day of the operation; in general they remain sitting or lying on a sofa—for walking causes pain—and they are unable to go out for some days; elderly patients generally keep their room for a fortnight.

After the operation I allow but little nourishment, and that of a nature admitting of easy assimilation, in order to avoid action of the bowels, which always occasions acute pain. When the patients feel a desire to go to stool, I advise them to inject into the rectum two ounces of lard melted in a water-bath; it is useful in facilitating the passage of the fæcal matters, and in diminishing the smarting caused by the motion. A tepid sitz-bath should be taken immediately after the stool. Sometimes I give, during the treatment, a gentle purgative, in order to remove constipation when the eschar is not yet detached; while it is, on the other hand, advisable that the patient should not go to stool at the time when the cauterized tumour is separating in order to avoid the flow of blood which might then take place. An examination of the parts enables us to get a glimpse of the cauterized hemorrhoid, and to ascertain whether elimination proceeds more or less quickly; experience, however, has shown that, in general, the older and weaker the patients are, the more slowly this process goes on. The falling off of the eschars requires a very variable time, generally from five to eight days; I have seen some become detached at the end of seventy-two hours, while in other patients they were still adherent fourteen days after the operation. To render the consequences of the operation as favourable as I have described, the patient ought to take every day several sitz-baths at a moderate temperature, rather cool than warm, to keep cataplasms or water-dressing constantly on the anal region, and to use a very light diet. When the swelling has disappeared the same treatment should be continued for some days, but the nourishment may be gradually increased; we may then, with advantage, form our opinion of the result of the operation, and decide whether it is necessary to cauterize other hemorrhoids.

[M. Jobert, who formerly preferred the actual cautery, now prefers the caustic potash with lime.]

M. Jobert de Lamballe has recently published a new mode of cauterizing hemorrhoidal tumours *en masse* by means of caustic potash with lime. This surgeon seizes the tumours with a double arch of silver, which forms a sort of capsule, and covers them with Vienna paste, which he leaves applied for some minutes. The only patient on whom this operation has been performed had hemorrhoids of ordinary size, and two successive applications of the paste were necessary to destroy them. This mode of operation cannot be compared with the circular cauterization of the base of the hemorrhoids I have described. By my proceeding, in fact, the caustic is applied on a surface of but very limited extent, and the pressure of the forceps, combined with the continual irrigation with cold water, so moderates the pain of the operation that it is only exceptionally I have recourse to anæsthetic agents. Moreover, the pain which follows the operation is much less than in M. Jobert's proceeding, the cauterized surface being less extensive. His plan can only be compared with the mode I formerly pursued with a stick of Filhos' caustic. The latter proceeding possesses the advantage of not requiring a special instrument, and

besides, the solid caustic acting more quickly than the paste, the operation lasts a shorter time, and I have never been obliged to repeat the application.

[Dr. Amussat finishes this interesting paper by describing some of the sequelæ of his operation.]

The two or three minutes which I judge necessary for the cauterization having elapsed, the forceps is removed, care being taken that the caustic it contains shall not touch the neighbouring parts, and the injection of cold water, which has not been interrupted during the operation, is continued. The jets of liquid should be directed chiefly to the linear depressions formed by the instrument, in which the caustic is situated, in order to remove particles which may not have chemically combined with the tissues. It is at this time especially that the patient feels the cauterization, which is no longer masked by the pressure of the forceps, and the cold fluid pouring on the tissues has, in addition to the effect just mentioned, that of acting as a local anæsthetic. When the parts have been washed sufficiently to have removed all the caustic, the tumours are carefully smeared with olive oil, and returned into the rectum. The oil both serves to facilitate the reduction, and, to a certain degree, protects the tissues by combining chemically with the alkalies, should any still remain.

Small quantities of cold water are also thrown into the rectum, and allay the pain, and afterwards the patient is placed in a cold sitz-bath, or, better, in a full-sized bath at an agreeable temperature, which will, of course, vary according to the time of year. In this he generally remains about an hour, being guided in this respect by his own sensations.

Generally speaking, on coming out of the bath, the pain, which has gradually diminished, is very slight; occasionally, however, in very nervous persons, or when the caustic has not been completely removed from the tissues, it is still severe. In this case we advise the patients to return to bed, and to place themselves in a position similar to that they observed during the operation, while a continued douche of water, which in winter should be tepid, in summer cold, is directed against the anal region. I have known some to find it more convenient to substitute for the douche, continued applications of lint dipped in cold, sometimes even iced water, and very frequently renewed. This latter means, though undoubtedly efficacious, should be employed with care, so as to avoid the reaction which is apt to set in when it is suspended or left off. Other patients remain all day in the sitz-bath, the temperature being kept up by the addition, from time to time, of warm water. I once attended, with my father and Dr. Pouget, a person who, after the cauterization of two tumours, remained in a sitz-bath almost constantly for a week; he said he was very well in it, and not the slightest bad effect resulted from this protracted use of it.

Most frequently two or three sitz-baths during the day, small continued douches, with poultices on the anal region in the intervals, constitute the local treatment.

This plan should be persevered in until the wound is almost completely citatrized, especially if the least sign of inflammation should be observed.

The only nourishment I allow is strong beef tea, and I recommend the patient to keep his bed for two reasons; first, because by so doing, he bears the restricted diet better; and secondly, because the bowels are then less liable to act. Those who will not submit to this plan are obliged at once to take a more substantial diet, the result of which is stools, which are more painful in proportion as they occur nearer the time of the separation of the eschar.

Frequently a dysuria comes on which lasts some hours, and subsides spontaneously, for I do not remember having had recourse to catheterism in a single instance. There is generally swelling, sometimes partial, sometimes general, of the areolar tissue about the anus. The patients complain of it a little, and generally think it is the hemorrhoids which have come down. I employ only the local means I have mentioned, and if the patients are very irritable, I have the poultices sprinkled with a little oil and laudanum. I sometimes give on the first night a composing draught with syrup of poppies. The state of the patients is generally so satisfactory on the following days, that they complain only of the diet and rest which are imposed upon them.

At the end of three, four, six, eight, or twelve days, portions of sloughs will be seen on the poultices, indicating that the mortified tumours are thrown off, of which the characteristic odour they diffuse likewise gives notice. The broth diet is con-

tinued as long as the patient does not complain too much of it, and in this way the work of elimination and reparation goes on very well, as there is nothing to interfere with it. I endeavour to postpone the first motion as long as possible, without, however, annoying the patient too much. When he feels a repeated desire to go to stool, which generally occurs at the end of six or eight days, I direct two or three ounces of lard melted in a water-bath, or a lavement of decoction of marsh-mallow, to be thrown into the rectum. The introduction of the tube should be managed with the greatest care, so as not to irritate or injure the wound. This duty should be intrusted to an assistant or an intelligent nurse.

The first stool is always rather painful, and the patient passes a few drops of blood; in two of my patients two or three ounces were lost without any bad effect except some increase of weakness, which I met by improving the diet. If the exclusive use of beef tea does not become too repugnant to the patient, which generally takes place, I induce him to continue it for some days, being guided by the time at which the eschars fell off, and the probable state of the suppurating surface, the extremity of which can generally be seen at the anal orifice. It is unnecessary to observe, that I discontinue the baths as soon as the pain about the anus has ceased, as they would, along with the restricted diet, lower the patient too much. My object in continuing the exclusive use of the broth, is to postpone the second motion, and to render it as small as possible. If, however, this diet does not sufficiently support the strength, or if disgust comes on, I substitute fresh eggs, gravy, and a little bread or pastry, gradually increasing their quantity. The second stool usually takes place from the twelfth to the fifteenth day, and ought to be preceded by the precautions I have detailed in reference to the first. The patient now begins to resume his ordinary mode of life, a great many go out, and some return to their occupations, taking care, however, to use an occasional sitz-bath, to foment the anus with tepid water several times a day, and particularly after having been at stool.

Such are the phenomena I have observed, and the measures I advise, after the circular cauterization of the base of hemorrhoidal tumours, whether simple or complicated with prolapse of the neighbouring rectal mucous membrane; and from what I have stated it will easily be inferred that not only have I never lost a patient in consequence of the operation, but I have never had reason to be uneasy about my patients. I may add, that in no case have I observed the slightest symptom of pyemia to arise.—*Dublin Quarterly Journal of Med. Science, August*, 1854, *p.* 231.

77.—TREATMENT OF HEMORRHOIDS WITH VIENNA CAUSTIC, AND EMPLOYMENT OF THE HEMORRHOIDAL CAPSULE.

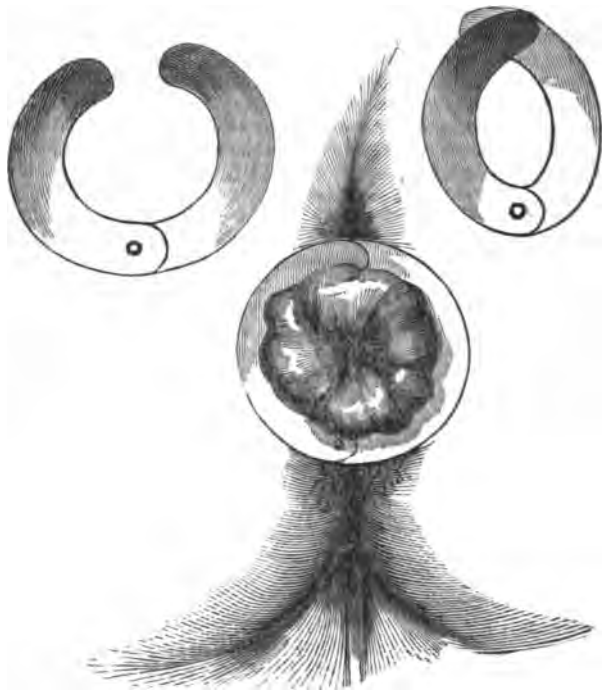
By M. Jobert de Lamballe.

Though we are advocates (observes the editor of the 'Bulletin Générale de Thérapeutique') for the employment of the actual cautery in the treatment of hemorrhoidal tumours, we think, however, that there are cases in which we may, without inconvenience, have recourse to the use of the Vienna caustic; and we think it advantageous to our readers to make them acquainted with an ingenious instrument which M. Jobert de Lamballe has had made by M. Charrière, to render this operation more easy, and to deprive it of any danger to the patient, and to which he has given the name of hemorrhoidal capsule. By conceiving two blades of silver, or other metal, concave and articulated with each other (fig. 1), so that their concave edges may be approximated, embracing a more or less extended ellipse, we shall have an exact idea of this little instrument, which is as simple as it is ingenious and easily managed. When the instrument is closed, as the two external edges which form the outer circumference are a little elevated, it represents exactly one of the capsules used in chemical laboratories, and the resemblance is still more striking if, instead of examining the instrument by itself, we view it when embracing an hemorrhoidal tumour (fig. 3).

As it is easy to have these instruments of all sizes, and as we can at will increase or diminish the curve formed by the two blades, it is evident that we can apply the capsule to tumours of every magnitude. If we have to operate on one or more hemorrhoids, distinctly isolated from one another, a little capsule, less ellipsoid and

almost circular, is pressed successively close round the base of each, and the caustic is applied; the blades of the capsule protect the neighbouring parts, and all the tumours may be thus separately cauterised, either on the same occasion, or at an interval of several days. If the case be one in which there is a tumefied hemorrhoidal circle (bourrelet) which, though distinct at first, has become agglutinated, and forms a single mass, the lobes of which cannot be separated sufficiently to admit of their being attacked one by one, the entire is included in one larger and more elongated capsule, and the whole surface of the tumour is acted on. M. Jobert de Lamballe has recently performed this operation on a man aged 55, who had a prominent tumour of this kind, of a bright red colour, and elevated at least two centimetres and a half (nearly an inch) beyond the margin of the anus, and formed, as is seen in fig. 3, by three tumours which were then adherent to one another, but

Fig. 1. Fig. 3. Fig. 2.



which must have been originally quite distinct. On the 15th of September the entire mass having been enclosed at its base between the two blades of an hemorrhoidal capsule, a layer of the Vienna paste was applied over the whole surface, and kept on for four minutes and a half. The caustic having been removed, and the tumour well washed, the surgeon seized each of the blades of the capsule with a forceps in order to separate them from one another. Cold lotions were applied during the following days. From the 16th the tumour was declining and diminished in size; and on the 18th a portion could be removed with the forceps. The cauterization was repeated on the 19th. On the 23rd the eschar was completely detached. A rose-coloured protuberance which existed around the anus was touched on that day, and the next day but one with nitrate of silver. On the 26th the cure was complete.—*Bulletin Générale de Thérap.*—*Dublin Quarterly Journal, Nov.* 1854, *p.* 469.

78.—*Hemorrhoids treated by the Galvanic Cautery.* By JOHN MARSHALL, Esq. —[Mr. Marshall is still pursuing his investigations on this application. He says:]

I am still, as opportunities offer, prosecuting my enquiries into the advantages and disadvantages of the galvanic cautery as a remedial agent in a great variety of cases; and the accumulated experience of nearly three years will be faithfully reported ere long to the profession.

As regards internal hemorrhoids in particular, I have used two methods; one, that of absolute removal by the heated platinum wire; the other, that of destroying the substance of the tumour *from the surface* by the application of a *disc-shaped* coil of incandescent wire—in fact, operating on the same principle as that adopted by Mr. Steel.

The results of my cases, of which details will be hereafter published, clearly establish the conclusions: that small, isolated, and prominent internal hemorrhoids may be easily and advantageously removed across their base; but that a large mass (and I have dealt with very large masses) may be *best* destroyed by deep cauterizations upon it from the mucous surface. That this latter proceeding is safe and successful I can also assert; but the originator of a method is, perhaps, the worst authority as to its relative advantages over other modes of treatment.—*Association Med. Journal, March* 31, 1854, *p.* 302.

79.—ON PROLAPSUS OF THE RECTUM.

By Dr. Fleming, Surgeon to Richmond Hospital, Dublin.

[The following case shows how efficacious is the nitric acid in these troublesome affections. The patient, a young man of 24, could not remain for any time in the erect posture without the escape of the rectum to an enormous degree.]

In the absence of any protrusion, the anus is patulous, and its circumference, covered with muco-sanguineous discharge, presents numerous ulcerated chinks in the direction of its rugæ. It is so wide, and the sphincter so relaxed, that a gaping funnel-shaped cavity is exposed, within which the fingers, placed conically, passed with ease. It and the greater portion of the rectum protruded on the slightest exertion, and constituted a tumour of considerable size, presenting somewhat the appearance of a prolapsed uterus. The circumference, at its base, exceeded, by measurement, nine inches, and a line drawn from the base to the free end, where the orifice of the bowel existed, reached beyond four. The mucous membrane hung in loose, pendulous, and concentric folds, coated over with the ordinary tenacious and slimy secretion seen in such cases, and its submucous tissue was thickened with copious infiltration. Its surface, when first exposed, was of a bright red, which, in proportion to the duration of the prolapsus, assumed a dark congested modena colour; it presented irregular patches of ecchymoses, of adherent lymph, and of extensive ulceration. It was by no means sensitive to pressure; on introducing the two forefingers within the intestinal orifice, grasping the protruded intestine between them and the thumb, and measuring the intervening depth from the anus, there was so much of thickness as to give the sensation of the complete prolapse of the bowel. It was curious to remark how he restored this vast protrusion, and sometimes he did so by muscular efforts alone, without the slightest manual help. He bent forward, pressed his hands on his knees, when a gradual diminution took place in the bulk of the protruded bowel, and with a sudden gulp it disappeared, all being apparently effected by a combined and violent muscular effort. Again he failed in this, and was obliged to assist it with his hands. It is unnecessary to specify the details of treatment during the months of March and April, further than to state, that the horizontal position was enjoined, diet properly regulated, the regular action of the bowels ensured, and all the ordinary local means adopted to restore the healthy condition of the protruded bowel. On the 16th of April, although much dissatisfied, he acknowledged some improvement as to the amount of protrusion, and as to the discharge from the bowel, and the latter was obvious from the altered state of his linen, which was considerably less stained. The ulcers around the anus had a much more healthy appearance, and the rectal mucous surface was less loose and pendulous, much less congested, and the excoriated and ulcerated patches healed; yet the protrusion was considerable; any operative measures appeared to me quite unsuited to his case, as well from the nature of his constitution as from the large extent of surfaces which would be necessarily implicated in them. It occurred to me that the

object sought by such could be accomplished by another expedient, and with that view I formed four equidistant radiated tracts, with concentrated nitric acid, commencing them near the intestinal opening, at the apex of the protrusion, and passing them as near as I could calculate to the upper margin of the inner sphincter. The breadth of those tracts was about a quarter of an inch, and the length two inches or so. They were well smeared with oil, and the bowel carefully returned: no pain was complained of during or subsequent to the application of the acid, which could be justly attributable to it: it was similar to that always complained of; it was relievable by the same expedients; it was not more severe during the act of defecation. There was no vesical, no abdominal irritation. From day to day there was noticed a diminution in the amount and in the frequency of the protrusion, and the irritability and uneasiness about the rectum and verge of the anus were less.

20th April.—Fourth day from the application of the acid: report most favourable as to the progress of the case and the sensations of patient: all prominent symptoms improved. I made a careful examination of the rectum, and found in the nitric acid tracts many deep, ulcerated sulci, with defined edges; the lateral ulcerations were much more marked than the anterior or the posterior; there was no tenderness of any amount worth noting, and there was decidedly less of laxity of the mucous membrane. The ulcers at the edge of the anus were more healthy: I touched them over with sulphate of copper.

25th April.—Progress good; discharge from the rectum and anus much less; local uneasiness comparatively slight; general health greatly improved, and feeling of great comfort and relief experienced in consequence of the absence of tenesmus and frequent prolapse, as heretofore. Feculent discharges once daily, and without pain, and subsequent easy returns of bowel.

28th April.—Improvement, in every particular, progressive; obvious diminution in bulk and shape of protrusion, and mucous membrane much more tensely spread over it; ulcerated patches visible, but more limited and superficial; proportionate decrease in discharge; two ounces of an astringent lotion to be thrown into the rectum morning and evening.

3rd May.—Fifteenth day from first application of nitric acid; report most satisfactory; protrusion of bowel less frequent, occurring only once in twenty-four hours; mucous discharge also much diminished; defecation natural and without pain; can cough, sneeze, and use any exertion without any protrusion, also walks about without annoyance; outlines of previous ulcerated patches merely visible, and intervening portions of mucous membrane much more fixed, but yet loose and flabby. Pencilled those spaces with nitric acid, as in the first instance, but less extensively; effect apparently now more productive of pain, yet not very acute, and easily controlled by hip-bath and anodyne enema. Complete subsidence of all uneasiness after a few days; subsequent steady and progressive improvement in tone and in functions of bowel, so much so that on 14th May the patient was very much improved: the bowel has not now descended for many days; its contents are discharged regularly once in the twenty-four hours, without pain, and without any appreciable amount of prolapsus, and the discharge from the anus is much less. Further more minute details of progress or of treatment would be here inappropriate. The case was witnessed whilst in hospital, at its different stages, by a very intelligent and observant class of pupils. The man left of his own accord on the 4th June, having then had full control over the prolapsus, and the most healthy action of the bowel being established. He moved about without any inconvenience—was recommended to go to the sea-side to bathe, and to attend to the state of his bowels. In August he called on me, on his way with his fellow-workmen to see the Industrial Exhibition, and expressed himself as being perfectly free from the prolapsus, or any other annoyance. I then examined the rectum, with Dr. M'Kenzie, of Edinburgh, who happened to be in my study at the time, and heard his report of his condition. The anus was loosely contracted, yet not patulous, and the ulcerations around appeared to have perfectly healed. There is every reason to assume that the cure has been permanent.—*Dublin Hosp. Gazette, March* 1, 1854, *p.* 37.

80.—*Prolapsus of the Rectum treated by Nitric Acid.* By Dr. M'Dowell, Dublin.—In this case at first the prolapsus occurred only when the patient was at

stool, and the bowel would remain protruded until returned by the finger. After a short period, however, the protrusion occurred whenever the patient stood up, or made any exertion. Attacks of inflammation of the protruded mucous surface soon became of frequent occurrence, which rendered difficult the return of the bowel, and added greatly to the patient's sufferings. A purulent and muco-sanious discharge had become established, and proved a source of great discomfort. In fact, at this period it was found necessary to keep the patient in bed, which, for the last six months, she had scarcely quitted.

The protrusion had become permanent, and occurred to the extent of about four inches; it could be replaced with facility, owing to the relaxation of all the structures, but returned almost immediately, even when the patient lay in the recumbent position.

Various means had been employed, but without benefit, when, by Dr. M'Dowell's directions, the treatment by strong nitric acid was commenced. At this period the integuments over the sacrum had begun to slough from the constant confinement to bed, which the unhappy condition of the patient rendered unavoidable.

The acid was first applied on February 16. Four white tracks were made with the acid in the vertical direction from the sphincter, down to the lowest portion of the protrusion; the bowel was then replaced; no protrusion occurred for two days afterwards. The acid was applied twice subsequently at intervals of seven days, and on the last occasion with the most complete success.

April 2.—Eight weeks have elapsed without any return of the prolapsus, and the patient, previously bed-ridden, suffering from bed sores, and debilitated in health from long confinement and want of exercise, is now able to go about as usual, and is restored to a state of health which she has not enjoyed for a very long time.

The treatment occupied a period of about three weeks.

April 29.—There has been no recurrence of the disease.—*Dublin Hosp. Gazette, May* 15, 1854, *p.* 122.

81.—*Prolapsus Ani treated by Strychnine.*—M. Duchaussay advocates the use of strychnine employed endermically near the region of the sphincter, as a means of curing prolapsus ani; and he is of opinion that Dupuytren's operation, by excision of radiating folds of skin around the anus, as well as that practised by Guersant, by means of the actual cautery, act merely by stimulating the sphincter muscle, and causing it to regain its contractility. With M. Guersant's permission, strychnine has been thus employed with success, in the case of a child, in the Hôpital des Enfants Malades.

The prolapsus arose from obstinate constipation, had lasted four years, and protruded at times four inches. Considerable improvement had followed simple laxative treatment, but the gut continued to protrude an inch and a-half after each evacuation.

The child was cured in about a fortnight by the application of small quantities (one-sixth to one-third of a grain) of strychnine to a blistered surface in the neighbourhood of the sphincter.—*Archives Gén. de Médecine.—Dublin Hosp. Gazette, Feb.* 15, 1854, *p.* 30.

82.—*Excision of Internal Hemorrhoidal Tumours.* By Henry Lee, Esq.—[In the 'Medical Times and Gazette' for August 19, Mr. Henry Smith remarks that when hemorrhoids are situated within the sphincter ani, excision is a dangerous remedy. This opinion is answered by Mr. Henry Lee, as follows:]

This assertion, involving an important point of daily application, must be admitted, I believe, with much reservation. Any operation in surgery may be dangerous, if care be not taken to secure the bleeding vessels. Before the invention of the ligature, any incision which divided a blood-vessel might be considered dangerous; and we have it upon record, that in two-fifths of the cases of internal hemorrhoids, excised by M. Dupuytren, alarming symptoms supervened in consequence of subsequent hemorrhage. Such a result is nothing more than might have been expected in this or any other part of the body, where vessels were divided and left to bleed. But here, as elsewhere, the danger disappears as soon as we have the means of securing the divided vessels. We now have the means of preventing hemorrhage

from divided hemorrhoidal vessels, as readily as from any other part. The danger from bleeding after operation is, therefore, not greater here than elsewhere; and, as the fear of hemorrhage was the only objection to the excision of internal hemorrhoids, that operation may now be considered as safe as any in surgery.

In support of these observations, I may mention that, within the last few days, I have excised internal hemorrhoidal tumours in three instances, without the loss of as much as a teaspoonful of blood in any case.

The first of these patients was brought to town by Mr. Cross, of Petersfield; the second was under the care of Mr. Carter, of Hadley; and, in the third instance, I was assisted in the operation by Mr. Harrison, of the Albany Court Yard.

The results in these cases have been most satisfactory; and, until I see reason to alter my opinion, I shall believe that (where any operation is necessary) excision, properly performed, is a safer, and often altogether a better, mode of treatment than the ligature for hemorrhoidal tumours, internal as well as external.—*Med. Times and Gazette, August* 26, 1854, *p.* 226.

83.—CASES OF FISSURE OF THE ANUS.

By John Hamilton, Esq., Surgeon to Richmond Hospital, Dublin.

[The patient in the first case (reported by Mr. Lloyd), was Arthur B., aged 32. The anus prolapsed, attended with great pain when passing a motion; it was also lax, and surrounded by loose wrinkled folds of integument, hanging like a fringe round the aperture. At the right side, rather near the coccyx, a fissure could be seen, the surface red.]

Jan. 17th.—The operation for fissure of the anus was this morning performed by Mr. Hamilton. A speculum was introduced into the rectum, and an incision made through the ulcer, beginning a little above its upper edge, carried through its base, and a little below the lower edge; there was very little bleeding, and a piece of lint was inserted into the wound.

Jan. 27th (the tenth day).—He was discharged well, the wound cicatrized, no uneasiness at the anus, and no prolapsus, the one operation having cured both the fissure and the prolapsing piles.

Fissures of the anus, as they are called, are in reality narrow oval ulcers at the margin of the anus and within it, which assume the appearance of fissures in consequence of the vertically plicated arrangement of the termination of the rectum. They generally cause the most excruciating sufferings, and after long continuance reduce the patient to a state of extreme nervous irritability. He feels uneasiness, and a sense of weight for some time previous to a motion, but the great pain is felt during the passage of the fæces, which, when they are hard and large, is dreadful. The fear of this suffering constantly causes the patient to delay for days together going to stool. But nearly as much pain is endured after the motion, continuing for some time, the period varying in different cases. In some there will be two or three hours of great suffering, in others six or seven. I have even known it last sixteen and twenty hours, till the patient was completely exhausted.

In very recent and slight cases, where the fissure is just at the margin of the anus, scarcely going within it, you will succeed in healing them by touching the fissure with a strong solution of the nitrate of silver, a drachm to the ounce of distilled water, or by very light touches with the solid caustic, and by keeping a small shred of dry lint well in the fissure; the last is a very essential part of the treatment, and you should make your patient come to you daily after the morning motion is passed. The solution of the nitrate should be applied every second day; if the solid is used, not oftener than every third day. The bowels, at the same time, kept relaxed by a few grains of powdered rhubarb and blue pill at bed-time, or a couple of teaspoonfuls of the lenitive electuary.

The following is a case of the mild form of the disease:—

Mr. H. P. came to me, May 31st, 1850, with a small fissure at the coccygeal side of the anus, readily visible by separating the sides of the anus; it was red on the surface, but did not go high up on the inside; not very painful at stool, bleeding a little each time. He had had it only a few days, and thinks it came on from his having been frightened at stool, when the passage of the fæces was suddenly ar-

rested. They were so hard next day that the fissure took place. I touched it with the nitrate of silver, and put a little piece of dry lint into it. On the third day a second application was made, and a gentle laxative ordered.

June 11th.—He passes motions well and without pain.

But if you have a well-marked fissure of any standing, it would be perfectly useless to try the application of caustic, which, while in such cases it gives the most intolerable pain, often lasting the whole day after, seems to exercise no influence whatever on the fissure. The established treatment is, to make an incision through the whole length of the centre of the ulcer. We owe this most effectual plan of cure to M. Boyer. His idea was, that the obstinate resistance of the ulcer was mainly due to the action of the internal sphincter, and if this was overcome, the fissure would heal. He, therefore, made a cut through the centre of the ulcer, deep enough to divide the fibres of the internal sphincter, and the successful result seemed to justify his theory. I believe, however, you will find that the division of the sphincter is not necessary to the cure; all that is requisite is an incision deep enough to go through the base of the ulcer. I was led to this conclusion by the fact, that fissure is most common at the coccygeal end of the anus, where there are no muscular fibres at all, and yet the operation is as successful as when the fissure is at the side where the muscular fibres abound. In this respect I fully agree with Mr. Syme.

You must not think that matters will always go on so smoothly, or that the mere division of the fissure will be sufficient. If you were guided by some authors who have written on this subject, you would be misled in thinking there was nothing further to do. Even in uncomplicated cases, about the third day after the operation, the part begins to get very sensitive and painful, and will continue so till the surface of the divided ulcer becomes granular, when all pain ceases, and cicatrization rapidly ensues. In other cases the pain does not materially diminish; though less, perhaps, than before the operation, the surface of the ulcer is unhealthy, greenish-yellow, and the surrounding inflammation too great. You see there is something faulty in the constitution acting on the local complaint, and it is through the constitution you must act on the local disease. The tongue is loaded with yellow, the complexion is sallow. I know nothing better than slight mercurialization; two grains of blue pill, two of powdered rhubarb, with a little extract of taraxacum, three times a-day, till the gums are slightly touched.—*Dublin Hos. Gaz., May* 1, 1854, *p.* 98.

84.—*On the Surgical Treatment of Fissure of the Anus.* By T. B. Curling, Esq.—In fissures of the rectum from ulceration it was, till lately, usual to divide the sphincter muscle. Another and much simpler method now seems efficacious, which Mr. Copeland has practised for some time. Instead of dividing the sphincter, he makes a slight superficial incision. Mr. Curling had an interview with Mr. Copeland on this subject, and has since that time modified his former practice.

He says:]

Since this interview, I have been in the habit, when treating severe cases of irritable ulcer, of making only a simple superficial incision of the part: performing, in fact, a slighter operation than the one I previously practised and recommended. The results have been in all instances quite satisfactory.

Mr. Copeland, in describing his operation, spoke of it as merely a division of the mucous membrane. I am convinced that on this point he is in error; at any rate, that this is not sufficient; and that, however slight and superficial the incision may be, a few at least of the fibres of the sphincter must be divided. I had occasion to examine the rectum of a lady suffering from this affection; whilst she was under the influence of chloroform, and the parts being very lax, and in a good light, I was able to bring the ulcer well into view, and could distinctly perceive the fibres of the sphincter forming the bottom of the sore. Now it is clear that in such a case, or in an ulcer which has destroyed the mucous surface, an incision through the base of the sore must reach and divide muscular fibres.—*Dublin Hospital Gazette, June* 1, 1854, *p.* 137.

85.—ON PELVIC ABSCESS.

[The reader will find a series of most interesting cases of this affection in the 'Medical Times and Gazette' for August 12th, if he wish for full particulars. We have only room for an abstract of some of the most interesting points.]

Among the most important of the cases in which purulent collections form within the abdomen are those known as *pelvic abscesses*, in which inflammation of the cellular tissue of one or other part of the pelvis ends in suppuration. These cases have, in England, scarcely claimed the attention which might seem due to them, as they are not very rare. Although several papers of more or less value have been published respecting them in our own country, yet the only monograph on the subject with which we are acquainted is that of M. Marchal, of Paris. The cases recorded by the latter author, amount to 75, of which all, except 15, occurred in women. If, indeed, we may be allowed to except all cases complicated with visceral disease, and limit the definition to genuine phlegmonous inflammation of the cellular tissue, it might be asserted that the disease, with extremely few exceptions, occurs only in the female sex. When met with in males, it is almost always a secondary effect of disease of the bladder or intestines, or of inflammation of the peritoneum, or of the veins about the prostate or rectum. The reason of its frequent occurrence in women is evident. The internal organs of generation in that sex are subject to much disturbance. Not only are they constantly engaged during a large part of life in functional activity, but those functions require great alteration both in the size and positions of the viscera. To permit of the latter changes, a large quantity of connective tissue necessarily exists in their neighbourhood, which it may easily be supposed is prone, when exposed to the pressure, contusion, &c., incident to childbirth, to become the seat of inflammation. Probably not a few labours are followed by some tendency to such disease, which, in the majority, subsides under treatment, without going on to suppuration. In a few, however, such a happy termination is not achieved, but there is left in some part of the pelvis an inflammatory induration, which, after occasioning to the patient various distressful sensations for a period more or less prolonged, finally ends in abscess, and discharges itself either externally or into the bowel or vagina. Experience fully bears out this description; for in a very large majority of pelvic abscesses in females, the disease is found to have followed on parturition, at periods varying from a week or two to several months. In those in which the interval is the longest, the patients almost always give the history of having been ill ever since delivery, and of having felt that all was not right with them. Of the 60 cases occurring in women mentioned by M. Marchal, 52 were consequent on labour, and several of the remaining 8, inasmuch as they were complicated with visceral disease, ought scarcely to be ranked in the same list. Of eight cases recorded by Mr. Bell (see 'Medical Gazette,' Dec. 12, 1845, and Jan. 9 and 16, 1846), all were in women: six were immediately consequent on parturition, and two appeared to have been caused by exposure to cold while menstruating, neither of the patients having been recently delivered.

With regard to the exact part in which these abscesses originate some difference of opinion has prevailed. Dr. Doherty, one of the earliest writers on the subject, in a paper on "Chronic Inflammation of the Uterine Appendages after Parturition," considers the ovary to be the part at first involved in inflammation; other authors have spoken of the "parts constituting the broad ligament," while Dr. Lever, with much plausibility ('Guy's Hospital Reports,' 1854) on account of the difficulty in determining this point, proposes that the disease should not be spoken of by any more definite name than pelvic "inflammation." In all probability the disease is generally located in the cellular tissue only, and may originate in any part of the pelvis.

The prognosis of pelvic abscess following parturition is probably for the most part favourable. The patients often sink into hectic under the profuse discharge which follows, but as there is no persistent cause of irritation (as in psoas abscess, or those depending on foreign bodies), they generally rally as the abscess diminishes in size. The treatment should of course consist in the most liberal diet, combined with such tonics as may appear necessary. The abscess should be opened as soon as a tendency to point in any direction is perceived. The most favourable class are those, perhaps, in which the opening is into the vagina, as a free and depending

channel of exit is then procured, while no risk from the contraction of the cicatrix afterwards is encountered, as might be the case were the rectum the part selected.

[In the third case (under the care of Dr. Budd) a firm and very tender swelling was situated just within the brim of the right ilium, and extending thence deep into the pelvis. This showed itself about five or six weeks after labour.]

The abscess having shown a tendency to point at a part about an inch and a half within the anterior superior spine of the ilium, it was to-day opened, when about half a pint of thick pus escaped. Ordered wine ʒiv.; *pt. med. et cataplasm.* Some bed sores having formed, they are to be covered with amadou, rendered adhesive by means of lead plaster.

[On May 16, the discharge had nearly ceased, and the wound nearly closed.

The next case was under the care of Mr. Erichsen.]

Three days after delivery, she had suffered a feverish attack, attended by much headache, and which lasted about a week. As this illness passed off, she began to suffer from shooting pain in the right iliac fossa. These pains continued to recur, and, five weeks later, she discovered a swelling in that region, and a second one more towards the median line. After a time, these two swellings joined into one large one, and from this she suffered frequently what she described as "dreadful" pain. Her bowels, at the time, were confined, and the stomach so irritable, that she vomited almost everything she took. With these symptoms she was admitted into an Hospital, where leeches were applied to the part with some benefit, and, after a time, she was discharged much relieved. While an inmate there, she had much pain and irritability about the bladder. After her return home, the tumour again became more perceptible, and about six days ago, while lying quiet on the sofa, she had once or twice felt what she compared to something snapping in it. This account of her previous history, it must be observed, was obtained from herself only; but, as she was an intelligent person, there appeared no reason to mistrust it.

Present Condition.—She is emaciated, and suffers from hectic fever, with evening exacerbations, and a troublesome cough; bowels confined; pulse weak and rapid. Directly above the symphysis pubis is a tumour of about two inches in diameter, hard at its borders, and soft and fluctuating in its centre. On pressing it a distinct gurgling noise may be produced. Its borders extend on each side into the iliac fossæ, and on the left the gurgling noise may also be caused. It is very tender, and subject to constant aching pain. She states, that several times during the last few days she has had a distinct perception of wind passing with her urine. The urine has an acid re-action; it contains pus, but has no fæcal odour.

April 27.—The catheter having been introduced, air passed by its orifice, together with purulent urine; the gas that escaped had no smell. The catheter was not stained. The remedies prescribed consist in anodynes and expectorants for the relief of the cough, and a full nutritious diet has been ordered.

May 2.—The symptoms during the last few days, have continued as before, and several times the patient has herself distinctly felt the rumbling of the air in the tumour. This morning no gurgling can be perceived, and the skin over the swelling is discoloured, as if it were about to break. An examination by the vagina, and one also by the rectum, have been made, but nothing material was discovered.

4th.—The tumour burst spontaneously this morning, and, some air and pus having escaped, its walls collapsed. A poultice has been applied over it, on removing which, an escape of gas always takes place. Mr. Erichsen has enlarged the opening in a direction upwards.

18th.—Since last date great improvement has been made; the wound continues to discharge, but has a healthy appearance, and the cavity seems to be filling from below.

June 11th.—Discharged, a very small fistula being all that remains. She has regained almost her usual health.

Remarks.—It was very difficult, in the above case, to account for the presence of air in the cavity of the abscess. There were no indications of communication with the bowel, inasmuch as fæces never escaped by the bladder, or pus by the rectum, nor had the gas any fæcal odour. It is possible that the gas was spontaneously generated from the decomposition of the pus; but, had such been the case, there would probably have been present more severe constitutional disturbance, and the

discharge would have been attended by more of fetor. Perhaps the most likely supposition is, that the air had been derived from the vagina, either by transudation through a much-thinned membane, or by means of a valvular aperture so small as to have escaped notice. Another interesting feature in the case is the opening of the abscess into the bladder, which is not a usual occurrence. The opening was probably small, and did not allow of the escape of urine backwards into the abscess sac. The complication did not appear to materially interfere with the progress of the case.—*Med. Times and Gazette, Aug.* 12, 1854, *p.* 161.

[Dr. Priestley demonstrates the confining or directing influence exerted by the different fascial layers. He says]

The peritoneum lining the pelvic cavity is much thicker than that covering the intestines or other abdominal viscera. It is so reinforced by strong fibrous tissue in this region that it may well act the part of a fascia in offering resistance to, or directing the contents of, a pelvic abscess. It is everywhere closely adherent to the muscular structure of the uterus, except on the narrow lateral margins of that organ; and, by this firm adhesion of the peritoneum, and the thickness of the uterine walls, the rarity of cases where the abscess bursts into the uterine cavity may be accounted for. Almost equally rare, however, are instances of pelvic abscess opening into the bladder. This is to be explained, not by the close adhesion of the thickened peritoneum, as in the case of the uterus, but probably, from the loose attachments of the bladder with the surrounding parts, which are necessary on account of the varying degrees of distension of that organ. A space of considerable extent exists between the superior pelvic fascia below, which forms the true ligaments of the bladder, and the peritoneum above, constituting the false ligaments of that organ. This anatomical arrangement would permit the bladder to be pushed aside by an abscess forming in this locality.—*Med. Times and Gazette, Sept.* 2, 1854, *p.* 239.

ORGANS OF URINE AND GENERATION.

86.—IMPERMEABLE STRICTURE OF THE URETHRA CURED BY DIVISION IN THE PERINEUM.

By R. H. Meade, Esq., Bradford, Yorkshire.

[In this patient, 50 years of age, the stricture had existed for twenty years. The difficulty of passing an instrument seems to have gradually increased for a considerable period. Mr. Meade says:]

On introducing a bougie into the urethra, I found an excessively irritable and sensitive state of the whole canal, which made it even difficult to reach the stricture. When I did so, I found that it was seated in the posterior part of the urethra, behind the bulb, and considerable thickening and induration could be felt in this situation through the perineum and from the rectum. There seemed to be a great tendency to spasm, and the slightest pressure of the instrument on the stricture caused great pain. He said that he always made water in a very fine stream, and frequently only in drops; that both night and day there was almost constant inclination to empty the bladder, and the attempts to do so were always accompanied by great straining and forcible involuntary action of the bladder. During the last two years he had been subject to repeated attacks of spasmodic retention of urine, requiring the frequent use of opiates (both by the mouth and in suppositories), and the warm bath, for their relief.

[Numerous large crystals of oxalate of lime were found in the urine. The bladder and urethra were very irritable, and for this reason nitro-muriatic acid was given, and the patient ordered to remain quiet for some weeks. At the expiration of a month the urethra was less irritable, but the bougie was grasped by the strictured urethra and could not be passed.]

I found that the stricture and the whole of the urethra were so exquisitely tender, that though I used very trifling pressure, the patient suffered so much for several days after every attempt, that he could not summon resolution to have the trial

repeated for a week or ten days, or sometimes more, so that no progress was made.

I also applied a small portion of potassa fusa to the stricture; but instead of doing good it greatly increased the irritation, and brought on retention of urine. About two months after I first saw him, a small abscess formed in the perineum, which burst and was succeeded by a fistulous opening through which a few drops of urine escaped when he made water.

The man was very anxious that something should be done, and had made up his mind to go to Edinburgh, and put himself under the care of Mr. Syme, unless I could relieve him. I told him, that I thought the best way would be, to divide the stricture in the perineum, and thus introduce a catheter into the bladder; and, though it was a proceeding of some difficulty and danger in a case where no instrument could first be passed through the stricture, still it might be accomplished, and would afford a fair prospect of permanent relief. He readily consented, and with the assistance of my friend Mr. Coates (the late House-surgeon of our Infirmary), I performed the operation as follows, on the 14th of Feb., 1854. After administering chloroform, and fastening the patient in the position for lithotomy, I introduced a grooved staff of rather small size down to the stricture. The tissues in the perineum were hard and thickened, so that, though he was a thin man, there appeared to be considerable depth of parts covering the point of the instrument. I now cut down upon the extremity of the staff, and after dividing the textures in the raphé fully to the depth of half an inch, I got into the groove. I made an opening in the urethra sufficiently large to introduce the point of my finger, and endeavoured to pass a small grooved probe through the stricture, from the wound towards the bladder, but without success. Could I have done so, it would have been easy to have laid the strictured part of the canal open, by running a small knife along the groove towards the bladder. Finding that the stricture was impermeable, I introduced the fore-finger of my left hand into the rectum, and feeling for the prostate, took a small straight and narrow bistoury in my right hand, and passing it by the wound into the urethra, thrust it onwards in the natural direction of the canal, for nearly an inch towards the apex of the prostate gland. I now pushed on the staff in the line of my incision, and had the satisfaction to find it enter the bladder. The staff being removed, a No. 8 silver catheter was introduced in its place with a little trouble, and clear urine flowed through it.

There was very little hemorrhage during the operation, and all bleeding appeared to have ceased before the patient was removed to bed; in the evening, however, a little blood flowed through the catheter, which was retained in the bladder. On the following day (15th) he was quite easy, the urine was clear, and there was no constitutional disturbance.

On the 16th he was not quite so well; the catheter appeared to be producing a good deal of irritation of the bladder, and the flow of urine through it was impeded by portions of clotted blood and ropy mucus; I therefore removed it, and introduced an elastic gum one in its place. The pain and irritation continuing, the patient took this out of the bladder himself during the night, and on the following day I found considerable difficulty in re-introducing it, for a good deal of inflammatory swelling had occurred in the neighbourhood of the wound. The attempts which I made brought on considerable arterial hemorrhage, and I was obliged to put the patient under the influence of chloroform, when I succeeded in getting a silver catheter into the bladder, and then restrained the bleeding by plugging the wound round the instrument with lint soaked in tincture of matico. There was no return of hemorrhage: I allowed the catheter to remain for five days in the bladder, when I removed it, and introduced an elastic one in its place without any trouble, which I kept in for six days more. The wound in the perineum had by this time almost closed, only a few drops of urine escaping from it when the patient evacuated the bladder; and there appeared no further necessity to retain any instrument in the urethra; I was, therefore, content with passing one daily for another week, and then taught the patient to introduce one for himself on alternate days, which he soon managed without any difficulty. At the expiration of a month from the time of the performance of the operation, a No. 8 catheter (as large a one as the orifice of the urethra would easily admit) entered the bladder with the greatest facility, so

that from the sensation communicated to the hand it would not have been known that there had ever been a stricture.

Three months after the operation, there was no tendency to contraction of the canal, but I recommended him to continue the use of the bougie constantly, introducing it every two or three days. A small fistulous opening still remained in the perineum, but this occasioned so little inconvenience, that I could not persuade the patient to submit to any further treatment for its removal, as the application of a heated wire, &c. His general health had greatly improved, the irritability of the bladder had almost subsided, he had become stout and strong, and was able to follow his usual occupation.

I have been induced to relate this case from the acknowledged difficulty of treatment which exists, when a very tight stricture is combined with great irritability of the urethra, rendering the introduction of bougies almost impossible. No exact rules can be laid down for the management of such cases, but the surgeon must be guided in each individual case by his own judgment, and select such a mode of operation as may appear best fitted for the occasion. Something, perhaps, may be learned, especially by the young practitioner, from the perusal of detailed cases, and from a description of the particular steps followed in each operation.—*Medical Times and Gazette, Aug.* 12, 1854, *p.* 159.

87.—ON URINARY CALCULUS ADHERENT TO THE WALLS OF THE BLADDER.

By R. H. Meade, Esq., Surgeon to the Bradford Infirmary.

[This patient was 62, and on admission into the Bradford Infirmary, a stone was at once detected. After some preliminary treatment, Mr. Meade proceeds as follows to describe the operation :]

The operation was performed on the 1st of October, 1852, and nothing unusual occurred in its progress until the stone was grasped in the forceps, when a sensation was communicated to the hand, as if it was breaking down in the instrument; and considering, from the state of the urine, that it was probably composed of the mixed phosphates, and, therefore, soft and friable, I at first thought that this was the case; but I almost immediately noticed that the distance between the handles remained unaltered, and, gently withdrawing the forceps, the stone was extracted without any difficulty. It was now seen that the calculus had been adherent to the coats of the bladder, and that its separation had communicated the sensation in question.

The place of attachment was out of the reach of the finger, the patient being a short and rather fat man, with a deep perineum; but, on the re-introduction of the forceps, a rough surface could be felt on the posterior wall of the bladder, from which another small portion of calculous matter was removed.

The man recovered from the operation without any serious symptoms, but the wound was rather longer than usual in healing, and he continued for some length of time in a weak state of health, the urine still depositing a phosphatic sediment. He was discharged from the hospital two months after the operation, but was several months more before he completely recovered; he eventually did so, however, and I have seen him several times lately; and find that he is able to follow his usual occupation, and is now quite free from all irritation of the bladder. When it was found that the calculus had been adherent to the posterior part of the bladder, it was remembered that before the operation the stone could only be felt with the sound, when the latter was held in a particular position, viz., with the handle depressed as far back as possible between the patient's thighs, and this was especially noticed when the patient was in the recumbent position; it excited, however, no particular attention at the time.

The calculus presented some points of interest, independent of its adhesion to the bladder. It was orbicular, and depressed in shape, measuring nearly an inch and a-half in its transverse diameter, but not quite an inch in thickness. It was attached by a kind of peduncle to the bladder, the adherent surface not being more than half-an-inch in extent. The exterior was of a pink colour, when recently extracted, and highly crystalline in structure, being composed of crystals of an unusually large size.

On being analysed by Mr. Thomas Taylor, it was found to consist of nearly pure crystalline phosphate of magnesia and ammonia, capping a small mass of the mixed phosphates. The stone has been deposited in the Museum of St. Bartholomew's Hospital.

The above case is chiefly worth recording, on account of the rarity with which a stone is met with adherent to the coats of the bladder. All authors agree that this is very seldom found, and though such cases are alluded to, I can find few or none distinctly recorded.

Sir B. Brodie says, in his 'Lectures on the Diseases of the Urinary Organs'—"You will hear not unfrequently of calculi which adhere to the bladder; but you may be assured, nevertheless, that this is a very unfrequent occurrence. Ask all experienced surgeons, and they will tell you what I tell you now, that adhering calculi are rare. It is not very uncommon to find a diseased bladder, a portion of which is incrusted with calculous matter; but that is a very different thing from an adhering calculus, and not likely to be mistaken for a stone in the bladder." (Edit. 3rd, p. 260.) He then goes on to explain how this incrustation of the bladder takes place, but he says nothing respecting the mode in which calculi can become adherent; except in one case, in which he removed a stone which was fixed to the inner surface of the bladder, and arose in consequence of an original malformation; the calculous matter having formed round a small portion of bone, and two imperfectly formed teeth.

I am indebted for the most probable explanation of the mode of attachment of the calculus to the bladder that can be offered, during the life-time of the patient, in the case I have related, to my friend Mr. Paget, of St. Bartholomew's, to whom I sent the stone. He says in a note which I received from him:—"I think it very likely that the broken part was once continuous with another portion contained within a pouch of the bladder, and of course remaining there still. The mass removed is too heavy to have been held in position by mere adhesion with incrusted calculous matter." The stone had probably been once entirely encysted, and then produced comparatively little irritation; but one portion of it being exposed to the action of the urine, had gradually increased in size by the successive deposition of crystals on its surface, until it gave rise to so much pain and general disorder of the health, as to lead to steps being taken for its removal.—*Med. Times and Gaz., Aug.* 12, 1854, *p.* 158.

88.—ON LITHOTRITY.

By Dr. S. Fenwick, Newcastle.

[We ought first to consider to what class of cases lithotrity ought to be confined. In many it is inadmissible. Dr. Fenwick endeavours to show from his own experience when and how it ought to be performed. The following advantages, in many cases, are most important.]

1st. Instead of lying for weeks upon the back, with the urine dribbling from the wound, none of these persons were confined to bed beyond a few days. And although, by the use of chloroform, we can remove all suffering during lithotomy, we cannot prevent the constant soreness and pain that follow the use of the knife during the many weeks succeeding it.

2nd. There was no danger incurred of important parts being wounded, such as the rectum or the vasa deferentia, as there would have been had the patients been subjected to the older operation.

3rd. The cure was more rapid. Thus, in one case, only a few pieces of a large stone remained at the end of a fortnight, and in three or four weeks the man was able to work. After lithotomy, however favourably the patient has progressed, it is long before his former strength is regained; but after successful lithotrity, he often improves after each operation, and, as soon as the stone is removed, he is frequently able at once to return to his employment.

4th. The danger of death is much less after lithotrity. It must be remembered that all the above cases were men between 30 and 65, at which ages a glance at the accompanying table will show that the chances of death are exceedingly great after the lateral operation.

	1 to 10	11 to 20	21 to 30	31 to 40	41 to 50	51 to 60	61 to 70	71 to 80
Number of cases	1,466	731	205	141	123	161	126	19
Number of deaths	112	71	31	24	27	44	39	7
Average mortality	1 in 13·08	1 in 10·20	1 in 6·61	1 in 5·83	1 in 4·59	1 in 3·65	1 in 3·23	1 in 2·71

I have operated with the lithotrite between fifty and sixty times, and with one exception, have never seen dangerous symptoms. Indeed, I have never seen any unpleasant results, excepting in cases in which my own imprudence and want of skill has caused them. I am perfectly willing to allow, that, as each stone usually requires five or six operations before its removal, the patient's risk is greater than the above statement may at first appear to convey; but we have it so entirely in our power to see him in a fit state before each operation is attempted, and to do much or little, according as he is able to bear it, that I consider the chief matter in the discussion is to show that the placing of a lithotrite in the bladder, and the crushing of a stone in that cavity, is an operation which, under certain circumstances, is attended with but small risk of dangerous consequences.

But it must be borne in mind, that these advantages are only to be expected from lithotrity under certain circumstances; and it is, therefore, a matter of the greatest importance that it should be clearly determined when these exist. The cases I have above quoted as perfectly successful, agree in the following conditions:—

1. The ages were between 30 and 65; there was, therefore, neither the excessive irritability of youth, nor the debility of old age, to contend against.

2. The urethra in all these cases was capable of admitting instruments of considerable size, and consequently, of permitting the ready passage of the fragments of the calculi. The bladder was healthy, and although in some cases very irritable, was in all able to retain six or eight ounces of fluid. The kidneys were in a natural condition, and there was, consequently, less fear than in another class of cases to which I shall shortly refer, of inflammation following the operation.

3. In all, the calculus was loose in the cavity of the bladder; in each it was readily crushed; and in no instance did its diameter exceed an inch and a quarter.

Where the above conditions co-exist in any case of stone, I believe it ought to be laid down as a surgical rule, that the lithotrite should be employed, and that lithotomy should only be regarded as an amputation is, in a simple fracture of a limb. No surgeon ever thinks of removing a leg for a simple fracture, unless some more serious injury be also associated with the broken bones; and a few cases of such accidents, terminating in gangrene, and eventually requiring an operation, do not shake his confidence in the ordinary rules of treatment. In like manner, it is not reasonable to abandon lithotrity, or the attempt to lay down rules for its performance, because an occasional case, performed under the most favourable circumstances, proves unsuccessful. I am desirous of drawing attention to the necessity of general rules for treatment in this disease; because, in the majority of cases, the above favourable conditions co-exist at one period or another of the complaint: and, if the above rule should prove to be correct, and be generally acted on, few persons above the age of 30 will ever be subjected to lithotomy. At present, however, a large proportion of the cases presenting themselves to the operating surgeon do not present the above conditions, and I will, therefore, attempt further to show under what circumstances lithotrity should still be preferred.

I have had no experience in crushing stones in children, and out of twenty cases of calculus which I have treated within the last six years, only two were under puberty; and as neither of these was a proper case for the use of the lithotrite, I must speak entirely from theory. The general rule appears to be to employ the knife, and, in the majority of cases, I believe it to be correct. We have in children usually the stone relatively larger to the urethra, and, consequently, to the lithotrite, than at a later period of life; so that frequently considerable difficulty must be experienced in reducing the foreign body to fragments in its subsequent expulsion. But, what is more unfavourable to the success of lithotrity, the system is more irritable; thus, I have known in this hospital death ensue in a child simply from sounding the bladder on suspicion of the existence of a calculus. A glance at the table

will also show that, at this period, lithotomy is a very successful operation; so that little advantage is to be expected from the lithotrite. I do not think, however, that this instrument should be altogether abandoned even in children. I have seen calculi of so small a size extracted, that a few movements of the blades would have been sufficient to have broken them up.

The following rule ought, therefore, I believe, to be employed:—Whenever, in children, the stone is so small and soft that two or three operations will be alone required, and where the child is in good health, and of a phlegmatic constitution, lithotrity should be preferred; in all other cases, recourse ought to be had to the knife.

The condition of the urinary organs is necessarily one of the most important considerations before attempting either operation, and no means should be neglected which may enable the surgeon thoroughly to ascertain whether or not they are diseased. I have treated cases with the lithotrite, in which strictures were present; but when the narrowing of the urethra is extensive and permanent, it forms a complete bar to the operation. Where hypospadias exists, with narrowing of the entrance of the urethra, of which I have had two instances complicated with stone, it is better to lay open the part with the knife. In other strictures, if slight, and lower down in the canal, I have employed dilatation alone. It often happens, that after one or two sittings, spasmodic stricture occurs. Whenever this is observed, the surgeon should immediately leave off all operative proceedings for a few weeks, and attempt to lessen the irritability of the passage by medical treatment. The spasmodic action, in itself, is of no consequence, but it is a mark of that irritable condition that often precedes inflammation of the bladder and kidneys.—*Med. Times and Gazette, June* 17, 1854, *p.* 617.

Simple enlargement of the prostate does not prevent the success of lithotrity, unless it is very considerable. I have never operated upon cases of this nature in which there was any very great difficulty in introducing the instruments, and this, I should imagine, will be what is chiefly to be feared. It might reasonably be expected that fragments would be more apt to be retained when the gland is increased in size: but where it is only moderately enlarged, I have not found, in practice, that such is the case. Where there is acute inflammation, or suppuration of the prostate, lithotrity is inapplicable, as every motion of the instrument must bruise the inflamed part, and must tend to produce an extension of the disease to the bladder and the kidneys. In a case of suppuration of the prostate, the knife should be used, not only as the best plan for the removal of the calculus, but also to give exit to the pus. This disease occasionally follows the operation of lithotrity, and if no other contra-indicating circumstance exists, should be always as soon as possible treated by incision. I shall, afterwards, return to the consideration of this point. There are few, if any, cases of calculus, in which some degree of inflammation of the bladder is not present; indeed, the greater number of the symptoms of stone arise from this source. There is no affection, the extent of which it is so important to discover, before determining to which operation the patient ought to submit; and the slightest consideration is sufficient to show this. In lithotomy the bladder is at once opened, the exciting cause of the inflammation removed, and the organ afterwards left at almost perfect rest, which is the state which is the most favourable for its regaining its natural condition; while in lithotrity, a fresh source of irritation is added in each attempt to break up the foreign body, and the number of sharp pieces, which are the result of those proceedings, often add to the inflammatory action. It is evident, therefore, that the stone should never be crushed, when the cavity in which the manipulation must be performed is in a state of acute inflammation. This, I believe, is the most important rule which can be laid down respecting this operation, and the success of every surgeon will depend, chiefly, upon his strict adherence to it. As an illustration of the effects which sometimes, though not always, follow an infringement of it, I append the following case.

Case.—A man, about 60, who had been affected with symptoms of calculus for many years, consulted me respecting them. A sound was introduced, and a stone discovered, which I conjectured to be of no great size, and to be composed of lithic acid. The urethra was healthy, but the bladder was inflamed and irritable. I put him under medical treatment for a few weeks; but, at last, yielded to his desire for the operation. The bladder would scarcely contain more than two or three

ounces of water, and only a very small piece of stone was chipped off, the instrument, which was a badly formed one, slipping two or three times. The pain in passing urine was considerably increased after the operation; but, by rest and treatment, he was able in a short time to walk about again. As he was anxious to return home, he again urged me to operate, and I unfortunately consented. The bladder was on this occasion so much inflamed as scarcely to bear any water; but I passed the lithotrite, and crushed a fragment or a small stone, while the organ was empty. The pain was greatly increased after this; the pulse became quick, the tongue foul, and the digestion greatly deranged. The urine was albuminous, depositing a large amount of pus and lithate of ammonia. He improved in about a week after the operation, and I recommended him to return home for a short time. He was exposed to cold when waiting for the steamboat, and, on his reaching home, was attacked with shiverings. He became much worse; the pulse became quicker, and the urine more purulent, while the emaciation of his body rapidly increased. On examination, no tenderness could be found, either in the course of the urethra or the prostate, and no increase in the size of the gland. From these facts, coupled with the other symptoms, I imagined that the cause of his unfavourable condition arose from inflammation of the kidneys. There was no evidence of peritonitis. He died about six weeks after the last operation. Upon examination of the body, the urethra and prostate were found healthy; the bladder was thickened and inflamed, and contained the stone. Both kidneys were very large, and so soft as to tear in their removal. The capsules peeled off very readily; the surfaces were of a dark red colour, dotted with yellow points, where small deposits of pus existed. The increase in the bulk of the organs arose from enlargement of their substance, and not from dilatation, as is so frequently the case; the lining membranes of the pelvis were thickened and inflamed, and contained pus. There was no peritonitis or other disease apparent.

I attribute the fatal result in this case to two errors—one of operating when the bladder was not fit for it; the other, of repeating the operation too soon; and, as far as I have seen of lithotrity, these are the two usual causes of failure. Although this is the only unsuccessful case in my own practice, I have seen death produced by inflammation of the bladder alone, without any extension of the disease to the kidneys. It is, manifestly, of the greatest importance, that we should have, if possible, some test by which we may ascertain whether or not the bladder is fitted for the operation. Neither the violence of the pain ordinarily suffered, nor the amount of discharge in the urine, is a sufficient indication of the degree of inflammation likely to follow any operative proceeding. I have always found, that the quantity of water which the bladder is able to retain is the best guide for the surgeon. A bladder, violently inflamed, can no more retain water, than an inflamed stomach can bear what is placed in it without vomiting, and violent efforts at expulsion are the results of the attempt in either instance to fill the inflamed cavity. If, in any case, it be found that the bladder is capacious, and able to retain eight or ten ounces of fluid, the operation (providing other circumstances are favourable, and it is properly performed) will be generally successful; but if, on the contrary, the cavity is so much inflamed as not to bear three or four ounces of injection, any attempt to crush the stone will usually prove dangerous. The reason of this is pretty obvious; a calculus produces, by its movements, the same kind of injury to the mucous membrane that the lithotrite does, and if the bruising of the stone has been, for months or years, borne without much injury, there is a prospect that the irritation of the instrument will also cause but little mischief; but if, on the contrary, the calculus alone has been able to excite violent inflammation, it is probable, that the manipulation in the same cavity will further add to its violence. If, in any case, the bladder cannot bear a few ounces of fluid, should the patient be at once abandoned to lithotomy?

The rule which ought to be laid down should be this: that if the bladder will contain four or six ounces of fluid, and the other circumstances are not unfavourable, the stone should be crushed; but, if injection of this quantity cannot be endured, the inflammation should be reduced by treatment; and, in case this cannot be effected, the knife should be employed for the removal of the disease.

It has been much discussed whether lithotrity or lithotomy is best fitted for cases complicated with disease of the kidneys. I suspect that the applicability of either

method of operating depends, less upon the condition of these organs, than upon the state of the bladder. There are, of course, many in which the kidneys are so diseased that the removal of the calculus is of no avail. The stone which I now present was taken from a patient of Mr. Stephens'. The man was about twenty years of age, and had suffered from symptoms of the complaint for many years. When I saw him he was greatly emaciated, and affected with hectic. The urine was purulent, and obstructed in its passage from the bladder. By means of a long pair of lithotrity forceps, the urethra was cleared of a quantity of calculous matter, and the stone was afterwards detected in the bladder. From the belief that the kidneys were incurably diseased no operation was recommended, and he died a week afterwards. Upon *post-mortem* examination the bladder was found healthy, but both kidneys were dilated into large bags, their cortical substance being in a great measure absorbed.

But where the kidneys are not incurably diseased, I think lithotrity, as a general rule, should be employed. Every surgeon must have remarked how frequently disease of these organs produces dangerous consequences in all operations where the knife is used, and how often fatal results ensue under such circumstances from wounds which would otherwise be considered trivial. It must be, therefore, in these cases important to avoid such deep incisions as are practised in lithotomy, as the condition of the patient is likely to produce unhealthy inflammation of the parts traversed in the operation. Wherever, therefore, in a case of stone complicated with diseased kidneys, the bladder is able to bear the injection of six or eight ounces of fluid, I believe lithotrity should be employed, unless some other circumstances should forbid it.

I will make a few remarks upon the treatment which I think ought to be observed after unsuccessful cases of lithotrity.

I have known dangerous symptoms arise from inflammations of the kidneys, of the bladder, and of the prostate gland and urethra; and the affection of each of these parts requires attention. I have seen chronic inflammation of the kidneys follow the operation, and afterwards removed by a repetition of the crushing; but it requires great care, lest, by such treatment, acute disease should be set up. The most perfect rest should be always enjoined, both before and after the use of the instruments, and all irritation of the bladder should be subdued by opium and the warm bath. Acute inflammation of the kidneys is one of the most fatal consequences of lithotrity, and deserves particular remark, inasmuch as the indications of its presence are often overlooked. Frequently, general fever, accompanied by a very white tongue and loss of appetite, is the only symptom presented by the patient; and often the combination of these with great irritability of the bladder misleads the surgeon, and induces him to suppose that it is only inflammation of the bladder against which he has to contend. When such symptoms are present, no operation should be thought of, and all the attention should be directed to allay the disease as soon as possible.

When acute inflammation of the bladder follows lithotrity, active measures should be immediately taken to relieve it, as it may spread upwards to the kidneys, or forwards to the prostate gland and urethra. In any case it prevents a repetition of the operation while it is allowed to exist. If the inflammation should return after every attempt to crush the calculus, the operation ought to be abandoned, and the bladder should be cleared of the stone by lithotomy. Inflammation of the prostate and urethra not unfrequently follows lithotrity, and often terminates in suppuration. Whenever dangerous symptoms follow the operation, a careful examination ought to be made through the rectum, as serious disease of these parts may exist without the general symptoms being sufficient to direct the attention of the surgeon to it. As soon as suppuration has taken place, it ought to be the rule to employ lithotomy, as both the abscess is thereby opened, and also the bladder is at once relieved from the stone. It is, I believe, of little use in such cases to wait with the hope of repeating the operation of crushing, as the movements of the instruments will necessarily irritate the parts which have already been inflamed. The following well-treated case of Mr. George Heath's illustrates these points, and shews the practice which, I believe, ought always to be carried out.

Case.—A man, about twenty years of age, consulted him with the ordinary symptoms of stone in the bladder. An attempt to crush the calculus failed, except-

ing that a few fragments were scraped off with the lithotrite. The patient went on well for a week, when a severe attack of inflammation came on, which continued for several months. Finding that he was gradually sinking, Mr. Heath resolved to perform lithotomy. An abscess of the prostate was opened in the operation, and a mulberry calculus extracted from the bladder. He was immediately relieved of all his symptoms and rapidly recovered.

The same plan should be adopted where abscess forms in the perineum, and when inflammation of the urethra or prostate gland seems to cause it; for not only do such patients usually recover, but it is generally inexpedient to irritate the parts again with the lithotrite.

With respect to the method of performing the operation, I will merely direct attention to such points as I have either seen errors committed in by others, or in which I have committed errors myself. I have before frequently mentioned the importance of regarding the capability of the bladder to retain fluid as a test of the applicability of lithotrity; but this rule requires some reservation. In many instances a patient forces out the injection only from nervousness, and not from any real incapacity to retain it. It is, therefore, advisable, in all such cases, to have an intelligent assistant, who may keep him in conversation while the operator very slowly and gently fills the bladder. By this means I have frequently been able to operate where at first I almost despaired of success. It is often useful to have the urethra grasped with the hand, so as to prevent the escape of the water, otherwise many, from impatience, will gradually empty the bladder during the operation. I have seen many cases, from neglect of these precautions, rejected, as unfitted for lithotrity, whose calculi, I feel perfectly sure, might have been easily crushed. Although I lay such stress upon fully injecting the bladder, it is only for the reasons above given; for I have many times seized stones in bladders perfectly empty, and such attempts are to be condemned rather on account of their liability to produce subsequent inflammation, than from any fear of doing injury during the operation.

Many writers upon lithotrity have been in the habit of treating the fear of catching the mucous membrane in the blades of the instrument as only theoretical. This is not the case, as in one instance of an old man, I repeatedly caught the sides of the bladder, but always discovered the mistake by the soft feeling experienced on attempting to close the lithotrite. In order to avoid this mischance, which will only occur when the stone is not seized, it is only necessary to be careful, in not thoroughly closing the blades after each unsuccessful attempt to include the calculus.

After injection, the surgeon should always, by suddenly changing the patient's position, attempt to throw the stone into the back of the bladder, as not only are the necessary manipulations more readily performed in that part, but he thereby also avoids the risk of bruising the prostate, and the anterior part of the bladder, which is the most sensitive and the most susceptible of inflammatory action. In many cases, where the stone is rather large, it will be found lying near the anterior part of the organ, and inclined to that side on which the patient is in the habit of resting. It is, therefore, often of advantage to direct him to change the side on which he has usually slept, as a weighty stone often forms a depression of the bladder in which it remains. Before passing the lithotrite, the exact position of the calculus should always be ascertained by the catheter, in order that as little sounding as possible may be used with the heavier instrument. The lithotrite should be passed on at once to the stone, without any of those thumpings of the bladder which are so frequently used in ascertaining the existence of a calculus. The only advantages which can be obtained by striking a bladder with a sound or lithotrite are, either to assist the surgeon in feeling the stone, partly or wholly concealed in folds of the mucous membrane, or to enable him to dislodge it from some position it occupies. Both of these can be obtained by very gentle motions, and the slightest violence to a sensitive organ like the bladder cannot be too much condemned. The blades should never be opened until the instrument is in contact with the stone in the fundus of the bladder. This I would direct attention to more particularly, because I have seen them attempted to be opened before the instrument had entered the bladder, and prostatic inflammation was the result. There should be no fishing for the stone in a random manner, as is so often done, but the

attempts to enclose it should be conducted upon a proper principle. In small or very moveable calculi, it is the best plan to pass the closed lithotrite to the stone, to open the blades, and, placing them as near as possible at right angles to the axis of the stone, to make gentle pressure upon the bladder immediately beside it. This, of course, makes the part of the bladder depressed by the instrument lower than that portion occupied by the calculus, and it therefore drops in between the expanded blades. Where the stone is so large or so heavy that it does not move easily, it is the best plan gently to insinuate the lower blade below it, and thus seize it. In all cases, however, it should be grasped as much as possible near its centre, as it is thereby more rapidly broken up. Impaction of the fragments not unfrequently takes place, and should be treated according to the nature of the obstruction. Single pieces which are stopped by their size or angularity are best removed by the long forceps, such as I now show; but, where it is caused by large quantities of soft calculus, it is better to pass a large-sized catheter down to the part, so as to dilate the urethra, and make the patient force his urine, while the instrument is gradually withdrawn. In crushing the stone, the upper blade should be always pressed down by the finger of an assistant, while the operator uses the screw. If this is not done, the outer layers are very apt to break off, and the remainder of the calculi to fall from between the blades.

As regards the lithotrite, the chief want is some mechanical contrivance which will subject the stone, when once caught, to the repeated action of the screw. But in the instruments as usually constructed, there are some faults which ought to be avoided. The lower blade is often too much curved instead of being flattened. By the curve there is, it is true, greater strength, but the stone is not so readily grasped as when it is flatter. In the upper blade the teeth should be always so placed as to present a sharp surface at either edge, so that, if the stone be grasped obliquely, it may still be held in its position.—*Med. Times and Gazette, June* 24, 1854, *p.* 641.

89.—NEW OPERATION FOR VESICO-VAGINAL FISTULA.

By Dr. Wagner, Dantzig.

The margins of the fistula are left untouched; the operation is commenced by performing two semilunar incisions meeting at both extremities, so as to surround the whole periphery of the defect. These incisions are made about a line distant from the edge of the fistula (or even further, according to the extent of the cicatrix tissue), and penetrate through the whole depth of the mucous membrane of the vagina. The operator then separates the mucous membrane from the submucous fibro-areolar tissue to the extent of a line or more all around the fistula, commencing from the incisions above-mentioned, so that two flaps of mucous membrane result, one on each side.

It will be easily understood that four flaps are now formed, two on each side of the fistula: 1st. The two last-mentioned, formed of mucous membrane, the free margins of which, meeting at both extremities, correspond to the incisions with which the operation commenced. I shall call these the external or inferior flaps. 2ndly. Two flaps, consisting of the other tissues of the vagina, the layer of fibro-areolar tissue with organic muscular fibres, and the external layer of fibrous tissue with numerous elastic fibres, and very vascular. The free margin of these flaps is formed by the surrounding margins of the fistula; these internal, or, according to the position of the parts and the anterior wall of vagina, superior flaps, are broader than the external ones by the distance in which the first incisions were made from the margin of the fistula. It is important to remember this fact for the third stage of the operation. The sutures are then applied as follows:—

The sutures at both extremities are merely passed through the flaps of mucous membrane; all the other sutures are first passed through the external flap on one side from above downwards, then through the internal flap in the same direction; the needle is then passed through the same parts on the opposite side in inverse order, from below upwards; all four flaps being thus taken up by each suture. The sutures being drawn tight, the double closure is obvious. The parts brought into close apposition are the raw margins, and raw submucous surfaces of the external

flaps, and the inferior raw surface of the interior flaps, the margins of which (they being broader than the external flaps) are turned inwards towards the cavity of the bladder, and, being closely approximated, effectually prevent the urine from coming into contact with the parts below.

I will leave you to judge of the merits of this proceeding, and only add that it is not well adapted for large fistulæ, and quite impracticable in cases where the tissues surrounding the fistula are indurated to a great extent, as the first incisions must be performed on healthy mucous membrane. I do not think that a great number of cases have been operated on in this manner as yet, so that I can offer no judgment as to its comparative merits, except theoretical deductions, which certainly speak in its favour. I think that this description is the first hitherto attempted.—*Med. Times and Gazette, May* 6, 1854, *p.* 468.

90.—ON SPERMATORRHŒA.

By John L. Milton, Esq.

[One of the most celebrated writers on this subject is Lallemand. In the treatment of spermatorrhœa he advocates the application of nitrate of silver to the neighbourhood of the vesiculæ seminales. Mr. Milton seems to differ in opinion from this eminent author, and the following remarks seem to prove that Lallemand was not always correct. Mr. Milton, however, thinks that a granular or strictured state of the posterior part of the urethra from gonorrhœa may require caustic, or that irritation of the urethra caused by the spermatorrhœa may require the bougie, or nitrate of silver, but that this is a different thing from using the remedy for spermatorrhœa itself.]

It has been urged that there is often an ulcerated, inflamed or injected state of the mouths of the seminal ducts and the neighbouring part of the urethra, and that so long as this is kept up, irritation will be conveyed to the seminal vesicles, and, as a result, spermatorrhœa will be maintained; and this assertion has been repeated again and again, as if it could gain strength by repetition. But what in the first instance caused this irritable ulcerated state? Was it the first link in the chain of disease? Clearly not; the urethra is quite healthy at the beginning of spermatorrhœa; it is only when it has reached a very advanced stage that the extreme tenderness of this canal, and the redness of the orifice, are met with, and not always then; I have seen it in its worst form without any apparent change, and unaccompanied by tenderness. It is self-evident, then, that it is either an effect or a complication of the spermatorrhœa, and to treat the disease here by beginning with its effects would be exactly the same as if a surgeon, in a case of irritable bladder in the female, were to limit his treatment to cauterizing the excoriations caused by the constant flow of urine over the vulva; or if a physician were to cauterize the rectum when diarrhœa produces heat and smarting there.

But the advocates of cauterization point to its success; one fact they say is worth a hundred theories: thousands of cases have been cured by it, &c. To which, however, they might add, that thousands of cases have not been cured by it, which might have been cured by other means; that where it succeeds, it is from its counter-irritant action, and that this action may be much more surely obtained by the use of gentler and safer counter-irritants.

Then, again, they say, what is to be made of those cases of spermatorrhœa caused by gonorrhœa? Does not the disease ensue in these cases from the gonorrhœa spreading backwards and settling in the posterior part of the urethra, where it sets up an irritation communicated to the seminal vesicles, whence arises spermatorrhœa?

Plausible as this style of argument may appear, it rests on a train of statements which have yet to be thoroughly examined. In the first place, when the action of gonorrhœa extends to the ejaculatory ducts at all, so that its presence can be determined, either the suppurative inflammation spreads, and then it rarely stops short of producing swelled testicle, or it is a rapid transient irritation, like that producing chordee and irritable bladder, which, though it may aggravate, can rarely, I think, bring on spermatorrhœa. In the second place, it has to be shown that the seminal vesicles have anything to do with this disease.

There are certainly cases in which the gonorrhœal action is kept in the posterior part of the urethra, and where at the same time the spermatorrhœa has become worse after the infection has taken place. Here cauterization may be productive of benefit. I have succeeded with it myself; but I have succeeded still better with other means, and in more than one instance, when I had proposed first to blister the penis, and then to resort to the caustic, found the latter no longer necessary after the blister had been applied. As there can only be two sides to a question of this kind, it follows either that the arguments I have used are wrong, or that the invariable employment of caustic, on the opinion that it is a specific for spermatorrhœa, must fall to the ground.

Gleet of the Prostate and Seminal Vesicles not a form of Spermatorrhœa.—Mr. Hunter, the Newton of medicine, whose true merits, to my thinking, even overshadow those of the giant of the physical sciences, says:—"Diseases of the vesiculæ seminales are very familiarly talked about, but I never saw one. In cases of very considerable induration of the prostate gland and bladder, where the surrounding parts have become very much affected, I have seen these bags also involved in the general disease, but I never saw a case where they appeared to be primarily affected." So far as I can learn, all other trustworthy observations confirm this view. I have never heard of nor seen a case in which disease of the seminal vesicles alone was detected; in a few rare instances they become mechanically involved by the spread of the destructive action, but they generally remain free in the most extensive disease, either of the urinary or generative organs.

The principal argument made use of to prove that the seminal vesicles are the receptacles of the semen, is the presence of spermatozoa or zoo-sperms in them. M. Lallemand, on examining thirty-three bodies, found spermatozoa in the seminal vesicles of thirty of them; but only in the testicles of two, one of whom had died from a fall, the other of gastro-enteritis, which he thinks would go to show that these animalcula are formed in the testes, and then pass into the seminal vesicles. The next argument is, that small, brilliant, granular bodies are found in the urine of spermatorrhœa patients; that they are met with in the masses of mucus squeezed out by these patients after going to stool; and as they are found in the seminal vesicles, *of course* these are the receptacles of semen. These are also met with at all times in the semen of healthy men, and in great abundance in that of birds just before the testes become ripe. The third is, that spermatozoa are wanting or few in the organs of castrated persons.

Mr. Pritchard says, "The molecular motions of Dr. R. Brown—viz., those seen under a deep magnifier in a drop of water, in which finely divided gamboge or other organic substances have been triturated; these motions have been compared with the spermatozoa of animals and plants, *which are now considered as physical motions only.*" Here, then, we have the alpha and omega of scepticism and credulity; the one elevating these little cells—for they are nothing more—into the essential part of the most important of all secretions, the other viewing them as *a mere appearance*, produced, I presume, by causes acting from without.

In cases where the generative power seemed quite lost, the testes having secreted no semen for a long time, I have found the vesicles containing their usual fluid. Among other observations, I may mention that last year I dissected with great care the generative organs of a man who died in St. Luke's Workhouse, at the age of eighty-five. The testicles had long performed no function at such an advanced age; they were very pale and somewhat wasted; the vas deferens was permeable, but very small, and its walls rigid; but I could observe no difference in the appearance and contents of the seminal vesicles from what I had noticed in young people.

It is asserted that the discharge which takes place in one form of gleet—viz., that of a thick mucus after going to stool or passing urine, is semen; that it comes in great part from the seminal vesicles; and that the disease is consequently a form of spermatorrhœa. Speaking of this view, Mr. Hunter says, "First, we may observe the discharge in question is not of the same colour with the semen, and is exactly of the colour of the mucus of the prostatic gland and of these bags (the seminal vesicles). It is not of the same smell, and indeed it has hardly any smell at all. The quantity evacuated at one time is often much more considerable than the evacuation of semen even is, and it happens more frequently than it could ever do were the discharge semen. It is a disease which often attacks old men, where one could

hardly suppose much semen to be secreted; and we find that those who are affected with this disease are no more deficient in the secretion and evacuation of the semen in the natural way than before they had the disease. If the mind be at ease, this will take place immediately after a discharge of semen, as well as before, which could not be the case were it semen. Further, if those that labour under this complaint are not connected with women, they are as subject to nocturnal discharges from the imagination as persons who are perfectly sound."

This close and comprehensive reasoning shows the depth and grasp of Hunter's clear, broad mind; and it is only surprising to me that the tissue of errors stated by M. Lallemand on this subject should have been admitted, after the truth had been set on so secure a basis by Mr. Hunter. But then we are told this substance has the peculiar smell of semen when rubbed between the fingers. Unfortunately for this part of the argument, it happens that the true semen has no smell! The semen, *when ejaculated*, has a peculiar odour, *but then it is mixed with the secretions of the vesiculæ seminales, the prostate, and Cowper's glands.*

Of one thing I am quite sure, persons subject to both kinds of discharge have no sensation when this mucus or vesicular gleet, as I shall henceforth take the liberty of calling it—is passing away, except that of a bulky body going along the urethra, and yet when they have emissions, are conscious of the usual sensation. This gleet may occur without seminal emissions, these without it; when both co-exist, one may be cured quite independent of the other. The microscope, it is said, gives proof that it is semen by making visible the spermatozoa; these cells, however, are found in the vas deferens, and it is possible some few may be ejaculated at the same time the vesiculæ seminales are emptied.—*Lancet, April* 29, 1854, *p.* 467.

91.—ON SYPHILIS.

By William Acton, Esq.

[There is a great difference between what is called the *infecting* chancre which is an *indurated* sore, and the *unindurated* sore. The former communicates constitutional syphilis, the latter does not. The infecting indurated sore ought to be treated by mercury, the *unindurated* form need not be so treated. Hence the difference of opinion amongst medical men is often founded on a misapprehension. Mr. Acton says:]

In accordance with the principles I learnt in Paris, I have always advocated treating simple unindurated sores without mercury, and I hope my writings have discountenanced the too general employment of that mineral; but private practice during the last twelve years has more fully confirmed my opinion, that the indurated chancre cannot be treated without mercury; and Ricord corroborates, with his sanction, the universal treatment of the infecting chancre with a long-continued course of mercury, and he thinks that it is of little use employing this valuable remedy unless it is continued for some months, not only for the purpose of removing the induration which attends this form of chancre, but for guaranteeing the system against the occurrence of secondary symptoms, which inevitably will occur unless the surgeon perseveres in its use long after the healing of the sore.

Within the last few days a case strongly corroborating the necessity of giving mercury in proper doses has come under my care:—A fair-haired man came to consult me, with spots of syphilitic psoriasis on his body, and most severe pain on one side of his head, together with an indurated sore on the penis, with its usual attendant, chronic enlargement of the glands of the groin. The patient told me that he had contracted the disease three months ago, when he was treated by a homœopath with the usual small doses of mercury. About a month ago the mercury had been left off, and potash had been given, but without any benefit; and, his confidence failing, he had come to consult me. Here, then, is one out of many cases I could cite of the little good of small doses of mercury.

I may mention that the doctrine of treating indurated sores with mercury appears to be generally received as the recognised treatment in Dublin, where formerly the

opinions of Carmichael were generally admitted—viz., that most of the forms of syphilis could be cured on the non-mercurial plan.

In France, at the present day, salivation is considered rather an unfavourable circumstance than otherwise; and as soon as this symptom arises, mercury is left off, to be resumed the moment the gums cease to be swollen. But it is a curious fact, that it is often a very difficult matter to induce salivation in the ordinary cases of indurated chancres; and notwithstanding the considerable doses of the mineral, and the long continuance of the treatment, the action of mercury is shown rather by the disappearance of the symptoms of syphilis than by any affection of the gums, persons labouring under induration apparently showing little tendency to salivate.

The preparation still most commonly employed in Paris is the proto-iodide of mercury, to the exclusion of almost all the others. In London I still, however, find it as objectionable as when I wrote the second edition of my book, when I stated that this preparation usually produces colic in the damp, cold climate of England, consequences which have caused me to exclude it from my own practice, and obliged me to prefer frictions, blue-pill, or the grey-powder.

The Phagedænic Chancre.—I saw several of these sores in the course of treatment by a new plan which Ricord has lately introduced into practice—namely, by destroying the whole of the unhealthy surface with a red-hot iron, having previously placed the patient under the influence of chloroform. Cases which formerly resisted every form of treatment, and lasted many months and even years, are now readily cured by the actual cautery in a short time, the surgeon taking care to cover the surface with strips of ammoniacum and mercurial plaster after the sloughs have separated; the internal treatment still consisting in giving the tartrate of iron, which succeeds alone in the less obstinate cases. In private practice, cases requiring the use of the red-hot iron, are very rare, but in hospital practice they are more frequently met with, and we occasionally witness cases, several inches in circumference, extending at one corner, while they imperfectly heal in the other; these are called serpiginous sores. The variety which we meet with most frequently in London is the phagedænic sore, attended with induration. This is a form not unfrequently seen in consultation, and in which the actual cautery is of no avail, but which must be treated by mercury, at the same time the system is supported by bark, wine, or steel. This variety is attended with the worst forms of secondary symptoms taking on the character of secondary ulcers, rupia, and the bones are early attacked, and I have met with it much more frequently in London than in Paris, influenced probably by food and climate, which are found to be such active agents in causing the diversity we notice in the forms of syphilis.

Constitutional Syphilis.—Little doubt can, I think, exist, that all the milder forms of secondary symptoms are as common in Paris as in London. In going through the French hospitals we witnessed all the varieties of that Protean disease, but we saw but few of those severe and afflicting cases that we daily witness in London, and the aggravation of the forms in London we attribute in a great measure to the climate, food, and intemperance which is commonly met with in England. A case came under my notice, within the last few months, which strongly impressed me with this opinion. A nobleman was travelling in Spain, and at Seville, after connexion with a Spanish lady, he observed what he considered to be an excoriation. As this gave him little inconvenience, he merely applied some lint and water, and continued his journey. The little sore remained for some time *in statu quo*, as long as he was subject to the climate of the continent; but in a few weeks after his return to this country he became alarmed at the increase in the sore, and consulted me, when I found a hard, indurated mass, which was very characteristic of the true infecting chancre, accompanied with well-marked psoriasis palmaris. This gentleman's health was still excellent when he consulted me, and I felt fully convinced that had he remained in the south these constitutional effects would not have occurred; and I mention the case as illustrating the inexpediency of contrasting syphilis and its effects, with or without treatment, in different countries, as what is true in the south may not be verified in the north; and I can hardly believe that constitutional syphilis is more amenable to the non-mercurial treatment in Scotland than in England. Those interested in investigating the subject further, I must refer to my last edition for further curious details.

Another peculiarity that deserves the attention of the English surgeon, is the

frequent occurrence of secondary ulcers in London as compared to Paris. In the latter capital, these late constitutional affections are very uncommon; with us, in England, they are frequently met with; and the previous treatment, with or without mercury, appears to have nothing to do with their occurrence. I should further say that in London there is a great tendency, in many of the forms of constitutional syphilis, if left to themselves, to take on an ulcerative form.—*Lancet, July* 8, 1854, *p.* 7.

DISEASES OF THE SKIN.

92.—TREATMENT OF ERYSIPELAS.

By Dr. James S. Hughes, Dublin.

[Early in the attack of erysipelas, when the pulse is hard and full, without gastro-intestinal irritation, Dr. Hughes gives an emetic, but this must be done very early in the case. He prefers tartarized antimony, either alone or combined with ipecacuanha. He then gives the tartarized antimony in solution, in minute doses at long intervals. As soon as the fever and erysipelas begin to abate, he commences with bark and nutritious diet, combined with wine or brandy. The most important point to decide, is, when to commence with wine or brandy. Dr. Robert Williams, of London, commences with wine very early in nearly all cases of this description, and we think justly so, as the affection is one which has more frequently a malarious origin than not. Dr. Williams puts his patients on a milk diet and gently opens the bowels, and then always allows them from four to six ounces of port wine daily. On this subject Dr. Hughes remarks:]

In the administration of wine in any given case of erysipelas, you ought, however, in my mind, to be guided, both as to the period of giving it and the quantity required, by the age, constitution, and habits of life of the patient, and by the type of the fever; thus, if you are called in the early stage of the disease, and if a high degree of fever exists, instead of pouring in wine at once, you should first unload the stomach and bowels by suitable medicines, and exhibit diaphoretics, after which wine may, according to my experience, be given, with greater safety and advantage: if, on the contrary, this first stage should have lapsed, or the attack has been ushered in by a low form of fever, the sooner you order wine the better.

In some cases, lately under our observation, we found six or eight ounces of wine during the twenty-four hours, sufficient; whereas, in others, as you are aware, it became necessary to order more than double that quantity within the same period. In hospital practice, where the patients have been more or less accustomed to the use of ardent spirits, we have found brandy, or malt whiskey, in suitable quantities, to be better stimulants than wine. In cases of erysipelas in which the patient loses his power of swallowing, liquid food should be introduced into the stomach by means of the œsophagus tube; or, nutritious and stimulating enemata should be administered to him at proper intervals; for, we should ever hold in recollection, the possibility of persons recovering from this disease under the most unpromising circumstances. And here, let me impress on you the vital importance of sedulously watching, in all these cases, the state of the patient's bladder. In my first lecture I told you, that although patients in the advanced stages of erysipelas frequently labour under incontinence of urine, yet, that retention of urine sometimes occurs in the latter stages of the disease; therefore, examine the condition of the patient's bladder, at each of your visits, not trusting to the nurse's report who may, possibly, mistake stillicidium urinæ for incontinence of urine; and should retention have taken place, you will of course introduce a catheter to unload the bladder, and have the operation repeated at proper intervals.

Bronchitis was, as you are aware, a prominent symptom in Austin's case, and in him we found support of the system by wine and light nourishment, in judicious quantities, as necessary as in the low forms of external erysipelas. In this variety of bronchitis, the application of sinapisms and blisters proved highly beneficial.

In the local treatment of the lighter forms of true erysipelas, the application of finely levigated flour, or hair powder, affords much comfort to the patient; in some instances, especially when the disease is situated in the lower extremities, the use of

the nitrate of silver, as recommended by Mr. Higginbotham, appears to limit the disease; in more severe cases, where the vessels of the skin are in a highly congested state, puncturing the surface freely with the lancet, as proposed by Sir Richard Dobson, affords relief; in a few instances, we have found the solution of sulphate of iron, as recommended by M. Velpeau, apparently of some advantage; and both Dr. Snow, of London, and Mr. Busk, of the Dreadnought, speak favourably of the employment of collodion to the surface of true erysipelas.

The topical remedy, however, which proved especially useful in the most unpromising cases of the prevailing erysipelas, was the employment of mercurial ointment to the inflamed surface. The mercurial inunction requires to be used in these cases with considerable caution; the diseased surface should be covered with the ointment, and the application renewed at proper intervals; but if we do not watch the effects of the inunction with care, the patient may, to our great annoyance and his discomfort, become suddenly hyper-salivated; in Robertson's case, three drachms of mercurial ointment had scarcely been used to the face and forehead, in divided portions, and at short intervals, when salivation set in, but from that moment all his symptoms improved, although we had previously looked on his case as one of the most unlikely to recover that we had met with during the session. Whilst using the mercurial ointment, externally, in erysipelas, we must not lose sight of the constitutional treatment—*Dublin Hospital Gazette, May* 15, 1854, *p.* 116.

93.—*Arseniate of Iron in Herpetic and Squamous Eruptions.*—M. Duchesne Dupare has read a memoir on this subject before the Academy, at the conclusion of which he advances the following propositions as the result of his researches:—

1. Arseniate of iron possesses, in common with all other arsenical preparations, unquestionable remedial properties, applicable to the treatment and cure of herpetic and squamous affections of the skin.

2. The great advantage of that substance is, that it may be administered in sufficient doses, without giving rise to any of the consequences with which various other arsenical preparations have been justly reproached.

3. The arseniate of iron, whether given singly or in combination with other substances, ought always to be administered in graduated doses commencing from one-twentieth, one-tenth, or even one-fifth of a grain, according to the age, the constitution, and above all, the state of the digestive organs of the patient.

4. Numerous facts accurately observed authorize M. Dupare in concluding, that a daily dose of one-fifth of a grain of arseniate of iron, uninterruptedly repeated during the necessary time, is competent in the adult to effect the cure of an herpetic or squamous affection, however extensive or long-established.

5. No absolute rule can be laid down with respect to the duration of the anti-herpetic treatment by arseniate of iron, for this must vary in accordance with the differences of age and constitution, the extent and severity of the disease, and more than all, perhaps, in proportion to the degree of toleration which the digestive organs manifest for the remedy.

6. An anti-herpetic treatment by arseniate of iron in no degree excludes the employment of topical remedies of acknowledged utility, and it is materially assisted by the internal or external use of certain non-sulphuretted mineral waters. MM. Serres, Andral, and Rayer have been commissioned to inquire into the merits of this new remedy.—*Gaz. des Hôpitaux. Dublin Hosp. Gazette, Sep.* 15, 1854, *p.* 255.

94.—*Use of Tincture of Iodine to prevent Pitting in Small-Pox.*—Dr. Crawford, Physician to the Montreal General Infirmary, advocates the employment of a saturated solution of iodine in spirits of wine, as a powerful means of allaying irritation, and also of preventing pitting; he brushes the tincture freely over the face once or twice daily, from the earliest period of the eruption in which it is practicable, and directs this treatment to be persevered in during the maturation of the pustules. The earlier the application is commenced, the more efficacious he considers its influence over the disease; the immediate effect produced is pain, which speedily subsides,

and the relief which then ensues to the sense of itching is so manifest, that of late he has extended his application more generally over the body. A series of cases, treated in this manner, are detailed by Dr. Crawford, occurring in his own practice and that of his friends; of which the most interesting is a return of an epidemic among the Iroquois Indians at St. Regis, in 1849, treated by Dr. Bergin: he states, that "the entire face was painted in eighty-five cases of confluent or semi-confluent small-pox, out of which only seven exhibited any marks, and these were slight; half the face was painted in seventy cases of grave disease, of those sixty-one were free from any marks on the painted side, five were badly pitted, four slightly on the painted side, while the unpainted side had numerous marks and pits, exhibiting a very striking and marked contrast." Three hundred cases, in all, came under Dr. Bergin's care, of which two hundred were very severe, either being confluent or partially so: there were but eight cases of modified variola observed. Twelve died during the epidemic, one of whom suffered from a hemorrhagic type of disease.—*Montreal Monthly Journal, Nov.*, 1853.—*Dublin Hospital Gazette, Feb.* 1, 1854, *p.* 16.

95.—*On Water-Strapping as a Surgical Application.* By C. HOLTHOUSE, Esq. —[Mr. Holthouse says, that wet-strapping is superior to ordinary plaster in ulcers of the leg. He maintains for it the following advantages:]

1st. Its innocuousness, being entirely free from the irritating effects of the latter, and never producing inflammation of the skin, or the eruption of pustules or vesicles.

2ndly. The comfort the patient experiences from its application.

3rdly. Its cleanliness.

4thly. The ease and quickness with which it is removed, from its not adhering to the hairs of the part.

5thly. Its cheapness.

6thly. It may be made the vehicle for the application of remedies.

The material made use of may consist of linen or calico, bleached or unbleached, and the older it is, provided it be not rotten, the better it answers the purpose. It must be cut or torn into strips of varying length and breadth according to the part to which it has to be applied; the strips must then be immersed in water till thoroughly saturated, when they are fit for use. If the disease to be treated be an ulcer on the leg, the strips should be about two inches in breadth, and of a length exceeding somewhat the circumference of the limb; they should then be applied exactly in the same manner as plaster, each piece overlapping a portion of the one immediately below it; in fact, the directions given by Mr. Baynton for strapping up the limb may be strictly followed in the application of the water-strapping, save and except that his directions to remove the hair from the part may be dispensed with; a roller must afterwards be applied in the ordinary manner.

Four cases in illustration of this method of treatment, and of its beneficial results, were then given; one being an ulcer of the leg of fourteen years' standing, that had resisted repeated attempts to heal it at other hospitals; another, a case of eczema impetiginodes, affecting both lower extremities, and of three years and a half duration; a third was a case of crushed thumb, followed by gangrene and subsequent amputation; and the fourth was an amputation of the thigh, for extensive disease of the knee-joint and upper third of the leg.—*Lancet, July* 29, 1854, *p.* 52.

96.—*Charcoal for the Prevention of Smell from Gangrenous Sores.*—It is well known that dry charcoal will effectually absorb any noxious or offensive gas which can be made to pass through it. On this power Dr. Stenhouse's disinfecting respirators depend for their efficiency. The difficulty in applying it in hospital practice has, however, arisen from the difficulty of keeping it at the same time dry and in a uniform layer around the part giving rise to effluvia. Mr. Wormald's plan consists in sprinkling freely between two sheets of cotton wool a thick layer of powdered charcoal, and then "quilting" them together in small segments, so that the powder is retained securely in its place. The pads thus prepared may be of any size, according as required to wrap round the end of a stump, or to cover a superficial ulcer.

The sloughing sore having been dressed in the ordinary manner, and a little lint or wool so placed as to absorb any discharge which may flow, over all is laid the charcoal quilt, which is then lightly confined by a bandage. It forms, in addition to its disinfectant properties, a very soft and comfortable envelope, more especially if the sore be in such a part that the patient is obliged to lie on it.—*Med. Times and Gazette, July* 1, 1854, *p.* 11.

97.—*Treatment of Favus.*—From the observation of about a dozen cases of severe favus (diagnosis by the microscope in all) recently treated by Mr. Startin at the Hospital for Skin Diseases, we can speak with great confidence of the efficiency of the following ointment. It is the Ung. sulph. comp. of the Pharmacopœia of that Institution. ℞. Sulph. sublimati lbss.; hydrarg. ammonio-chlorid. ʒss.; hydr. sulphureti cum sulph. ʒss. Leviga simul, dein adde olivæ olei ʒiv.; adipis recentis ʒxvj.; creasoti ♏xx.; misce. To correct the state of general health, Mr. Startin commonly orders simultaneously a mild course of the iodide of potassium, but this, we suspect, has but a small share, if any, in the local result. Often when the scalp has been for many years thickly covered with the peculiar favus crust, four or five nightly applications of the above ointment have sufficed to make it perfectly clean. So long as the patient will continue regularly to use a small quantity every day, the disease may be prevented from reappearing, and the condition assumed by the scalp under its influence might easily be mistaken by the inexperienced for one of complete cure. As soon, however, as the inunction is suspended, the eruption reappears. This liability we have known, in more than one case, to extend over nearly a year, and probably it may for much longer periods. The ointment, however, which does not smell much, need only be applied at night, and may be washed entirely away every morning, so as to entail but little inconvenience on the patient. The hair will, to a considerable extent, grow during the treatment, provided that the scalp has not been too much destroyed. In a most disgusting disease for which as yet no real cure is known, it is much to be in possession of an almost certain means of ensuring its absence. The ointment no doubt acts as a parasiticide. Before its first application it is desirable to clear away the crust as much as possible, either by fomentation or a poultice.

We may remark, that the ointment mentioned is used by Mr. Startin in the treatment of scabies, and also in that of the contagious form of porrigo.—*Med. Times and Gazette, June* 10, 1854, *p.* 594.

98.—TREATMENT OF PHAGEDÆNA.

For a considerable period past, a mild form of Hospital phagedæna has been prevalent in the various London Institutions. In very rare cases have fatal results ensued, and in the majority, beyond the local condition, no urgent symptoms have developed themselves. It has generally attacked superficial wounds, whether those resulting from accidents or operations, and, in a few instances, even stumps have been affected with it. The treatment which has certainly been found of most general benefit has been the application of pure nitric acid. On board the Dreadnought Hospital-ship, where it has prevailed severely, the acid has, we believe, been found most successful. In the London Hospital, Mr. Ward informs us, that he has carefully watched upwards of twenty cases, and in no single one of these has the gangrene spread after the application of the escharotic. In St. Mary's Hospital, some remarkable cures have also been effected by its aid. In a case under the care of Mr. Coulson, a man, in good health, admitted on account of a laceration on the dorsum of the right foot, had the wound so severely affected by phagedæna, that amputation was seriously proposed. The nitric acid was freely applied, by soaking a piece of lint, and then pressing it into the sore; it was allowed to remain seven hours, and completely achieved its intention. The gangrene ceased to spread, the sore took on healthy action, and rapidly cicatrised. Neither in this nor in any one of several other cases in which the dorsum of the foot was the affected part did any sloughing of the tendons follow the use of the acid. It appears to be in cases of simple phagedæna that the local use of nitric acid is most to be depended upon,

since those of syphilitic origin not unfrequently resist its influence. A case of the latter character, occurring in a young and seemingly healthy woman, was lately under the care of Mr. Stanley, in St. Bartholomew's Hospital, and, in spite of all measures, ultimately terminated in death. Nitric acid had more than once been freely applied, and opium, cinchona, chlorate of potash, &c., had been fairly tried. We mentioned briefly, at page 264 of this Journal for Sept. 9, 1854, a case of cancrum oris, under the care of Dr. Baly, in which the acid arrested the phagedænic action after the internal use of large doses of chlorate of potash had quite failed to do so. In the Middlesex Hospital, the favourite treatment for phagedæna is by opium pushed freely until its effects become apparent. Some very successful cases have been cured by this plan. It is observed, that there seems scarcely any limit to the quantity which may be borne, and that, generally, before any narcotic effects become apparent, the sore takes on a healthy action. Now and then the acid succeeds admirably in syphilitic cases; as a rule, however, they seem more amenable to constitutional than local measures. On this account, we but seldom see a simple trial either of the acid or any other one remedy alone. The following case, in which the only change of treatment to which the result could be referred was the application of the acid, is worthy of mention. A prostitute, aged 26, was admitted on Feb. 1, 1853, into St. Thomas's Hospital, under the care of Mr. Le Gros Clarke. She was suffering from a large and deep phagedænic sore on the left labium. The treatment consisted in the use of wine in liberal quantities, and of a draught containing full doses of ammonia and bark. A morphia pill was given each night, and the nitric-acid lotion was applied to the sore. This plan was persisted with for three days, during which the sore slowly spread. On the fourth day, the phagedænic action had much increased, and it was determined no longer to delay the use of the strong acid. The woman was very ill, feeble, and without any appetite. By means of a piece of lint, the fuming nitric was freely applied to the whole sore. Only a few minutes' pain was complained of. On the following day, the sore presented an almost clean surface, and it afterwards rapidly cicatrised. Exactly the same general treatment had been persisted with throughout. This case is an example of what is very often observed, viz. that the pain produced is but very transitory, and is much less severe than it is commonly esteemed.

The most convenient mode of applying nitric acid is by means of a glass brush; in the absence of which, however, a glass rod succeeds very well. To the use of wood or of lint there is at least a theoretic objection, on account of their permitting the charring effects of the fluid.—*Med. Times and Gazette, Oct.* 28, 1854, *p.* 445.

99.—*Treatment of Bubo.* By JOHN HILTON, Esq.—Some cases of suppurated bubo, which have recently been treated by Mr. Hilton, in Guy's Hospital, have well illustrated the efficiency of counter-irritation, as a means of procuring the absorption of pus. The application which Mr. Hilton employs is a solution of nitrate of silver of the strength of one drachm to two of water, with the addition of about three drops of the strong nitric acid. This is painted freely into the skin over the inflamed gland. Its effect is to produce great soreness of the skin and much pain, which is usually followed by rapid diminution in the size of the enlargement. Mr. Hilton informs us, that he has long used the remedy, and now considers the cases of bubo, which require opening, as quite exceptions to the rule. In several, in order to afford proof that the fluid absorbed was really pus, it was examined by a grooved needle, prior to the treatment, the puncture being at once re-closed. Even in those instances in which ultimately it becomes necessary to make an external opening, great benefit in the limitation of the suppurative action, and the absorption of the surrounding hardness, results from the previous counter-irritant treatment. The plan is applicable to many other diseases besides glandular abscesses; in fact, to almost all in which the absorption of recent inflammatory deposit either purulent or fibrinous is desired. We reported a case in the 'Medical Times and Gazette' for Dec. 17, 1853, p. 629, in which it had been effectually used to remove an induration of tissues around the femoral artery, which had so obstructed the vessel, as to cause a tubular dilatation. Superficial abscess in the cellular tissue may also be so treated. Chronic non-suppurated glandular enlargements and swellings of one or other lobes of the thy

roid gland may also be made to diminish by it. It is probable that the nitrate of silver has no particular virtues beyond those of a severe counter-irritant, and might, were it wished, be substituted by other remedies, as, for instance, a caustic solution of iodine. It is, however, a very convenient, manageable, and efficient means, which there is no motive for wishing changed. The substitution of nitric ether as a menstruum, instead of water, would be a great improvement. (See 'Medical Times and Gazette,' for Oct. 14, 1854, p. 394.)—*Med. Times and Gazette, Oct.* 28, 1854, *p.* 444.

DISEASES OF THE EYE.

100.—THE OPHTHALMOSCOPE.

By Dr. E. Williams, Cincinnati.

[The following remarks by Dr. Williams, are forwarded to the 'Medical Times and Gazette,' by Mr. Dixon of Green-street, who seems to have improved the apparatus, by placing the reflecting mirrors in a spectacle-frame, so as to leave the hand at liberty. Mr. Dixon says:]

It has occurred to me, therefore, to fix into a spectacle-frame, to be worn by the observer, a pair of reflecting mirrors, differing from those employed by Coccius and Anagnostakis, only in being of less diameter, and having a smaller central hole. By this contrivance, both hands of the observer are left at liberty, and he is enabled, without assistance, to command the movements of the patient's lids with one hand, while with the other he uses the magnifying-glass.

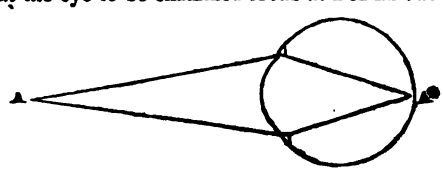
The focus of the mirrors should nearly correspond to the distance at which the observer's unassisted eye can ordinarily define minute objects. I think it better to place the lamp immediately behind the patient, rather than by his side, and at such a height that the rays of light just clear the top of his head. The observer, in making his examinations, will find it best to use only one of his eyes at a time; but two mirrors are desirable, as they balance each other in the frame. When a less concentrated light is preferred, plane glass, silvered at the back, may be used instead of concave mirrors; a small round portion of the silvering being removed from the centre for the observer to look through.

[By this means a strong light can be reflected into the eye, and its interior can be minutely examined.

Dr. Williams proceeds first to describe the principle of the construction of the apparatus:]

Principle upon which the Construction of the Ophthalmoscope is founded.—The pigmentary layer of the choroid is not sufficient to account for the black appearance of the bottom of the eye as seen through the pupil. The pigment would, indeed, absorb all the rays of light that enter the eye, were there no tissues situated anteriorly to it, capable of reflecting a portion of the rays. The vessels of the retina, and the optic nerve at its point of entrance, must certainly have this power. Besides this, however transparent the retina may be supposed to be during life, a membrane so brilliant must necessarily reflect a large number of the luminous rays that fall upon it.

Why, then, does the Pupil ordinarily appear Black? It is only in the refracting properties of the eye that the explanation of this phenomenon must be sought. Let us suppose that the eye to be examined looks at a luminous point A. The rays



projected into the eye from this point, being refracted, will be concentrated at the point A* of the retina, and reflected, in their turn, by this membrane, they will again issue from the organ. But since they must pass through the same media they had traversed on entering, they will undergo the same refraction, and be con-

sequently collected at the point A from which they first departed, in order to form there the retinal image.

From this it follows, that we could see the retina of an individual only when he looked attentively at our own eye, which, in that case, would be the luminous point. But it is clear, that the quantity of light which our own eye can project is too feeble to illuminate the bottom of the eye explored: and in attempting to look into the interior of that eye by the aid of ordinary day-light, we only intercept with our head the rays which should illuminate the cavity. Placed in this manner, in the shade, the pupil will naturally appear black.

After these considerations, it is easy to comprehend the object to be kept in view in using the ophthalmoscope. It is to throw into the eye, indirectly, a large quantity of light, in such a way that the observer, without intercepting the rays which enter, may receive directly into his own eye those reflected from the retina of the explored organ. With this view, any mirror may be employed, provided it be held obliquely with respect to the eye examined, and have a transparent spot, or a round hole, through which the observer may look.

Construction of the Instrument.—To Helmholtz, Professor of Physiology at Konigsberg, is due the invention of the first ophthalmoscope. The limits of my paper forbid a detailed description of his instrument. It is only necessary to add, that this ophthalmoscope, with all the modifications to which it has been subjected, has but very imperfectly fulfilled the desired end; since not only the illumination which it affords is insufficient, but its complexity also renders it extremely difficult of application. My present object is, simply to describe the instrument of Dr. Anagnostakis, of which an account will be found in his original and admirable monograph above cited. Combining, as it does, the entire advantages of all the complicated and expensive ophthalmoscopes heretofore invented with the utmost degree of simplicity, facility of application, and portability, it will, without doubt, wholly supersede them. It is now exclusively used in France and Belgium, and the results of its application have been so satisfactory as to leave little else to be desired.

The instrument is composed of a small, round, concave mirror, about two inches in diameter, with a focal distance of four and a-half inches; the silvered surface being accurately adapted to a plate of blackened copper. The centre of this little mirror is perforated by a small hole about three lines in diameter. Around the border of the glass and plate thus adapted is placed a brass rim, to which is attached a small handle of wood or ivory.

Method of Using the Ophthalmoscope.—The pupil of the eye to be examined, if not already very large and fixed, should be previously dilated with atropine or belladonna. A dark room is selected; on a level with the patient's eye a bright lamp is placed upon a table as near as possible to the patient, who is seated beside it. The observer is seated on a chair close in front of him, so that their eyes shall be upon the same level, and holds the mirror with the reflecting surface directed in such a manner that the rays reflected from the lamp are thrown upon the eye to be examined. Then, applying his eye against the central hole, the observer looks through it into the bottom of the eye thus illuminated; and, still holding the instrument constantly and steadily against his eye, he approaches towards or recedes from the patient till the reflection becomes small, brilliant, and oblong in form, and the tissues at the bottom of the globe are recognised. If it is desirable to magnify the image of these parts, or if, in consequence of the extreme presbyopia of the patient, the observer is obliged to place himself at a greater distance in order to see the deep tissues of the eye, a convex lens, of suitable magnifying power, must be used. Accordingly as the refracting power of the eye examined is greater or less, so must the eye of the surgeon be brought nearer to or removed further from the patient, in order to have a clear image. But to secure, at the same time, a sufficient illumination, the focus of conjunction of the mirror must fall upon the cornea of the eye observed. Since, however, this focus, in a concave mirror, varies with the distance of the light from the mirror, by altering that distance, the focus may be lengthened or shortened at pleasure. Thus the instrument may be adapted to all eyes indiscriminately, whether myopic or presbyopic.

These are the general rules to be observed, and a little practice will give the surgeon that facility in the use of the instrument necessary to accurate observation.

A few words with regard to the appearances presented by the healthy eye are necessary to enable the reader to appreciate the pathological changes which the ophthalmoscope reveals.

On first looking, as above directed, into an eye free from disease, the pupil is seen of a reddish colour, but none of the deeper structures are distinguished. As the parts at the bottom of the eye are necessarily seen through a refracting medium, which varies in degree in different individuals, it is evident that the surgeon must approach towards or recede from the eye inspected, till the proper focus is found; he will then see red vessels traversing the bottom of the organ. When these blood-vessels are distinctly recognised, it is certain that the proper focal distance is attained, and the retina is in view.

This point, once gained, should be steadily preserved, while the patient is directed to turn his eye-ball in different directions; thus all the parts of the retina are successively brought into the field of vision. When the globe is turned upwards and inwards, a spot, usually circular, is seen, much whiter than the surrounding parts, with a small umbilicated indentation in the centre, through which pass four blood-vessels (two directed upwards, and two downwards), which branch off to the different parts of the membrane. But very frequently these principal trunks divide before entering the globe, and then a larger number of vessels are seen coming in at the white spot formed by the entrance of the optic nerve. Except this patch, which is quite white, the rest of the retina presents a reddish colour, which becomes decided towards the periphery.

I must now speak of the pathological changes which have been already revealed by the aid of the ophthalmoscope. I shall adopt the natural order of noticing, first, the lesions that have their seat in the crystalline lens and vitreous humour; secondly, those confined to the essential organ of vision, the retina.

I imagine that no opacity of the lens, however slight, can escape detection by the use of this instrument. Certain it is that opacities which even materially interfered with vision, but which escaped the scrutiny of the most experienced eye, have been readily detected by a resort to the ophthalmoscope. Some very eminent oculists have reported cases of this kind. I have seen, at the clinic of M. Desmarres, beautiful opaque streaks in two lenses, which had eluded all other means of examination. When seen by the aid of this instrument, opacities of the crystalline appear as greyish-brown patches or streaks, of various sizes and shapes, upon a reddish back-ground, afforded by the illuminated retina. The importance, as to treatment and prognosis, of distinguishing the very commencement of cataract from impairment of vision produced by other more serious lesions of the vitreous humour or of the retina itself, is too obvious to need illustration. Who that has had even a limited experience in diseases of the eye, has not seen patients, with incipient cataract, tortured for months or years with leeches, cupping, blistering, and a thousand pernicious drugs, under the supposition of a commencing amaurosis? And who has not been consulted by patients sent to be operated upon for cataract, in whom the lens was perfectly transparent?

The most common alteration of the vitreous humour is the appearance in its substance of deep brown or greyish corpuscles, in greater or less abundance. These bodies are of very various sizes and shapes. Sometimes we see only two or three large irregular flakes, either floating separately in the liquid vitreous humour, or united to each other by a number of small filaments. At other times an immense number of smaller corpuscles, some angular, others in the form of shreds, and others, again, resembling pieces of membrane with ragged edges. When agitated by the sudden movements of the globe, these floating bodies are seen bounding and darting about in the interior of the eye, and gradually subsiding towards the inferior part when the organ is at rest.

Whether these substances are inflammatory exudations from the ciliary body, retina, or choroid, that have become detached; whether they are the remains of blood effused into the cavity of the eye; or, as is more probable, attributable sometimes to one and sometimes to the other of these sources, is not yet settled. Future observations can alone decide that question. Since, in all cases where these corpuscles exist, the vitreous humour is disorganized and liquid, I have thought that some of them might be opaque and thickened portions or shreds of hyaloid membrane.

Doubtless many cases of muscæ volitantes are produced by these little bodies floating before the retina. As, however, they are not unfrequently seen in patients who are not at all troubled with muscæ, and are absent in others where this phenomenon is complained of, they cannot be considered as its sole cause. If small, and occupying the anterior segment of the vitreous body, they may throw no shade upon the retina, and hence not interfere in the slightest degree with vision. But when of large size, and situated in the posterior segment, near the retina, they must necessarily derange the sight by mechanically intercepting the rays of light.—*Med. Times and Gazette, July* 1, 1854, *p.* 7.

101.—ON ULCERS OF THE CORNEA.

By George Critchett, Esq., Surgeon to the Royal London Ophthalmic Hospital.

In cases of acute and spreading ulcers, where penetration of the cornea is threatened, it becomes a matter of extreme importance to determine whether there is any means of arresting the progress of the ulceration. However judiciously the constitutional treatment may be conducted, there is every fear that penetration may occur before a favourable reaction has time to take place; it, therefore, is an anxious question, whether we can, by any local application, bring about an altered action in the part, and avert the impending danger. It is in such cases as these that I have found the nitrate of silver, in substance, of great value; it should be finely pointed, and carefully limited as much as possible to the ulcer itself; in order to effect this object, glycerine should be dropped into the eye previously, so as to protect the remainder of the surface from the action of the caustic. It seldom requires repeating more than once or twice, and it will frequently be found that the ulcer will begin to fill up and heal from that time. In other cases it must be admitted that the effect is less favourable—that it is productive of considerable pain, of increased inflammatory action, and of infiltration of matter between the corneal layers. This I have chiefly observed amongst the old and feeble; but the happy result that occurs in numerous cases, and the extreme urgency and threatening aspect of the symptoms, fully justify and even suggest the plan I am now advocating.

I have usually observed that these ulcers occupy a considerable area; and if we are unable to arrest their progress, and penetration occurs, the iris immediately falls forward, in contact with the opening, which gradually enlarging, allows of its protrusion, and thus "prolapsus iridis" occurs; the natural resisting power and elasticity of the globe is weakened, the prolapse increases, the anterior surface of the eye bulges forward, and staphyloma occurs. It is very desirable, if possible, to obviate this latter result; and here, again, it has been suggested to apply the nitrate of silver, in substance, to the protruded part. The objection to this plan is, that it often causes severe pain, and is by no means uniformly successful, and therefore I cannot recommend it. I much prefer either puncturing the prolapse with a needle, which often causes it to contract and shrivel away, and thus to close up the corneal opening; or in case that fail, and the protrusion still increases, to remove it entirely. Under the most favourable circumstances, when disease has produced such results, the integrity of the organ is seriously compromised; but it is quite possible that sufficient space may be preserved for the formation of an artificial pupil, and under any circumstances, it is most desirable to prevent the occurrence of a large staphylomatous protrusion.—*Lancet, July* 15, 1854, *p.* 23.

102.—ON VASCULAR OPACITY OF THE CORNEA.

By George Critchett, Esq.

[Independent of the immediate effects of catarrhal and purulent ophthalmia, there are remote changes which are exceedingly troublesome,—amongst these we may mention vascular opacity of the cornea. Mr. Critchett gives us some good remarks on the subject. He says:]

Some of the most marked and obstinate forms of this disease that have come under my care at the Ophthalmic Hospital, have occurred in men who have served

in the Indian army, where the original disease is probably due to atmospheric influence, and where we find that all diseases of mucous membranes are severe, intractable, and prone to lapse into this hypertrophied state, and so continue for an indefinite period. I have never observed this condition in infants, even after the most acute purulent ophthalmia. As may readily be imagined, the existence of even a thin vascular layer over the surface of the cornea very seriously interrupts vision, and the worst cases only allow perception of light, and a dim outline of objects, and it becomes an anxious question to what extent the brilliancy of the cornea can be restored by suitable treatment. In forming a judgment upon this point, what we have especially to notice is the direction of the vessels—whether they creep down from above only, or whether they encroach from all sides—as upon this circumstance depends the fact, as to whether the vascular opacity is entirely due to the granular condition, or to some inherent vice in the conjunctiva and cornea, and in the general state of system. If we can satisfy ourselves that the disease is purely local, and due to the mechanical change of the conjunctiva, we are justified in giving a favourable opinion as to the result. Our treatment must be exclusively directed to the removal of the granular condition. If we can succeed in this, the subsequent vascularity, which is a consequence of it, spontaneously subsides, and leaves a clear cornea. Various plans have been suggested for their removal. Thus, excision by means of curved scissors has been recommended. It has, however, this serious objection, that if it be only partially and superficially done, it has the effect of pruning, the granulations are reproduced, and usually to a greater extent, or, if they are deeply and thoroughly removed, they leave an irritating, irregular cicatrix, that keeps up the disease of the cornea, and may cause entropium, and is worse than the original condition, since it is irremediable. Frequent scarification has been suggested, and is now sometimes had recourse to. My own experience is unfavourable to it, and I have seen cases where it has been employed by other surgeons for many months without advantage, and it seems improbable that a slight and temporary unloading of the vessels, by small punctures or cuts, should remove, or even diminish, these growths; they irritate, but do not cure. The treatment that is most uniformly recommended, and that which I have found the most valuable, is the application of some powerful astringent or stimulant to the surface of the enlarged villi. This should be done by everting the upper lid, and applying the remedy freely over the surface. The principal stimuli for this purpose are the solid nitrate of silver, or strong solution; the sulphate of copper; the acetate of lead, alum, zinc, &c. In the selection of a stimulus, attention must be paid to the condition of the granulations, their size, amount of organization, nervous sensibility, and the time they have existed. In the more recent cases, where they are small, round, vascular, and sensitive, the stimulus must be mild, as, for instance, a solution of the nitrate of silver, or the sulphate of copper, about ten grains to the ounce of distilled water; in the large, pale, insensible forms, the solid nitrate of silver may be freely and frequently applied. As a general rule, and in an average case, my own experience leads me to prefer the sulphate of copper to any other application; it may be rubbed freely over the surface, and may be repeated either daily or every other day, according to the effect produced; it is far less painful and irritating than the nitrate of silver, and except in very indolent cases produces a sufficient effect. Recently, the acetate of lead, in powder, has been much recommended. I have tried it in some cases. It appears to incorporate itself with the tissue, and by degrees forms a sort of permanent enamel over the surface. I have not been able to trace any favourable effect in diminishing the villi, but I have no doubt it renders them less irritating to the corneal surface, and may be conveniently and advantageously employed when other means of acting upon them have been exhausted. I have no doubt the ointment recommended by Mr. Guthrie is very useful in these cases. Whatever caustic or astringent is selected, it is desirable to limit its effect as much as possible to the part to which it is applied, and to protect the rest of the conjunctival surface. This is more particularly necessary when the nitrate of silver is employed in substance. The best mode of effecting this object, is to put a little glycerine into the eye previous to applying the caustic, and also to keep the eyelid everted for some little time after the application. Although caustics and powerful astringents are the only reliable means in these cases, it is only when applied carefully to the part, and at stated intervals, that they are of service. Used in weak solutions, as drops or

lotions, I have often observed them to do harm by irritating the eye, without acting on the disease.

In undertaking a case of this kind, it is important that the patient should be made aware that, under the most favourable circumstances, and with the most judicious management, the process of treatment must be tedious, extending over many months, and requiring great perseverance in the use of remedies. If the disease be limited to the upper half or even two-thirds of the cornea, and if the vessels can be all traced from above, you may depend upon a cure, provided the plan I have recommended be judiciously carried out. The first result is the diminution of the villi, until by degrees the conjunctiva becomes nearly even and smooth.—*Lancet, June* 10, 1854, *p.* 611.

103.—*On Cold as an Anæsthetic in Operations on the Eye.* By GEORGE CRITCHETT, Esq.—[The following operation for cataract under the local influence of cold is interesting.]

Some pounded ice was put into a bladder, mixed with salt, and placed over the right eye, temple, cheek, and brow, and kept there for about twenty minutes. At the end of that time, all sensation being lost, I made a rapid section of the cornea, which was immediately followed by the cataract and some portion of vitreous humour. Some slight hemorrhage occurred, but slowly, and not to an extent beyond half an ounce. As sensation returned, our patient complained of extreme soreness and discomfort about the eye, and some of the old pains, taking the course of the fifth pair of nerves, came on. All this, however, speedily subsided, and we had the satisfaction of seeing him in a few days quite free from pain, the section of the cornea gradually approximating, and with every prospect of a speedy and complete recovery, without fear of relapse, now that the cause of all the suffering was removed.

It seems to me that the application of cold fulfilled, in this case, all the indications that were desired, and from the slight hemorrhage that occurred, and from my previous experience of somewhat similar cases, I am of opinion that if the operation had been performed without local anæsthesia, there would have been very severe pain at the time, extensive bleeding, consequent painful distension of the globe, and a tedious recovery.—*Lancet, Sept.* 23, 1854, *p.* 258.

104.—*Coniin as an Application in Scrofulous Intolerance of Light.* By PROFESSOR MAUTHNER.—The author employed with great effect the following preparation in the non-inflammatory blepharospasm of scrofulous children:—Coniin gr. ss., olei amygd. dulc. 3j. Mix, to form a thick fluid, with which the lids may be daily pencilled. In eight, or at most fourteen days, this troublesome condition ceases. Hard glandular swellings of the neck bear this remedy better than salves composed of iodine.=*Journ. f. Kinderkr.*, 1, 2, 1854.—*Medical Times and Gazette, Aug.* 12, 1854, *p.* 169.

105.—*Treatment of Tinea Tarsi.*—The treatment which is the favourite at the Moorfields Royal Ophthalmic Hospital in this troublesome disease consists in carefully taking away all scales and crusts, and then rubbing the edge of the lid with solid lunar caustic. The application must be repeated twice a-week, and patiently continued. It is absolutely necessary that all deposit be cleared away, so as to allow the remedy to come in direct contact with the cuticle, the opening of the hair follicles, orifices of the Meibomean glands, &c. If the treatment be steadily persevered with, the redness subsides, the thickening of the lid is removed, and the hairs previously destroyed grow again. Mr. Critchett is accustomed to relate a case in which a fair set of eyelashes were reproduced, after they had been absent for many years. The disease appears to be much more frequently secondary to measles than to any other affection, and its subjects are generally more or less cachectic. It often varies remarkably with the condition of the patient's health. Tonics are, therefore, indicated, together with a liberal diet; but it is well proved, that these, without

local treatment, are not competent to cure the disease. At the Hospital for Skin Diseases, in several cases recently, the hairs and their bulbs have been examined with the microscope. It does not appear that any cryptogamic sporules are ever present, the structure of the hair being healthy. In pulling out an eyelash, the hair-sheath is generally brought away too, and on its exterior are numerous pus and exudation-cells, showing that the inflammatory action has been external to the hair-sheath. In this respect, the hairs resemble those of sychosis and of impetigo occurring on hairy parts of the face. Mr. Startin's treatment consists in smearing the edge of the lid every night with a mild mercurial ointment (ammonio-chloride, 10 grains to the ounce), and administering an arsenical tonic in combination either with iodine or mercury.—*Med. Times and Gaz., Oct.* 28, 1854, *p.* 444.

MIDWIFERY,

AND THE DISEASES OF WOMEN.

106.—ON THE PELVIC ARTICULATIONS IN THE MECHANISM OF PARTURITION.

By Dr. J. M. Duncan, Edinburgh.

[It is known that in the guinea pig there takes place at the time of parturition a very considerable separation of the pubic bones. In the cow these changes differ in some important respects.]

They consist in an increased development of the large sacro-sciatic ligaments, which from being of moderate thickness, and in a state of tension in the non-pregnant cow, become much increased not only in thickness, but also in length, and are thus made slack and yielding. The tension of these ligaments tends to fix the sacrum and consolidate it with the ilia, and their relaxation leaves it freer to move. Further to facilitate this motion, the sacro-iliac joints, which in the non-pregnant cow are described by Mr. Barlow as secured by a material closely resembling intervertebral substance, now have the opposing bony surfaces smooth and lubricated, and the surrounding fibrous ligaments relaxed. By these changes the ilia become extensively movable upon the sacrum (or *vice versâ*), in an antero-posterior direction, the motions being analogous to those of flexion and extension in the limbs. The final result of these changes and motions is to enlarge the genital passages in this animal.

It has hitherto been customary to regard the articulations of the pelvis in man as virtually immovable, and to describe in the female at the time of parturition cases where motion evidently takes place as morbid in their character. But Mr. Zaglas has lately pointed out that in man there is distinct motion of the ossa innominata in an antero-posterior direction, or upon an imaginary line passing transversely through the second sacral vertebra from one side to the other. In other words, the sacrum may be described as having a nutatory motion upon this imaginary transverse axis, the promontory of the sacrum advancing downwards and forwards, while its apex moves in a contrary direction, and *vice versâ*. In the downward motion of the promontory, which in the non-pregnant is to the extent of about a line, the brim of the pelvis is diminished to the same extent in its conjugate diameter, while the corresponding upward motion of the apex of the bone to the extent of about two lines puts the sacro-sciatic ligaments on the stretch, and enlarges the dimensions of the outlet. By observations on the living and on the dead subject, Mr. Zaglas has shown that in the erect position the sacral promontory is not in the position of greatest projection into the brim of the pelvis, but the reverse, and consequently that the apex is in its forward position diminishing the outlet, and relaxing the sacro-sciatic ligaments. When the body is bent forward, on the other hand, the base of the sacrum is protruded into the brim, the apex is tilted upwards, the sacro-sciatic ligaments put on the stretch, and the outlet of the pelvis consequently enlarged. These movements take place ordinarily in both man and woman, but in her they are of greatest interest and importance in the function of parturition. Before entering on this part of the subject, I shall first point out some peculiarities in the pelvic articulations in woman, and describe the changes taking place in them in the end of pregnancy.

The three large pelvic articulations present the following important peculiarities in regard to their mode of union:—Each articulating surface of bone presents two distinct parts; the one, comparatively smooth, covered with cartilage and only partially united to its neighbour; the other, rough for the attachment of very strong and numerous bands of fibrous and fibro-cartilaginous tissue firmly uniting it to the corresponding surface of its neighbour. The former surfaces form the anterior and

inferior parts of the sacro-iliac joints, and in the skeleton are known as the auricular surfaces. In the symphysis pubis these surfaces form the superior and posterior parts of the joint. Interposed between the investing cartilages at these parts is a synovial bursa. It is on these surfaces that the articular motion is most free, the ulterior advantages of which, in the physiology of the erect position, &c., this is not the place to demonstrate.

Mr. Zaglas points out that on the os innominatum we may divide the entire articular surfaces into four parts. The two antero-inferior of these correspond with what is called the auricular surface, and are inclined to one another at an angle which looks outwards and forms a ridge inwards. The two posterior superior surfaces (which are separated by a large mass of fibrous ligamentous tissue from the corresponding surface of the sacrum) are inclined to one another at a similar angle; and there is accordingly a crooked ridge running between the four surfaces in a direction from the spine to the symphysis. Taking the four surfaces, however, in another relation, it may be seen that the two posterior superior surfaces are separated from the two anterior inferior by a groove (running nearly parallel with the axis of the sacrum), and are inclined to them at an angle which looks inwards. The consequence of this arrangement is, that while some motion is permitted, any tendency to displacement is entirely obviated, so long as the innominate bones are retained in a due degree of proximity by the pressure on the acetabula, and by their ligaments. But besides these just remarks on the general arrangement of the whole joint, it is important to notice another striking peculiarity always observed, and frequently in a very marked degree. This consists in the existence upon the posterior articular surface of the ilium of a bony prominence of irregular outlines, but frequently assuming the form of a solid projecting angle. This projection is found to correspond to a distinct cavity on the opposed surface of the sacrum, which is, in some of its functions, analogous to a cotyloid cavity. For, upon these parts (which are generally on a level with the upper part of the second bone of the sacrum) the motions of the ilia must take place; and whilst they will offer no absolute resistance to the motions of the ilia upon the sacrum analogous to flexion and extension in the limbs, they will, like the general arrangement of the entire articular surfaces of the articulation, prevent the slipping upwards or downwards of the one bone upon the other, motions which would necessarily prejudice the security of the erect position.

In the latter half of pregnancy the soft tissues contributing to form the pelvic joints are invariably, or almost invariably, found softened as if by serous infiltration; and the joints are consequently relaxed. All anatomists and obstetricians, who have paid attention to this subject, agree in this statement. The softening of these tissues is generally accompanied by their increase in thickness, a change which will in itself have, as a necessary consequence, the separation of the bones, and the enlargement of the pelvic circle. And I have no doubt that this favourable circumstance, together with others connected with the motions of the joints to be presently discussed, forms an important part of the explanation of some cases of delivery, by a simpler operative procedure than was predicted to be necessary. Indeed, the experiments of MM. Giraud et Adsiaux seemed to them to show that in contracted pelves this change in the joints takes place to a greater extent than in well-formed pelves. In some cases the thickening of the tissues goes on to quite an extraordinary extent. Boyer states that in one case he found the sacro-iliac joint separated to the extent of half an inch; Chaussier found the symphysis pubis separated still more in an easy labour, and Madame Boivin asserts that she sometimes found the pubic bones separated to the extent even of an inch. In some cases, as in those of Smellie, Diemerbroek, and Denman, the separation appears to have taken place chiefly during the course of a difficult labour.

But although there can be no doubt as to the thickening and softening of the tissues forming the pelvic joints, there is great difference as to their capability of motion. In this country, indeed, most authors seem to think that motion in these joints in pregnancy is always to be considered the result of a morbid process. This opinion is, without doubt, erroneous, although there are observed not unfrequently cases where the natural relaxation of these joints increases to such an extent, as to interfere with the function of progression.

The observations which I have already made upon the movements in the pelvic

joints in the non-pregnant, set aside at once all the arguments adduced to show that there is naturally no such mobility in pregnancy, and that when movements are observed in these joints, a morbid condition exists. Founding upon what has just been stated, as to the condition of the ligaments of the pelvic articulations in the latter part of pregnancy, we can, without difficulty, assert that at that time the pelvic bones enjoy freer and more extensive movements than at other times. In very numerous cases scattered through obstetric literature, where these joints have been examined after delivery, authors have described the mobility of these articulations, sometimes, indeed, as being to a very great extent. In addition, cases are not very unfrequent where these movements, either from their freedom and extent, or from their causing pain, attract the attention of the patient and physician. They are then sometimes easily perceived on making the proper examinations.

The movements which occur may be described as consisting in the elevation and depression of the symphysis pubis, the ilia moving upon the sacrum; or if the sacrum be regarded as the moving bone, it describes a rotatory motion upon an imaginary transverse line passing through the second bone. By the elevation of the symphysis pubis (or nodding forwards of the promontory), the angle of inclination of the pelvis is lessened, and the conjugate diameter of the brim of the pelvis is diminished to the extent of one or even two lines; the corresponding diameter also of the outlet is increased probably about twice as much. This different ratio of the effects of the motion upon the brim and outlet, results from the fact of the centre of motion being much nearer the promontory than the apex of the bone. The promontory, therefore, will describe an arc of a smaller circle than the apex.

That the alteration of the dimensions of the brim and outlet by these movements is not insignificant, but the reverse, is a proposition which every obstetrician will confirm. It only remains, then, to be observed how these alterations correspond with the phenomena of the progress of the child in parturition. Now it has been already stated, that in the erect position the brim of the pelvis is in its enlarged condition, the symphysis pubis being then depressed, while the outlet is correspondingly contracted. Now in the course of the first stage of labour, while the head is pressing into the brim, the human female is generally standing, sitting, or lying on her back, or in an easy position. But as soon as the head has descended into the pelvis and impinged upon the sensitive vagina, then forcing efforts accompany the pains. These forcing efforts consist, in great part, of powerful contractions of the anterior abdominal muscles, the effect of which, especially the action of the two recti muscles, will be to tilt up the symphysis pubis, thus throwing the promontory forwards, contracting the brim, and enlarging the outlet, and diminishing the angle of inclination of the pelvis. To all these changes the position usually assumed by the female in the second stage of labour will contribute. For it has already been stated, that the simple bending of the body forwards has for its effect the tilting upwards of the apex of the sacrum and enlarging of the outlet. And it is a curious fact, that a woman in her forcing pains, in the second stage, is found to draw up her legs and bend her body forwards, thus inducing changes in her pelvis, which facilitate the advance of the child in that stage.

The motions of the pelvic bones, which we have been detailing, agree exactly with those that take place in the cow in parturition. In that animal the first effect of each pain is to elevate the tail, and thus enlarge the outlet for the escape of the calf.

The mechanism we have just been describing in the human female is analogous to that which we have previously shown to occur in the pregnant and parturient cow. The changes which occur at the time of labour in the guinea pig find their analogues in the altered conditions of the symphysis pubis in the human female. But in her they are only to a small degree comparatively. It is important, however, to remember, that in this joint the thickening of the ligaments is generally more apparent than in the sacro-iliac joints. The distension of the pubic ligaments will be easier in the recumbent than in the erect attitude, which last implies strong compression of the tissues of the joint by the pubic bones. Moreover, the separation of the thighs, which is habitually practised at the latter part of a labour, will favour any possible slight separation of the pubic bones, especially if the internal femoral muscles are in a state of contraction while the thighs are apart. Indeed, the study of the whole subject illustrates beautifully how nature leads the human female in

the act of childbirth to assume positions and make exertions which are necessary for perfecting the mechanism of the process.

There is another source of information as to the state of the pelvic joints, especially the sacro-iliac, namely, the results of section of the symphysis pubis, an operation which appears to me to have been prematurely abandoned. In the performance of this operation upon the living female in order to aid in delivery, it has been found that the pubic bones can be separated to the extent of from one and a half to two or even three inches, without any damage to the sacro-iliac joint, or with the result of merely lacerating the capsular fibres of the anterior part of the articulation. In many of the cases the pubic bones, after the division of the symphysis, seemed to part from one another with resiliency, as if their union counteracted some force tending to separate them. This phenomenon admits of two explanations; either by attributing it to the weight of the limbs acting upon the acetabula, or by ascribing it to the elasticity of the great mass of elastic fibrous tissue in the posterior and upper parts of the sacro-iliac articulation, which is not resisted by the different mode of union in the anterior and lower part of the joint as already described. In the pelvis of the male and non-pregnant female, when this operation is performed after death, it is found that a separation of from one, in some cases, to even two inches, can be effected without injury. These facts illustrate the relaxation of the sacro-iliac joints at the end of pregnancy. They are, however, perhaps of more importance in regard to symphyseotomy, as showing the amount of separation that may be produced without injury. But the dread of injury is probably, in the main, a groundless fancy; for we know that to destroy the sacro-iliac joint, after section of the symphysis pubis, it is necessary to use great force, a circumstance which is, in itself, the best guardian of the safety of the joint.

The operation of symphyseotomy, as reintroduced to the profession in 1768 by MM. Sigault and Le Roy, is one which has, with justice, been condemned. But the jealousy of the Academy of Surgery, which discountenanced M. Sigault's operation at the first, led the members, after the subsidence of the excitement produced by its first and only occasional successes, to repeat their condemnations of it, and prevented it obtaining a fair consideration. It yet remains to be seen whether the operation, as more broadly proposed, long before Sigault, by Severin Pineau, may not be one which is destined to have a small place among the operations of practical midwifery, devoted to saving the life of the unborn child. In this country the operation received, after its proposal by Sigault and Le Roy, the high sanction of W. Hunter and Denman, so far as its own peculiarities were concerned. But they, at the same time, showed that it could be of very little, if any, service, in the cases for which it was proposed, namely, those of extreme pelvic distortion where Cæsarean section would otherwise be required. In this condition matters have been allowed to rest. British obstetric authors have loaded the operation itself with calumnies which are quite unfounded, and raised difficulties about it which are sufficient to deter a superficial inquirer from its consideration.

There is every reason to believe that the operation, in itself, is one of slight danger at the time, or even ulteriorly, if compared with the dreadful results of craniotomy and Cæsarean section. For the latter operation it can very seldom be a substitute. But it remains to be seen whether the former, namely craniotomy, may not in some cases be superseded by it. There is every reason to think that the operation would be much less dangerous to the mother than craniotomy, even with the allowance of great freedom in the selection of cases; and it would give a chance of saving the child, whose life is necessarily compromised by that proceeding. Moreover the operation might probably be simplified by adapting to it the subcutaneous method.

I conclude these remarks with the following quotation from the most esteemed author in British obstetrics, whose name and influence have contributed greatly to the neglect into which the operation has fallen:—

"It is proved," he says, "in the first place, that some enlargement of the capacity of the pelvis is actually obtained by dividing the symphysis of the ossa pubis.

"Secondly, that the evils which have followed this operation have been very much occasioned by its being performed unskilfully, or by injudicious endeavours to increase that enlargement of the capacity of the pelvis beyond the degree which naturally follows the division of the symphysis.

"Thirdly, that many women who have undergone this operation have recovered; though of those who recovered, many suffered very serious complaints for a long time, or for the remainder of their lives.

"Fourthly, that some children were born living when this operation was performed.

"We may, therefore, presume to say that if a case could be so precisely marked that there should only be a deficiency of just so much space as would be supplied by the simple division of the symphysis, the operation might in that particular case be considered.

"We may also say, that this operation is not so certainly fatal to those women on whom it may be performed as the Cæsarean operation; nor so certainly destructive of children as that of lessening the head.

"We may, then, be allowed to suppose a case, and such a one is more than possible, in which a person of very high rank, the life of whose child might be of the greatest public importance, could not be delivered without the destruction of the child, or her child be preserved but by the Cæsarean operation at the expense or great hazard of her life; and that she, through human frailty, might refuse to submit to the Cæsarean operation, yet the great interests and policy of the nation might forbid the destruction of the child. Of course both the mother and child would be inevitably lost. Should such a case occur, which, as said before, is more than possible, then the section of the symphysis of the ossa pubis might be proposed and performed, as it would in some measure meet both these interests; being less horrid to the woman than the Cæsarean operation, and, instead of adding to the danger, give some chance of preserving the life of the child."

This testimonial from the eminent and sagacious Dr. Denman is the more extraordinary, as he is an author who joins strongly in the cry against the operation, and expressly says, in regard to the above passage quoted from his own work on Midwifery, that he does not "mean to insinuate a wish or advance an argument in favour of this operation, in the cases for which it was originally proposed, or any other which can be imagined."

The last paragraph of the passage just quoted gives in few words a general notion of the cases to which this operation may yet be adapted. But it must be remembered that, in our day, a section of this class of cases has already been provided with a suitable treatment in the operation of premature labour; an operation, however, whose use is not inconsistent with the simultaneous use of symphyseotomy. —*Dublin Quarterly Journal of Med. Science, Aug.*, 1854, *p.* 60.

107.—ON THE INDUCTION OF PREMATURE LABOUR.

By Dr. John Rose Cormack, F.R.S.E.

The recent advocacy of certain new methods of bringing on labour suggests a few remarks upon this important subject.

Premature labour may be induced with more or less certainty by the following methods, when employed either singly or in suitable combinations with each other.

1. Puncturing the membranes.
2. Detaching the decidua from the internal surface of the cervix uteri.
3. Dilatation of the os and cervix by sponge tents.
4. The uterine douche.
5. Galvanism.
6. Administration by the mouth and anus of medicinal stimulants of the uterus.
7. Topical excitation of the mammæ.

The five first mentioned, and in particular the third and fourth proceedings, are those which are most to be relied on. The other methods enumerated may occasionally be taken advantage of as adjuvants.

1. *Puncturing the Membranes.*—This operation is often performed in the following manner. The patient stands when the sac is being tapped, as in this position the uterus comes down more within the reach of the practitioner. A piece of pointed whalebone, a catheter armed with a stilette, or some such instrument, is then taken in the right hand and pushed cautiously upwards, resting upon the fore-

finger of the left hand till it is made to pierce the membranes. From the opening being made in a position which is central and dependent, no obstacle exists to retard the flow of the liquor amnii, which consequently soon drains off, and the unprotected fœtus is pressed down upon the unprepared, *i. e.*, upon the undilated cervix and os. This energetic uterine action occasions great pain to the mother, while she also incurs the risk of sustaining injury from bruising and laceration of the parts concerned in parturition. The life of the fœtus is likewise endangered by the severe pressure to which it is thus suddenly and rudely subjected. It is better therefore to puncture the membranes high up; which can be done by a male catheter, from the extremity of which a stilette can be pushed out when it has been carried up to the proper place. The convex aspect of the instrument must be towards the sacrum; and the position of the instrument ought to be between the posterior wall of the uterus and the product of conception, so that when the stilette is pushed out of its sheath it cannot come in contact with the uterus, but with the membranes. When the membranes are punctured high up, according to this plan, the uterine parietes form a partial plug, and in place of a rapid and unceasing flow of the liquor we have it coming away gradually, and with intermissions. Natural labour is more closely imitated, for the membranes continue sufficiently distended to perform their office of a soft, insinuating, and powerful dilating wedge. From the moment of rupturing the membranes to the completion of labour, from thirty hours to some days may elapse. So long as progress is being made, it is wrong to hurry on the issue; but if, after the parts have become well dilated, the patient is impatient or exhausted, a turpentine enema or a dose of ergot may be administered with propriety. In certain very sluggish cases, with or without hemorrhage, it may even be necessary to supplement the operation of puncturing the membranes by using galvanism, or the douche. Such cases are not, however, of frequent occurrence.

These remarks are intended to apply to puncturing the membranes for the purpose of inducing labour. It need hardly be remarked, that there are circumstances in which the practitioner can both with facility and advantage expedite delivery by tearing the membranes with the nail of the fore-finger.

2. *Detaching the Decidua from the Internal Surface of the Cervix Uteri.*—Premature labour may be induced by detaching the membranes, by means of a catheter, or some instrument of a similar form. The downward pressure of the unruptured sac becomes a useful wedge in dilating the os. Some practitioners prefer this method of proceeding to puncturing the membranes. It may sometimes be advantageously resorted to, especially when combined with the judicious exhibition of ergot of rye. The use of sponge-tents and the uterine douche are, however, to be generally preferred to instrumental detachment of the decidua.

3. *Dilatation of the Os and Cervix by Sponge-Tents.*—This is an excellent method of inducing labour; as by preserving the integrity of the membranes during the process of dilatation, we get the aid of their beautifully designed wedge-power, just as it is exerted in spontaneous labour at the full time. As Dr. Simpson remarks, the dilatation of the os uteri is in a great degree advanced, before the labour itself actually begins. It is to Cluge that we are indebted for the introduction of this plan of inducing labour. On the continent it has likewise been advocated by Stultz, P. Dubois, Chailly-Honoré, and others; while in this country its general adoption has been much promoted by the teaching of Professor Simpson, who uses it, not only as a means of inducing premature labour, but likewise as a means of dilatation for the purposes of diagnosis and treatment, in some affections of the os and cervix.

Dilatation by the sponge-tent, or *l'éponge préparée* of French authors, is practised under several modifications.

The following description of the operation is translated from one of the latest and best French works on midwifery—the 'Traité Pratique de l'Art des Accouchements' (Edition 1853) of Dr. Chailly-Honoré of Paris.

"The patient is placed transversely at the edge of the bed, with her pelvis resting upon a hard pillow, and her limbs supported by assistants. When the bladder and rectum have been emptied, and the state of the pelvis has been re-examined—not with the view of ascertaining the presentation, which would be of no use, and is generally impossible—the operator introduces a tubular speculum, in such a way as to get the cervix uteri in the centre of the hollow of the instrument. He then, by

means of a plugging forceps (*pince à tamponnement*) insinuates a small cone of prepared sponge into the cervix. This cone ought to be about four *centimètres* (one inch and a half) in length, and one *centimètre* (two-fifths of an inch) at its base: it ought to be well lubricated, and to have attached to it a thread which can be distinguished (when hanging external to the passage) from the thread attached to the other sponge. This precaution enables the operator, when he proceeds to remove the sponges, to withdraw first that which has been last introduced. The second sponge ought to be sufficiently large to fill the vagina, and it ought like the other to have a thread attached to it by which it can be pulled. The second sponge is introduced above the thread of the first, and pushed up close to the os uteri, having been first soaked in tepid water, and then had the water squeezed out of it. The speculum is now gradually withdrawn, care being taken during the withdrawal to keep the sponges *in situ*, by pressing upon them with the point of the forceps. The operation having been thus completed, compresses and a T bandage are applied." (p. 256.)

The effects which follow these proceedings are thus described by the same author:—

"After some hours labour commences; the prepared sponge swells, in consequence of its imbibing the secretion of the cervix and the water retained in the second sponge. The expansion of the prepared sponge gradually dilates the cervix; and, by the irritation which it causes, it excites uterine contractions." (p. 256.)

Dr. Simpson recommends a pyramidal sponge-tent to be constructed with a perforation at its base, so that it may be temporarily fixed upon a curved wire or bougie whilst it is being introduced into the cervix; and he disputes the necessity of the second sponge. In July, 1844, addressing the Medico-Chirurgical Society of Edinburgh, he said that he had "found that the tent, when made and introduced in the mode already stated, required no vaginal plug, or other means to hold it *in situ*."

The time which elapses between the introduction of the conical sponge and the commencement of decided dilatation rarely exceeds twelve hours. When the subsequent progress is slow, successive doses of the ergot may be given with great advantage; or the membranes may be ruptured. Both of these measures are safer in the management of premature labour when the os and cervix have been previously dilated by the expanding sponge, than when they are used as initiatory measures.

From the time that the sponge-tent has been introduced the practitioner ought not to be beyond call; because labour may set in very quickly; or unsuspected placental presentation may suddenly demand his presence. The importance of this caution is illustrated by a statement recently made by Mr. May, at the Reading Pathological Society, and reported in the 'Association Journal' for 18th February, 1835 (p 145). He "mentioned the case of a lady whom he had attended in two labours, in which he had been obliged to perforate the head. During the third pregnancy, he advised the induction of premature labour. Without difficulty he introduced a sponge-tent into the os uteri. The next day he was summoned in great haste: there had been excessive hemorrhage, and the floor was deluged with blood. He proceeded to deliver; and then found a placental presentation. Without losing more blood, she almost instantly expired."

4. *The Uterine Douche.*—The uterine douche as a means of inducing premature labour seems to be differently understood and differently practised by those who have written in its favour. In fact, upon carefully examining what has been written on this subject, I find that there are three operations comprehended under the term *uterine douche*, which considerably differ from each other, both in theory and in practice.

First: Causing a stream of warm water to flow against the os uteri, at certain intervals of time.

Second: Causing alternate streams of warm and cold water to flow against the os uteri.

Third: Injecting water between the walls of the uterus and the membranes.

The first method belongs to Dr. Kiwisch, of Würzburg. His proceeding consists in allowing a stream of water at a temperature of 34° R., *i. e.*, 86° F., to flow against the os uteri, by means of a syphon. The following details are given in the 'N. Zeitschrift f. Geburtsk,' xxiv (1848), p. 153:—"He uses an apparatus consisting of a tin box of the capacity of ten cubic inches, which is fastened to the wall at an elevation

of about nine feet, and is connected with a tube which reaches to the ground. The lower part of the tube is elastic, and has at its middle a stop-cock. The elastic part ends in a mouth-piece, which has the form of an uterine tube (*eines mutterrohres*). The whole apparatus is intended to admit a continuous and powerful stream of water to the os uteri. The stream is allowed to flow for five minutes at intervals of six hours or longer until labour pains set in.

Dr. Shekleton, of Dublin, details a case ('Dub. Jour. of Med. Science,' *Retrospect* xxvii.), in which he employed a modification of Dr. Kiwisch's method, which he thus describes:—"The apparatus employed was the common elastic bottle-syringe, manufactured by Robertson, of Dublin, and made use of in the Rotunda Hospital for injecting the vagina, uterus, and rectum with cold water, in cases of post-partum hemorrhage. Accordingly, upon the evening of the 9th, the first douche was applied in the following manner. She was put upon her left side in the ordinary obstetric position; a large pan holding nearly two gallons was filled with tepid water, and placed upon a stool at a convenient distance from the bed: and another (a smaller one) was held so as to catch the water as it poured from the vagina, after having played against the os uteri. One person now introduced the bone-pipe at the end of the flexible tube, and with the index-finger guided the stream against the orifice of the uterus, which stream was forced onwards by another person, who held the bottle extremity of the apparatus in the large reservoir of water. The process was continued for about five minutes."

When the uterine douche is used after the method of Dr. Kiwisch, we presume the principal object is to cause the uterus to contract by exciting a reflex action, just as we accomplish the same object by means of galvanism. If the stream, however, be powerful, and if it be directed straight against the orifice, some of the water must undoubtedly enter the uterus; and in addition to the reflex action caused by the shock, we shall have the aid of mechanical distension of the os and cervix by a soft water-wedge, and also possibly a greater or less detachment of the membranes. The first method may therefore sometimes involve the third to a certain extent.

It is obvious that the various modifications of Dr. Kiwisch's apparatus, the vaginal douche of Dr. Simpson, or a common stomach pump, as suggested by Dr. Shekleton, may be employed according to the exigencies or inclination of the practitioner.

Dr. Kiwisch's uterine douche has been used pretty generally and very successfully for the last ten years in Germany, and it has more recently been employed in France, England, and other countries.

The second method belongs to Dr. Tyler Smith, of St. Mary's Hospital. It is a very rational modification of Dr. Kiwisch's douche. Dr. Smith remarks:—"Kiwisch recommends the use of warm water; but knowing from experience in cases of hemorrhage the increased efficacy of the alternation of hot and cold temperatures in causing uterine contraction, I determined to try the effect of alternating the hot and cold douche." The method of Dr. Smith seems to have answered very well in a case described by him in the 'Lancet' of 2nd October, 1852, and also in the hands of Mr. J. P. Lacy, who used it twice in the same individual. ('Lancet,' 4th December, 1852, and 22nd April, 1854.)

The third method, though it is obviously of older date, is generally connected with the name of Dr. Cohen, of Hamburgh. This physician published a paper in 1846, in the 'N. Zeitschrift f. Geburtskunde' (xxi. p. 117), on a new method of exciting premature labour. He says that for several years he had observed the effects of uterine injections in several experiments which he had instituted in the treatment of a lingering disease, and that he had in the course of these observations come to the conclusion that such injections must be very efficacious in inducing premature labour. In the same paper he relates a case in which he induced labour in six or seven hours. The patient recovered without any bad symptoms. He used a common syringe with a metal tube attached, and he injected the water between the anterior wall of the uterus and the membranes. He used tar-water, not because he thought it of greater efficacy than simple water, but because he had used it to correct excessive uterine secretion in the experiments referred to.

The exact method of proceeding recommended and practised by Dr. Cohen, is thus described by him, in the 'N. Zeitschrift f. Geburt." xxi. (1846).

"To a common metal syringe, which will hold about from two to two and a half ounces of fluid, I screwed a tube of eight or nine inches long, and with a diameter

of from one-sixth to one-eighth of an inch at the farther end, with the curve of a female catheter. I cause the patient to lie flat on her back, with her buttocks raised by supports placed under; I then introduce two fingers as far as the posterior lip of the os uteri, and make the tube glide over these into the os uteri between the anterior wall of the uterus and the fœtal membranes; I depress the handle of the syringe, making the tube glide upwards over the os pubis: but I do not begin to expel the fluid from the syringe, until the end of the tube has entered about two inches into the os uteri, for which purpose it is necessary to proceed gently and slowly; I now raise the hand somewhat upwards, so that the orifice of the tube may not be obstructed against the walls of the uterus. I now slowly discharge the contents of the syringe. If I feel any obstruction, the mouth of the syringe lies against some soft structure, and the direction must be somewhat changed, until I perceive that the syringe is easily emptied; I then press down the piston slowly. In ten minutes the patient can go about again. At the end of six hours, I repeat the injection, unless signs of approaching parturition have shown themselves."

Dr. Leopold Harting, in reviewing the various methods of inducing premature labour ('Monatsch. f. Geburts.' for 1853, vol. i.), though he does not mention Dr. Cohen's method of employing the uterine douche, seems to have repeatedly adopted it. He mentions seven cases in which he had used the uterine douche to induce labour. In the first four, he employed Dr. Kiwisch's method; in the last three, he inserted a tube within the os uteri for about an inch. In the first four cases, pains came on in from two and a half to fourteen days; and parturition occurred in from three to fifteen days. In the three cases in which the injection was thrown into the uterus, the first pain came on in from one and a half to three days, and the birth took place in from two to three days.

In the 'Deutsche Klinik' of the present year (January 14, &c.,) Dr. L. Steitz, of Hamburgh, refers to Dr. Harting's operation as "nothing but Cohen's plan, somewhat changed, but not improved." The same fact appears to have been pointed out by Professor Hohl, in No. 22 and by Dr. Sack, in No. 40 of the above periodical for the year 1853. Dr. Steitz relates ten cases in which he used Dr. Cohen's plan "with the best results." The cases occurred in the Lying-in Hospital at Hamburgh. He closely followed Dr. Cohen's instructions. The following is his account of the effects produced.

"At the end of one, at most of two, hours after the injection, the patient complained of a heavy pain in the hypogastrium, frequent desire to pass urine, and had a copious discharge of mucus *per vaginam*. The injection being repeated more or less frequently, there appeared relaxation, softening, and shortening of the vaginal portion of the uterus, with dilatation of the os uteri, and descent of the uterus and fœtus; the walls of the uterus became firmer and more contracted; distinct labour pains then always continued, until the os became fully dilated and the second stage of labour was entered on. In every case, the first injection produced the premonitory symptoms of labour. The operation did not give rise to any unpleasant symptoms in the course of the labour, nor was it followed by any considerable hemorrhage; but the end of the tube introduced was always covered with bloody mucus. The quantity injected on each occasion was two ounces. Tar water was generally used: simple warm water was employed once, with the same results."

Dr. Cohen, in the 'Deutsche Klinik' for February 11, 1854, says that eighteen cases of his operation have been recorded; viz. ten by Dr. Steitz; one by Pontonnier; six by Dr. Cohen; and one by Professor Kilian. The last case is recorded at p. 490.

Injection of the uterus as a means of inducing premature labour has, I think, been practised before Dr. Kiwisch or Dr. Cohen wrote: though I cannot at present give any evidence from my notes in support of this statement. I find, however, in the 'Edinburgh Monthly Journal' for October, 1844, the following anonymous article taken from the London Medical Gazette' of July 19th of the same year, upon "injection of the uterus, as a means of expediting and facilitating delivery,"

"A farmer in the neighbourhood of Edinburgh, son of a late eminent surgeon of that city, and well known to the editor of this journal ('Med. Gazette'), frequently had cows in great distress during their accouchement, and now and then perhaps, like other farmers, he lost a cow in the act of parturition. On one occasion when a poor animal of considerable value had been suffering for a very long time, and there was every prospect of an unfavourable issue to the affair, as it seemed inevitable

that the poor creature must die undelivered, the owner hurried into Edinburgh, distant about five miles, and took counsel with the eminent veterinary professor, Mr. Dick, as to the course to be pursued.

"At the suggestion of that gentleman, he, with all the expedition possible, threw into the uterus from six to eight quarts of tepid water, the animal's hind quarters being previously elevated by a bundle of straw, so as to aid in preventing the return of the water. The instrument employed in the injection was the flexible tube (in fact, that used for the stomach pump), attached to Read's patent syringe, which was easily introduced over the shoulder of the fœtus, the muzzle and fore-feet of which had been ascertained to be presenting at the commencement of the labour. The liquor amnii had completely escaped at a very early stage, and it was fully twenty-six hours afterwards before Professor Dick's excellent and ingenious advice was applied. But after the injection, the calf was felt floating freely in the cavity of the womb. The animal, however, was so totally exhausted, that there still seemed no hope of the calf being expelled by the natural efforts. Nevertheless, within five minutes after the injection of the water, a vigorous pain came on, and the patient was speedily and safely delivered of a live calf, and sustained no other ill consequences than a few days' weakness, the natural effect of her previous suffering.

"Our friend mentioned this very ingenious plan to a neighbouring surgeon-accoucheur in large practice, who was so much struck with its simplicity and apparent safety, that he resolved to adopt it on the first favourable opportunity. An occasion soon presented itself, in a case where nothing but the long forceps could have effected the delivery of the sufferer; and shortly afterwards in a second instance, where turning and delivery by the feet would have been indispensable to save the patient's life.

"In both cases the injection of about a quart of tepid water was attended with complete success; the patients were both delivered of living children by the natural efforts, without any unfavourable symptoms ensuing.

"We have gathered these particulars from the owner of the cow, the gentleman who, with his own hands, made practical application of Professor Dick's valuable suggestion, believing that the procedure indicated is new, free from danger in itself, and likely in many cases to obviate the necessity for more formidable and painful operations."

5. *Galvanism.*—In 1844, Dr. Radford of Manchester directed the attention of the profession to galvanism as an obstetric agent. He pointed out its advantages in uterine hemorrhage. Dr. Simpson of Edinburgh, about the same time, wrote in favour of this method of treatment: and it has since been adopted by many other practitioners. In a letter which I received from Dr. Radford, dated 10th May, 1854, he says, "I have four times successfully employed galvanism to induce premature labour, in cases in which the pelvis was contracted. In a fifth case, it only induced a temporary and partial influence. The uterus hardened under the application; but this effect was transient. It was afterwards necessary to use the sponge-tent."

Dr. Barnes and others have successfully employed galvanism in the artificial induction of labour. It appears, however, that it is rather in formidable cases of uterine hemorrhage that galvanism is most appropriately employed to excite uterine contraction; and for this reason, further details are postponed until that subject comes under consideration.

6. *Administration by the Mouth and Anus of Medicinal Stimulants of the Uterus.*—There can be no doubt that premature labour may in certain cases be induced by the administration of ergot of rye, Indian hemp, and other drugs which act on the uterus, in virtue of their peculiar powers. Purgatives which irritate the rectum, and medicines such as cantharides, which excite the bladder, also have a tendency to induce premature labour, by causing congestion and irritation of the uterus. Medicinal stimulants of the uterus cannot, however, be relied on exclusively when we wish artificially to induce labour; and, indeed, except when there is a predisposition to miscarry, they seem to have little power in determining parturition, even though administered in violent and dangerous doses. As adjuvants in lingering cases, when the labour has actually been instituted by artificial means, the ergot of rye in repeated doses, and the employment of turpentine enemata, may be advantageously resorted to, just as we use them in slow natural labours, when there is no contra-indicating

cause. An enema, consisting of about six or eight drachms of turpentine, mixed with twice or three times as much castor-oil, olive oil, or other bland fluid, has often an admirable effect in expediting labour. It cannot injure the fœtus, as the ergot often does; and it can be employed when the ergot has been or is likely to be rejected by the stomach.

7. *Topical Excitation of the Mammæ.*—Dr. Scanzoni has advocated the irritation of the mammæ by successive applications of the breast-pump, as a new and efficient means of inducing labour. From some experience of the galactagogue and emmenagogue effects of mammary irritation, I am inclined to believe in the frequent success which may attend Dr. Scanzoni's plan: but its superiority over other means, or the propriety of relying upon it as an independent proceeding, is at least problematical. According to present evidence, it is neither a certain, nor is it always an innocuous proceeding.

In a paper which I published in the 'Association Medical Journal' (*Retrospect*, Vol. xxviii.), entitled "Galactagogue and Emmenagogue Effects of Warm and Stimulating Applications to the Mammæ," a caution was given as to the bad consequences which were apt to result from pushing mammary irritation too far, which may here be quoted as a testimony in favour of the reality of the power over the uterus possessed by the irritating action of the breast-pump of Scanzoni. I remarked:—"As regards the ovaries and uterus, the effects produced upon these organs must be attentively watched; for the very greatness of the power which irritation of the mammæ exerts over these organs, may constitute an element of danger. In endeavouring to excite menstruation, we must take care not to excite inflammation of the ovaries, or of the womb." When these sentences were written, I had not become acquainted with the proposal of Scanzoni.

One great objection to Scanzoni's proposal is, that the irritation excited in the breasts may remain after labour, and give rise to a serious complication of the puerperal state.—*Association Med. Journal, June* 2, 1854, *p.* 487.

108.—ON PUERPERAL CONVULSIONS.

By A. B. Steele, Esq., Liverpool.

In the treatment of puerperal convulsions, the highest authorities recommend copious depletion, almost without reservation. Dr. F. Churchill says:—"The first thing to be done is to take away blood from the arm or temporal artery *largely*; if the paroxysms continue, this may be repeated." He insists on no condition as to the state of the patient, or as to the probable exciting cause of the fits, apparently considering that the existence *per se* of convulsions is a sufficient indication.

The experience of the late Mr. Crosse, of Norwich, was in favour of bloodletting. In his work on Midwifery, he gives the history of twelve cases; in eleven of which free depletion was practised and followed by recovery. In the only fatal case, he says:—"In consultation bleeding was not thought proper, and counter-irritation, &c., were the only means employed."

Dr. Copland, in his 'Dictionary of Medicine' in the article Puerperal Convulsions, observes:—"Depletion may be carried further in those states of the disease which assume the character of eclampsia, or which are attended by great fulness about the head, or stertorous breathing, than in almost any other malady."

Dr. Rigby advises free depletion; and all the writers, with one exception (Tyler Smith), whose works I have consulted, recommend copious bloodletting as the first and great remedy; the extent to which it is to be carried, being regulated mainly by the violence and frequency of the fits.

The history of the patient, her previous habits, as well as the peculiarities in each case in reference to the predisposing and exciting causes, are ignored altogether as indications for or against the use of the lancet; so that the practitioner is left without any guiding principles upon which to regulate his practice in this particular, and he uses this powerful remedy almost entirely empirically. It is, nevertheless, most important that some sound data should be given to regulate this very important and powerful remedy, for it is as injurious in unsuitable as it is indispensable in suitable cases.

There is a somewhat similar difficulty with regard to what is perhaps the second matter in importance in the treatment of puerperal convulsions—namely, the question of interference in the progress of labour by artificial delivery; but here authorities are not all agreed as to the main question; and few, if any, have pointed out any very satisfactory principles to guide us in deciding when it is necessary or proper to interfere, and when we ought to trust entirely to nature. Dr. Ramsbotham is in favour of turning; Dr. Collins is strongly opposed to it; and Dr. Churchill considers it a most hazardous measure. Dr. R. Lee deems it an essential part of the treatment to empty the uterus of its contents as speedily as possible. Dr. Rigby says there is little chance of the convulsions ceasing until delivery is completed. A later writer, however, has thrown much light upon this subject, and his observations appear to me highly valuable, and of the last importance in removing the difficulties above mentioned; I shall therefore quote his views, as stated in his recent work. The author I allude to is Dr. Tyler Smith, whose views are founded upon Dr. Marshall Hall's researches into the physiology of the nervous system; and his practical application of that physiologist's discoveries to the pathology and treatment of puerperal convulsions is interesting and useful.

Dr. Tyler Smith considers that puerperal convulsions depend upon one or two causes—viz. irritation of the spinal marrow, or upon some irritation of excitor-spinal nerves; and then he goes on to say:—"The action of bloodletting on the spinal marrow is greatly modified by the condition of the circulation; in fulness of the vascular system, it is the most powerful sedative of spinal action we possess; hence venesection is the grand remedy in the simple form of puerperal convulsions, where the disease chiefly depends upon stimulation of the spinal marrow by excess of blood, or on the mechanical pressure exerted by the blood on that organ, together with the counter-pressure of the distended brain on the medulla oblongata. But another most important intention of bloodletting is that of preserving the brain from injury during the convulsion. Besides the primary cerebral congestion, which may have been the cause of the attack by its counter-pressure on the medulla oblongata, the convulsive action itself with the glottis closed, the various sphincteric actions in operation which constitute what is called by Dr. M. Hall sphagiasmus laryngismus, cause the greatest turgidity of the vessels of the head, and are dangerous sources of fatal cerebral congestion, or of serous or sanguineous effusion; thus, in plethoric states of the circulation, bloodletting is curative in its action on the spinal marrow, preventive in its action on the brain.

"In the absence of definite ideas regarding the effect of bloodletting in this malady, it has often been pushed to excess, or practised where it should have been altogether avoided. In the numerous cases where, besides vascular excitement of the spinal marrow, some irritation of the spinal excitor nerves exists as a conjoined cause of convulsions, repeated bleedings will often fail to subdue the disease, unless the eccentric irritation be at the same time removed; when irritation of the uterus, rectum, or the stomach, is in part excitor of the convulsion, bleeding *alone* cannot be relied on. It may at first diminish the impressibility of the central organ, rendering it less sensible of the incident irritation; but if persisted in to a large extent without the removal of the eccentric irritation, it becomes in the end positively injurious, by increasing instead of diminishing the excitability of the spinal marrow."

He then furnishes the rule by which we are to be guided:—

"The propriety and the extent of bloodletting must be estimated not by the violence of the disease, but by the state of the circulation in the interval of the fits, and with especial reference to the different effects of vascular plethora and vacuity upon the spinal centre."

These observations of Dr. Tyler Smith will, I think, render the practitioner valuable aid in coming to a decision upon the question of depletion in the treatment of this disease, and especially in reference to the repetition of bloodletting when the first quantity taken, although large, has failed to relieve the fits. I apprehend that, in most cases of puerperal convulsions of a severe character, one full bleeding would be justifiable and proper, on the principles laid down by Dr. T. Smith; and when this has been done, and the fits still continue unabated, before pushing depletion further, bearing in mind the principles he has enunciated, we are led to investigate the various possible sources of irritation to incident excitor nerves, such as distension of the uterus or of the bladder, or the presence of irritating matters in the stomach

or intestines, and the propriety of the administration of opiates, especially by the rectum, would demand a full consideration.

In considering the propriety of interference in the progress of labour—a question upon which, as we have before noticed, the authorities are not at all agreed—we derive equal assistance from the views of Dr. Tyler Smith. After observing that the question of interference is one for which no arbitrary rule can be laid down, but which must be decided in each individual case by the particular circumstances, due regard being paid to the excitability of the uterus under the stimulus of the fœtus, and under the artificial interference, he advises the evacuation of the liquor amnii, which, by relieving the distension, diminishes the size of the organ and the quantity of blood circulating through it; and hence, although it renders the uterus more active by bringing its parietes into contact with the fœtus, it renders the organ itself less irritating to the general spinal system. The evacuation of the liquor amnii is to the uterus what the partial action of an emetic or an enema is to the stomach and intestines. With regard to the general principle which should guide us in deciding upon artificial delivery, it is to be observed that, whenever it can be effected with less irritation than would be produced by the continuance of the child in the parturient canal, and its expulsion by the natural process, it is advisable that it should be performed.

These general principles appear to be applicable to all ordinary cases of puerperal convulsions, and will be found of material assistance in enabling us to treat that formidable disease, if not with invariable success, at least upon rational and scientific principles; and they will also serve in some measure to reconcile the apparently contradictory and diversified statements and theories of different and differing authors.

It does not fall within the object and limits of this paper to speak of the numerous other special remedies which have been recommended in the treatment of puerperal convulsions, my intention being to draw attention to the two main questions of bloodletting and artificial delivery; nevertheless, I do not wish to leave the subject without some allusion to the use of chloroform. I have no experience of its effects in this disease in my own practice, but have had considerable opportunities of noticing the phenomena attending its administration in surgical cases; and from observation and reasoning, I should not be led to expect much benefit from it, beyond the collateral advantage of allaying nervous excitement in some exceptional cases. I say exceptional cases, because I believe that, in the most common form of this disease, in which there is more or less congestion of the brain, and where the fits are produced by other causes than those which depend upon excitement from without, or upon any impression made upon the sentient faculties, such as fright, excessive pain or irritation from prolonged suffering, or the like, chloroform could not be expected to exert any influence over the origin of the disease. It might, on the contrary, act as a predisposing cause, and render the paroxysms more frequent and violent; for it has been noticed by Dr. Marshall Hall, that, when the functions of the true cerebral or sentient portion of the nervous system are diminished or abolished, as is the case in anæsthesia, the irritability of the excitomotory or true spinal system of nerves is increased. Hence it follows, that convulsions being a disease of the true spinal system, they would be the more readily produced, the spinal nerves being more susceptible of any morbid impression conveyed to them: so that the only probable advantage to be derived from the use of chloroform is to shut out any exciting causes of convulsions which act through the medium of the functions of sensation; and this, be it remembered, is at the risk of rendering the system more susceptible of those much more important exciting causes which are probably, in almost every case, the real origin of the disease. That chloroform does not directly control the functions of the excito-motory system, is manifest enough from its ordinary effect upon the progress of natural labour; for there, although sensation is more or less diminished or annihilated, yet the contractions of the uterus, an excito-motory act, still continue.

It is further worthy of remark that chloroform frequently produces a state of system nearly approaching, if not actually reaching that of convulsion; and I have frequently observed that it produced a considerable amount of congestion of the vessels of the head and neck—in fact a slight degree of aphagiasmus—a condition which cannot be looked upon as favourable to the patient, already nearly asphyxiated by the paroxysms.

Another objection might be urged against the use of chloroform in many cases of puerperal convulsions; namely, the peculiar condition of the circulation which, as noticed by myself and by other observers, frequently obtains in this disease. The heart's action fluctuates in a most remarkable manner, the pulse sometimes being full and indicating a fair amount of vascular force; at others, so small and feeble as scarcely to be felt. Now, if chloroform should be given to a patient in the state indicated by the latter condition of the pulse, it might be regarded as a most fortunate escape if the heart's action were not altogether abolished, as we have seen in some unfortunate cases which have occurred in surgical practice, where a very small quantity of the anæsthetic agent has destroyed life by paralysing the action of the heart, already enfeebled by the supervention of depressing causes, such as fear or the like.

In cases where there exists, as a prevailing peculiarity, a great amount of nervous excitability of the system accompanied by restlessness, chloroform may be useful by producing sleep; or, in cases where pain seems to be the exciting cause, anæsthesia would no doubt be a useful means of relieving the patient; and to these and to analogous forms of the disease I believe its use should be limited.

In the sthenic forms of convulsions, accompanied by congestion and fulness of the vascular system, it can, I think, do little good, and might do much mischief.—*Association Medical Journal, July* 28, 1854, *p.* 669.

109.—ON SYPHILIS AS A CAUSE OF ABORTION AND PREMATURE LABOUR.

By Dr. Robert Johns, Dublin.

[Dr. Johns commences an interesting paper with the following case:]

Mrs. H., aged 30, married five years, consulted me on the 17th of February, 1852, under the following circumstances:—She stated that she had brought forth four dead and putrid children: the first a hydrocephalic male, after a natural labour, at the full time; the second a female, prematurely, at the eighth month, the breech presenting; the third, a hydrocephalic male, prematurely at the seventh month, the breech presenting; the fourth, a hydrocephalic male, at the sixth month, the breech presenting. She ceased to feel the movements of each of her children from ten days to a fortnight previously to the advent of labour.

The catamenia first appeared when she had attained her eighteenth year, returning regularly and naturally every month, and continued to do so until her marriage, from which time, for three months, they were very irregular and painful. In consequence of her husband's avocations she was separated from him for eight or nine months, during which period the menstrual secretion was perfectly regular and natural. Immediately on returning to her husband she proved pregnant, and fell into bad health, in which state she continued for the first three months of gestation, but from thence until the birth of her child she enjoyed good health, with the exception of irritation of the bladder causing a constant desire for micturition; and such was the condition of all her subsequent pregnancies. Very early in her first pregnancy she had some white blisters on the labia pudendi for a day or so, but which disappeared without treatment.

This lady had consulted several other medical men, all of whom, considering the unfortunate issue of her labours to have been caused by debility, treated her by tonics, but without success.

At the time of Mrs. H.'s visit to me the following were her symptoms. The menstrual secretion had ceased for three months; she considered herself pregnant; when not so her periods were irregular and painful. There was not any pallor of countenance, but the face was greasy, and covered with spots not to be washed off. She suffered much from giddiness, as if about to fall, accompanied by great fear, but had not any cephalalgia; a sinking feel in the stomach, with much debility and languor; palpitations and other hysterical symptoms; constipated bowels, with flatulence, loss of flesh, appetite, strength, and sleep; darting pains in the breasts under the nipples; dragging diagonal pains from the loins to the pubes; pain in the region of both ovaries; pain and weakness over the sacrum; weight and bearing

down in the vagina and in the rectum; enlarged inguinal glands; a greenish, watery, and sometimes a whitish vaginal discharge, very like to the unboiled white of an egg; pain in congress, and uterine inertia. On adopting the toucher the os uteri was found patulous, the cervix soft, elongated, otherwise enlarged and insensible to pressure, but not conveying to the finger the idea of the presence of ulceration. The speculum revealed extreme vascularity and inflammation of the os and cervix; an ulcer of the cock's-comb variety covering the entire of the anterior and the greater part of the posterior lip, and extending for some distance into the canal of the cervix, from which cavity issued a tenacious and purulent discharge, slightly tinged with blood; a purulent discharge also was seen escaping from the urethra, which was very much inflamed. The fœtal heart was not audible at this visit, but it was heard occasionally until about a fortnight before labour set in.

She was ordered a tonic mixture with iodide of potassium. The acid nitrate of mercury was applied to the canal of the cervix, and to the ulceration on the labia uteri, also a solution of nitrate of silver to the urethra.

22nd February. To have half a grain of the oxymuriate of mercury in decoction of sarsaparilla three times a day; the cervical canal was again touched with the acid nitrate of mercury, and a solution of nitrate of silver was freely applied to the ulcerated surface of the cervix.

25th March. Up to this date Mrs. H. has been taking the mixture just mentioned, which being found to disagree was discontinued this day, the following being substituted for it: one grain and a half of Plummer's pill, and two grains of extract of henbane, every night; and a mixture containing extract of dulcamara and iodide of potassium twice a day. This treatment, together with the occasional use of the before-named caustics to the ulcers was continued up to their cure (the fifth of June), the mouth having been sore from the 10th of March till that date.

11th June. She was delivered this day of a putrid hydrocephalic female infant, presenting with the breech.

This lady recovered well and quickly of this confinement, and went to the country to recruit her strength, where she remained until the 12th August, 1852, when I was again called to visit her. On that occasion the uterus was again examined, when a great amount of inflammatory engorgement of the os and cervix, together with superficial ulceration, was found to exist.

Blood was taken by the lancet from the os and cervix, and potassa fusa was freely applied to these parts. At the same time she was ordered to rub in one drachm of mercurial ointment twice a day, which treatment (her mouth becoming sore in two days) was continued until the 29th of September, when, as she was too sore to persist with frictions, the following pills were substituted for them: blue pill, a scruple; calomel, five grains; opium, three grains; in six pills, one every night. These kept up the ptyalism until about the beginning of December, when she was pronounced cured. From time to time the potassa fusa and nitrate of silver were used with the diseased womb.

She proved pregnant on the 23rd of December, 1852, enjoyed excellent health during the whole period of gestation, and was safely delivered of a fine, full-grown, healthy, male child, on the 29th of September, 1853.

Mr. H. (this lady's husband) was also submitted to treatment on the 10th of July, 1852. He commenced taking the following pills, it not being convenient to employ frictions, which I very much prefer; blue pill, a scruple; calomel, twelve grains; opium, three grains; into twelve pills, one twice a day. These made his mouth sore, in which state it was kept until the 10th of October, 1852, when an eruption of the nature of acne rosacea came out on his face and neck, but which was removed by the following mixture: extract of dulcamara, iodide of potassium, of each one drachm; lime water, twelve ounces, mix; two tablespoonfuls to be taken three times a day. Of course this couple lived apart whilst under treatment.

This I regard as a very interesting and important case, containing many features of practical utility, whether regarded in a curative or medico-legal point of view, and one requiring a very accurate diagnosis, the accomplishing of which was not without some difficulty arrived at, for the following reasons:—

1. The husband of the lady denied ever having had syphilis in any form, primary or secondary, and stated himself to be in perfect health; and though he was very accurately and minutely examined, not the slightest trace of lues was discoverable.

2. Mrs. H. never had any symptom whatever of the disease, locally or constitutionally.

3. All her symptoms could have been produced by disease of the womb of a non-specific nature. I may here be permitted to state, that I do not recognise any ulcer of the womb as necessarily syphilitic, save the Hunterian chancre, which is very rarely met, indeed I never saw it but twice; all the other affections of the os and cervix which have been laid down as syphilitic have come under my observation in females in whom it was most improbable, if not impossible, that such a taint could ever have existed, and have given way to treatment of a non-specific nature.

The plan of treatment which terminated so successfully here was employed on the following grounds:—

1. Because other means had failed.

2. On account of the order of the premature labours which I believe to be diagnostic of this disease, as from my experience I conclude that, in the generality of such cases, there is a regularity in their occurrence, either in a retrograde direction, as in the present instance, or in a progressive one, as in most others. For example, a female miscarries at three months, then at four months, and so on up to seven or eight months, when there will be a putrid child, with the skin peeling off, and other appearances so well known as a "pocky" child; then perhaps she may carry the child to the full time, when it will be born either with the disease, when "he who runs may read," or apparently healthy, in which the disease shall appear in some few days, or even in months after.

3. Because, had these arrests of gestation depended solely on the disease of the womb, she should have gone to the full time, as the ulceration was cured before the birth of her last child.

[Dr. Johns maintains that there is but one method of preventing abortions and premature labours in females tainted with syphilis, and that is the use of mercury for both parents—to the female when not pregnant—the patients being kept under its influence for six weeks or two months. After referring to several cases, Dr. Johns draws the following deductions:]

1. That secondary syphilis is not curable in the pregnant female.

2. That mercury is the only certain means of preventing the abortions and premature labours depending on syphilis.

3. That both parents must be submitted to this treatment.

4. That as the disease in such cases exists in the secondary form, the system must be kept under the influence of the metal for at least six weeks, and in some cases even longer.

5. That syphilis is communicable to the female through the semen of the male, without the presence of any ulcer or purulent discharge.

6. That secondary ulcers on the female genitals do not infect the male by whom she was contaminated, so long as he is poisoned by the infection which gave it to her.

7. That as syphilis is communicable from a child to its nurse, and *vice versâ*, great care ought to be taken that an infected child be not given to a sound nurse, nor a pocky nurse be hired for a sound child.

8. That syphilis in infants is only curable by mercury, which is best given directly to the child, as also to its nurse.

9. That there is no disease of the uterus, save the Hunterian chancre, which is pathognomonic of syphilis.

10. That the order of the abortions and premature labours is a very good test of their cause.

11. That ulceration may exist to a great amount on the os and cervix uteri, and not be discoverable by the toucher.

12. That the patulous state of the os uteri, induced by inflammation, may be present during pregnancy.

13. That children dying in the uterus from syphilis, like those dying from other causes, are thrown off within a fortnight or so after the cessation of vitality.

14. That there is a class of abortions and premature confinements preventible by iodide of potassium and similar medicines.—*Dublin Quarterly Journal of Medical Science, Nov.* 1854, *p.* 307.

110.—ON THE TREATMENT OF DESCENT OF THE WOMB.

By Dr. A. Desgranges, Surgeon in Chief to the Hotel Dieu, Lyons.

[Various attempts have been made to contract the vaginal canal in order to enable it to support the prolapsed womb without artificial means. The following methods seem new, but we doubt whether they will have any advantage over other means of accomplishing this object.]

Dr. Desgranges proposes to diminish the calibre of the vagina, and to render the uterus stationary by two entirely new methods. The first consists of a number of small forceps applied to the upper portion of the vagina, and allowed to remain until they come away themselves. The instruments he employs consist, first, of a number of small forceps with the blades crossed, like the tenaculum forceps of Sir P. Crampton. The points (A), which are sharp, are prominent, and cross each other, and the edges are rounded off, but the free extremity is armed with a prominent edge. The spring is sufficiently strong to make adequate pressure, without the instrument being too cumbrous.

In order to apply the vaginal "pinces," we have next a pair of forceps with a groove at their extremity, in which the outer end of the "pince" is to be placed up to the point where the blades bend in order to cross each other. The firm closure of the blades of this forceps presses open the points of the "pinces vaginales," and so enables us to apply them in situ without difficulty.

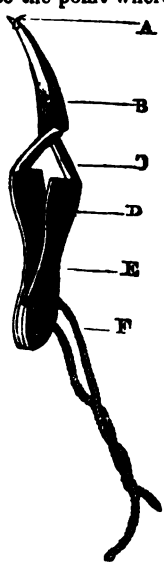


Fig. 1.

In applying these little instruments Dr. Desgranges makes use of the three-bladed speculum or of the gorget as a guide.

They should not merely seize the parietes by their points, but also by the curved portions of the blade, the convexity being towards the vagina, which it will hold firmly by the overlapping edge. The number of "pinces vaginales" to be applied will vary from six to nine, according to the state of the vagina, and it is better to commence with the posterior wall and the lower extremity of the vagina. When all are applied, the double T bandage is to be fixed, and the patient confined strictly to bed until the instruments come away, which is generally from the sixth to the tenth day.

When the parts are healed, the operation is to be repeated, the instruments being applied to a different portion of the vaginal walls; and when the canal is reduced in size, the gorget or the fingers only should be used as a guide in applying the "pinces vaginales," as the distension by the speculum might be injurious. In this way, by the successive application of these instruments, and, as Dr. Desgranges says, "mieux vaut trop que pas assez," the calibre and length of the vagina are so much reduced in diameter that it is quite capable of supporting the uterus and preventing prolapse. There is very little irritation occasioned by the application, and the number of small cicatrices which are perceptible at first, diminish by degrees, and finally disappear; so that, as Dr. Desgranges states, the operation does not interfere with any of the functions of this part. So much for the mode of operating. We have next a minute account of eight cases, of which six appear to have been completely cured by this method, and in two the relief was incomplete. If further experiments furnish similar results, which we think very probable, we shall have to thank Dr. Desgranges for an additional means of relieving a most unpleasant disease.

The second plan of Dr. Desgranges is a combination of mechanical pressure, and the application of caustic. The instrument consists of a pair of forceps armed with sharp points, below which is a receptacle for caustic; this is called the "pince elytrocaustique," and its application is aided by a vulsellum with one point only in each branch. The caustic used by Dr. Desgranges is the chloride of zinc, for the action of which he prepared by a previous application of another kind of caustic (caustique Filhos).

The operation is as follows:—

A fold of the mucous membrane of the "cul de sac" of the vagina is to be seized with the vulsellum, and drawn downwards, whilst the cervix uteri is to be pushed to the opposite side of the pelvic cavity; then the "elytro-caustique," guided by the finger, is to be applied as largely as possible to the fold of mucous membrane, and maintained so by the spring at its lower end. In about forty-eight hours the vitality of the portion thus included is destroyed, and a similar operation is to be performed on the other side of the cervix. After each operation a double T bandage is to be applied, and the patient confined to bed. Dr. Desgranges thinks that five or six operations may be necessary, but as no cases are given it is not clear that he has ever tried this operation.

Not having ourselves had an opportunity of trying or seeing tried these operations, we shall not venture an opinion, but content ourselves with extracting Dr. Desgranges' conclusions:—

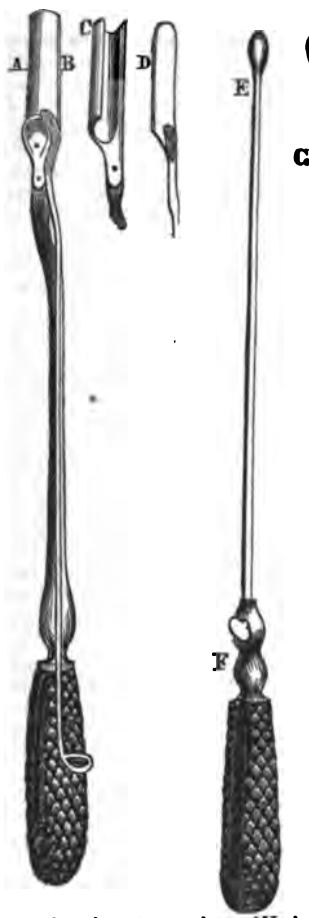
I. Prolapse of the uterus, though difficult to cure, is not incurable. II. It may be cured by an union of means, which constitutes the new method. III. This treatment for prolapsus may also advantageously modify other displacements and flexions of the uterus. IV. The basis of this method is the constriction and destruction of folds of the vagina by means of "*pinces.*" V. The "*pincement du vagin*" includes two procedures, the first by means of the small forceps, "*pinces vaginales,*" and the second by means of the "*pinces elytro-caustique.*" (A.) 1. *First procedure.*—As many "*pinces*" should be applied in the first operation as possible. 2. They come away themselves from the fifth to the eighth day. 3. The number of operations varies from eight to ten; better more than fewer. 4. The treatment occupies about three months. 5. No death, no local disorder, nor general symptoms. (B.) *Second procedure.*—1. An "elytro-caustique" is applied on each side. 2. They are to be removed in forty-eight hours. 3. The number of applications and duration of treatment must be determined by circumstances. 4. No death; feeble reaction; no local symptoms. 5. The caustic in the grooves does not alter the plan, as its action is limited, and the forceps alone may succeed. 6. The chances of cure are greatest in young and healthy women, and in the absence of complications. 7. Considerable "engorgement" of the cervix will require appropriate treatment; moderate "engorgement" need not interfere with the operation. 8. The cure is probably less owing to narrowing of the vagina, than to an organo-plastic process, which, extending beyond the organ, restores their lost tonicity to the utero-sacral ligaments, and to the areolar tissue of the pelvis. 9. The treatment does not prevent cohabitation nor fecundation, nor will it be a cause of dystochia. 10. If the cases are not sufficient fully to establish this method, they prove its harmlessness, and justify our hopes.—*Dublin Quarterly Journal of Med. Science, Nov.* 1854, *p.* 405.

111.—INTRA-UTERINE SPECULUM AND STILETS FOR CAUTERIZING THE INTERIOR OF THE CERVIX UTERI.

By M. Jobert.

In the number of the 'Bulletin Thérapeutique' for the 15th July, 1853, is reported the history of a patient on whom M. Jobert had performed intra-uterine cauterizations for the cure of an obstinate neuralgia of that organ, complicated with metrorrhagia. The operator, for want of a special instrument, then made use of a probe mandrel, heated to whiteness, introduced through the external orifice of the cervix uteri by means of an ordinary speculum. M. Jobert, reflecting on this case, has since conceived the idea of having a little speculum made which should serve both for exploring the interior of the cavity of the cervix uteri, and conveying to that part the heated iron, or any other local agent, the action of which it should be important to limit, so as to protect the neighbouring sound parts.

This speculum (made by M. Charrière) consists of a hollow cylinder A, B, which, when furnished with its stopper G, very well represents an ordinary solid speculum, but reduced to dimensions which admit of its passing through the orifice of the os tincæ, previously exposed in the field of another speculum. In order that it may be easily manœuvred it is furnished with a sufficiently long handle (fig. A, B).



This speculum is so divided that it can be at will transformed from a cylinder A, B, into a groove C.

The instrument having been introduced, the movable part of the wall of the cylinder D, which slides in a groove made in the fixed part C, is withdrawn by means of a special handle. The advantage of this division in exploring successively the several portions of the parietes of the cavity of the cervix uteri, in the same manner as we examine the walls of the vagina, by removing one of the valves of a three or four-valved speculum, is easily understood. An olive-shaped cautery, of a diameter corresponding to that of the speculum, may be introduced into the interior of the womb without touching the walls of the cervix when they are to be avoided, or may, at the will of the operator, be made to act only on a given point of these same walls, which is accomplished by taking away the movable blade D, and leaving in the uterus only the groove C.

M. Jobert has twice had occasion to use this instrument. In the two cases, which closely resembled one another, ulcerations existed in the very interior of the cavity of the cervix, with fungous granulations, secreting pus and giving rise to frequent hemorrhages. In one case cure was obtained after three cauterizations. The second patient is in progress of recovery.—*Dublin Quarterly Journal, Nov.* 1854, *p.* 467.

112.—*On Hypertrophy of the Womb.* By Dr. Meigs.—Let it be remembered that the womb is constitutionally prone to set off on a race of hypertrophic development, and we may then understand how it shall be readily provoked to commence a process of hypertrophisation, by the awkward, unnatural, and irritating posture it acquires in retroversion. We have met with some of these cases in which the womb grew so much that the fundus was jammed against the hollow of the sacrum, and the os against the symphysis, or on the soft parts above it, compressing the bladder most mischievously, but not at all bending the womb itself. In other specimens, we find the uterus bent like a retort. The angulation in some of the examples is almost acute; so that the calibre of the canal of the neck is seriously diminished, and its function as an efferent duct for the menstrua in a measure hindered: whence we have distressing dysmenorrhœa.

Medical men called upon to diagnosticate in such cases, should be very careful first to establish in their minds the ideal or standard, by which to compare; otherwise they will be apt, upon finding the whole pelvis occupied or blocked up with an immense and immovable solid mass, to conceive of it as a tumour, whereas it may prove in fact to be no tumour, but only an overset and enormously hypertrophied uterus. We have met with repeated examples of such misinterpretation of the case, women being supposed to labour under tumours within the pelvis, whereas the supposed tumours were nothing more than the posterior aspect of the corpus and fundus uteri turned over, and touched through the posterior membranous wall of the vagina; and wholly disappearing as soon as the womb could be properly reposited.—*Association Medical Journal, April* 28, 1854, *p.* 376.

113.—ON THE USE OF POTASSA FUSA AND POTASSA CUM CALCE IN UTERINE DISEASES.

By Dr. J. Henry Bennet.

[We are glad to perceive that the *fashion* of practising on the os and cervix uteri with these powerful caustics is subsiding. There may be cases in which they are useful, but we certainly think that the remedy has been dreadfully abused. Even Dr. Bennet acknowledges that in unskilled hands it is a dangerous remedy—a double-edged sword.]

M. Gendrin always used a paste made of the potassa cum calce of the Pharmacopœia, moistened with alcohol. I myself followed his example for some years; but, finding its application difficult, I first tried the caustic potash of Dr. Filhos, and then potassa fusa alone. The former consists of two parts of lime and one of potash, run into lead moulds. I found these tubes convenient for use, but not sufficiently active; whilst the pure caustic potash in cylinders was so very deliquescent, that it required a troublesome process of packing the surrounding parts with cotton steeped in vinegar, to limit the action of the caustic to the region on which it was intended to act. This induced me to try if I could obtain cylinders of potassa cum calce in a more active form: two parts of potash to one of lime, and in a free state—that is, not cased in tubes. In this attempt, with Mr. Squire's assistance, I completely succeeded, by casting it in iron moulds, and obtained sticks of potassa cum calce nearly as active as the pure potassa fusa, and yet having the non-deliquescent properties of the potassa cum calce paste. These cylinders, which are made of various sizes, render the application of this powerful remedy as easy as that of the nitrate of silver, no previous packing of the parts being necessary, and the action being limited to the part to which it is applied. Thus has been attained a great desideratum—a valuable agent, which could previously only be used with some trouble and risk, having been rendered manageable and safe.

The conditions of local uterine disease in which I consider that potassa cum calce may be used with advantage, are—*intractable* chronic inflammation, or inflammatory ulceration of the mucous membrane covering the cervix uteri, or lining the cervical canal; chronic inflammatory hypertrophy of the cervix; and lastly, chronic inflammation of the body of the uterus, in which form of disease I merely apply the caustic potash to the cervix, to produce a derivative issue.

The principles on which I have endeavoured to found the local treatment of the chronic inflammatory conditions which are so common about the cervix, its os and cavity, are those which ought to regulate the treatment of all inflammatory diseases of the skin and mucous membrane in explorable regions. If the acute or subacute stage of inflammation still exists, emollient applications and local depletion are indicated; if that stage has passed, and the disease appears in the chronic form, astringents should be used to directly modify the diseased capillary circulation, and they failing to restore healthy action, caustics should be resorted to, especially if ulceration be present, with a view to substitute healthy, reparative, manageable inflammation for that in existence, which is unhealthy, destructive, and unmanageable. This appears to me the true *modus operandi* of caustics and of the actual cautery, whenever they are used in the treatment of morbid inflammatory conditions, from a minute ulcer of the cornea to hospital gangrene. The inflammation set up by nature to throw off the eschar artificially produced, is naturally of a healthy, reparative kind, which admits of being controlled and brought to a favourable termination, *provided the stimulation be sufficiently powerful.* Thence it is that if one caustic, the nitrate of silver, for instance, does not produce the desired effect, another more powerful, such as the acid nitrate of mercury, may; and that failing, a still more powerful agent, such as the actual cautery or caustic potash, will certainly succeed. This law—for law it may be termed—deserves a more general recognition in surgery than it has hitherto obtained, for it points out the true mode of treatment in many intractable forms of chronic inflammatory disease. It will be observed that I speak of the actual cautery in the same paragraph with caustic potash, the *rationale* of the action of these agents being identically the same.

In chronic ulcerative disease, the caustic should only be lightly applied, the object being merely to renew the surface of the sore. In chronic hypertrophy, the object in view is rather different. It is not the destruction of the hypertrophied tissues

rhagia, or there is a constant sanguineous discharge, only interrupted by short and irregular intervals, with more or less leucorrhœa between.

If the polypus be of a species which will attain a considerable size, it gradually distends the uterus, and, by preventing that amount of contraction which closes the mouths of the vessels opening upon its internal surface, adds another cause of hemorrhage to that which already exists.

The health and powers of the patient soon flag under this continued drain; she becomes pale, feeble, and cachectic; the digestive organs are deranged, she has pain of the back, sense of weight and dragging about the hips; she finds that the hemorrhage is daily brought on more and more easily, until she is seldom free beyond a few hours, and then, perhaps, only when she preserves the horizontal position. "As the polypus grows larger," says Dr. Gooch, "it gradually dilates the uterus, till at length this organ, stimulated by its bulk, begins to contract upon it, protruding it through the dilated orifice. The polypus sometimes passes through the orifice gradually and insensibly, sometimes suddenly during the action of the bowels. I have known several instances in which patients, after this action, have been suddenly seized with retention of urine, and, on examination, a polypus was found in the vagina, compressing the urethra."—*On some of the Most Important Diseases Peculiar to Women.*

When once this change has taken place, the circulation of the polypus becomes a good deal impeded by the pressure of the os uteri which encircles it; the mass swells; large varicose veins, much distended with blood, begin to ramify over its surface, and give rise to severe hemorrhage by bursting either from over tension or the effects of ulceration.

From the foregoing account, it will be evidently of the greatest importance to detect the presence of a polypus at as early a period as possible before the patient has suffered much from the continued irritation and menorrhagia.

It is to Professor Simpson, of Edinburgh, that we are indebted for a masterly paper "On the Detection and Treatment of Intra-Uterine Polypi," in which he has pointed out the great value of dilating the os uteri with sponge tents, for the purpose of exposing and operating upon polypi which are still within the uterine cavity.

On examination of the polypus at an early stage, when it has not attained such a size as to press upon, still less to dilate, the os uteri, we find the os and cervix somewhat swollen, and the uterus itself more bulky than natural, very much as it is just before a catamenial period. Occasionally a careful examination with the uterine sound, if it be of the larger kind of polypus, will assist our diagnosis; but it usually happens, that we have no other evidence than what can be inferred from the character and history of the patient's symptoms, amounting merely to a suspicion of the case, the true nature of which can only be cleared up by dilating the canal of the cervix with a succession of sponge tents, and then examining the uterine cavity with the finger or the speculum.

If the polypus be one of the larger kind, it begins to press upon and dilate the os uteri internum. The cervix shortens, the lower segment of the uterus becomes evidently more bulky, the os uteri externum (os tincæ) loses its lip-like form; it becomes more circular, and is now sufficiently yielding either to allow the polypus to present, or at any rate to admit the tip of the finger to be insinuated far enough to feel it. A little careful manipulation will dilate the os uteri sufficiently to allow the finger to pass completely round the tumour. This will show us that its attachment is higher up, the distance of which will be, to a certain degree, indicated by its greater or less mobility. There is no doubt but that the uterus (in the same patient) varies greatly in size at different times, even from one day to another, depending on the congestion produced by the amount of exercise she may have used, by the period of the catamenial interval, the state of the bowels, or on the condition and tone of the patient's health, &c; so that we frequently find, in cases of polypus at an early stage, a considerable difference in the results of two examinations, although there has been not more than forty-eight or perhaps even twenty-four hours' interval between them. The os uteri may have become more closed, the uterus itself more contracted, so that what was easily felt by the finger at the first examination is quite out of reach at the second.

As regards the treatment of these cases, it will be guided by the nature of the polypus. The small mucous polypi of the os and cervix are easily snipped off with

the common tonsil scissors, which may be made long and slender, to use through a speculum where necessary. In many cases, these growths are so small, and hang by means of their slender pedicles so loosely in the vagina, that they elude and puzzle the finger, especially if there be more than one of them, as is usually the case. It is far better to have the assistance of the speculum here: the more so, as there are generally one or two smaller growths, which the scissors can scarcely get hold of, and which may be twisted off or crushed with a forceps; moreover, it will be desirable that nitrate of silver should be freely applied to the part afterwards. The little vesicle-like growths which Professor Simpson has described upon or beneath the membrane lining the cavity of the uterus, and chiefly seen in the lower part of the organ, may be picked off with a forceps, or even by the finger-nail.

With regard to the larger ones, I prefer the ligature, as being much safer than cutting them off, by which I have seen severe hemorrhage produced. The fine hempen cord which Mr. Ferguson, the well-known instrument-maker, procures for this purpose, is far superior to any other species of ligature which I have used.—*Medical Times and Gazette, Oct.* 28, 1854, *p.* 437.

116.—*Hearder's Condensed General and Obstetric Electro-Galvanic Machine.*—[The great objection to many improvements is that they are not *handy*. The busy practitioner cannot get through a good round of patients unless his remedies and appliances are ready for him. Consequently he neglects many of our best applications, and electricity amongst the rest. This, however, need not now be the case, since the introduction of Mr. Hearder's electro-magnetic machine.]

In its present form, it seems impossible to carry compactness and convenience to a higher point. The range of power of the instrument is so extensive, as to be more than equal to any therapeutical object. This power admits of the nicest and easiest graduation. The instrument is furnished with two sets of discs, of different sizes, and with a uterine conductor. Whilst its adaptation to obstetric emergencies is perfect and peculiar, it is equally fit for general medical purposes.

To give an idea of the compactness of this instrument, we may state that the length of the box which contains it, together with every requisite, including a bottle of dilute acid, for use, is seven inches, depth seven inches and a half, and the width less than eight inches. When arranging it for use it is not necessary to remove the coil or the battery from the box. The ingenuity displayed in getting rid of every superfluous complication or incumbrance, in reducing the essential parts to the narrowest space, and in attaining the highest degree of effectiveness and readiness of preparation, is perfectly surprising. No other machine we have yet seen approaches the elegant machine of Mr. Hearder, in the combination of those qualities.—*Lancet, June* 24, 1854, *p.* 670.

117.—*Cancer of the Womb.*—Mr. Lloyd has long employed in cases of cancer of the vagina or uterus which have come under his care in St. Bartholomew's, injections of arsenical solutions for the purpose of preventing fetor. He states that such treatment is perfectly innocuous, and that it succeeds far better than the ordinary disinfectants, which at the best are but of very temporary benefit, the smell generally returning as bad as ever quickly after their employment. The strength of the solution used is from two to eight grains of white arsenic to the pint of water. The great difference between the chemical disinfectants and such applications as this, no doubt arises from the circumstance, that the one only destroys fetor, while the other prevents it. The extraordinary influence of arsenical applications in cleaning cancerous ulcers is well known to those who have tried them. It is not necessary that they should be made to act as escharotics, since repeated dressings with a paste only strong enough to stimulate will effectually serve the end. The following prescription is that of the powder used by Mr. Startin at the Hospital for Skin Diseases for the purpose mentioned, and it differs, we believe, not very materially from one which was a great favourite with Dupuytren:—

℞. Hydrarg. chloridi ʒiss.; hydrarg. bisulphureti ℈ij.; acidi arseniosi ʒj. M.

This powder, made into a paste with water, is more or less lightly, as required, to be brushed over the sore, the application being repeated once a week, or fortnight, or month, as may be necessary. Mr. Lloyd's injection, in all probability, acts in the same manner on the sloughy, cancerous ulcerations concealed within the vagina. It must be borne in mind some real good is probably produced in addition to the prevention of fetor, since the diminution of discharge and sloughing will save the patient's powers, and perhaps, also, somewhat retard the growth of the cancer.—*Med. Times and Gazette, July* 1, 1854, *p.* 11.

118.—*On Uterine Polypus, its Nature, Early Detection, and Treatment.* By Dr. Robert Barnes.—[Dr. Barnes brings forward some interesting cases of polypus uteri as introductory to some more general remarks. It is easy enough to find out and treat a polypus which has descended into the vagina. The difficulty is to form an accurate opinion about one which is still small and out of sight and touch. It is, therefore, during their early stage of formation, before they have cleared the os uteri, to which Dr. Barnes particularly draws our attention.]—*Lancet, June* 10, 1854, *p.* 613.

119.—ON POLYPI SPRINGING FROM THE MUSCULAR WALL OF THE UTERUS.

By Dr. Robert Barnes.

[Dr. Barnes says that little has been added to the observations of Vogel on this subject.]

Not only was the muscular character of the fibrous tumour of the uterus clearly ascertained by that eminent pathologist from the examination of tumours taken from both unimpregnated and puerperal uteri, but he appears to have arrived at a higher and more general law—namely, that many fibrous tumours developed in the organic muscular structures of other organs, as well as the uterus, are likewise muscular. I think, then, it may be concluded that the so-called fibrous tumour of the uterus, is in reality a muscular tumour: and there can be no doubt that the so-called fibrous polypus is nothing more than a fibrous tumour of the uterus, which in the course of development, or in some cases as the result of an effort of elimination, is made to project into the cavity of the organ, preserving only a pedicular attachment. Dr. Lee cites Dr. Sims as describing the progress of these tumours of the uterus in the following manner:—1st, If they happen to be developed in the centre of the muscular wall, they grow there; 2nd, If near the outer wall, they expand into the cavity of the abdomen covered by the peritoneum; 3rd, If near the mucous surface, then they project into the uterine cavity, protruding before them a layer of muscular tissue, so that the neck of the tumour would be formed by a portion of the substance of the uterus. Boivin, Dugès, Breschet, and other pathologists, have entertained the same view.

The Frequency of Fibroid Tumours and Polypi.—The identity of structure of the fibroid tumour and the uterine muscular tissue, offers the readiest explanation of the great frequency of fibroid tumours. Bayle has observed that out of 100 women who had died after the age of thirty-five years, in twenty, tumours of this description were found in the uterus. This frequent occurrence of fibroid tumours gives strength to the presumption that fibroid polypi also are more common than is perhaps suspected.

The Mode in which Muscular Tumours become converted into Polypi is a subject of considerable interest. The first appearance of a polypus has frequently been observed immediately after delivery; and the history of many of these cases leaves no room to suspect, or at any rate affords no distinct evidence to justify the conclusion, that a tumour of a polypoid form existed in the cavity of the womb prior to delivery. It appears to me that the process of extrusion of a solid tumour from the walls of the uterus into the cavity, admits of a satisfactory explanation.

The anatomical identity of these tumours with that of the muscular walls of the uterus, renders them susceptible of the same influences that act upon the uterus itself. Under the stimulus of pregnancy, the same developmental activity will be

imparted to both. It has accordingly been repeatedly observed that these tumours grow with great rapidity during gestation, and that they become diminished in size after delivery. The latter process, however—that of retrocession—seldom, perhaps never, takes place to an extent proportionate to the diminution of the normal muscular fibre of the uterus. The texture of the tumour is usually more dense and compact than that of the uterine walls, and consequently less capable of contraction. The process of extrusion depends mainly upon two conditions—1st, The difference in the degree of density of the proper muscular tissue of the uterine wall, and of the contained tumours; 2nd, The contractile efforts of the uterus. When a tumour of greater solidity than the proper uterine tissue is embedded in the walls of the uterus, it will almost necessarily be thrust outwards or inwards when the uterine walls contract upon it. It cannot follow or partake in the uniform contraction of the organ; as an unyielding body, preserving to a great extent its original dimensions, it must be driven towards one or other surface of the uterus as this diminishes in size. An attentive consideration of the history of many cases in which polypi have first been observed to come down immediately after delivery, amply confirms this proposition. Not to encumber this essay with unnecessary cases, I refer to the account already quoted of the case of Dr. Crisp.

Previously to parturition there was no suspicion of a polypus in the womb. After the birth of the child, and the removal of the placenta, then some other substance, which Dr. Crisp at first thought might be a second child, was felt. At a later period, violent expulsive pains continuing, Dr. Chowne discovered a mass in the womb, now distinctly polypoid in form. Energetic action of the womb still going on, the polypus was next forced low down in the vagina. The dense structure of this polypus is especially attested both by Dr. Oldham and by Dr. Bristowe.

This process, by which the uterus endeavours to cast off a tumour after delivery, merits the careful attention of the obstetric practitioner. It is not difficult to imagine a case, in which the extrusion of the tumour may be less complete than in the above case of Dr. Crisp. The tumour may be retained in the uterine cavity, where its existence may not be suspected. Its presence there may be the occasion of long-continued and even fatal hemorrhage.

That the process by which a muscular tumour, in an unimpregnated womb, assumes the polypoid form is sometimes similar to that just described, is exemplified in the first case of Professor Vogel. It can hardly be doubted that the presence of the tumours in the walls of the uterus was the cause of the violent contractions, simulating labour-pains, which in all probability brought about the extrusion of one of these bodies into the cavity of the womb. In this instance the contractions had actually effected the entire detachment of one tumour, which was found lying free in the cavity. Another step, and it might have been expelled from the body altogether.

The process of extrusion is further facilitated by the slightness of the attachments by which these muscular tumours are connected with the uterine walls in which they are imbedded.

It is certain, however, that uterine tumours, when they become polypi, do not do so, in all cases, under the agency of active uterine contraction. Sometimes this takes place as the result of the different ratio of growth of the tumour and of the uterus. A dense, solid substance, isolated from the uterine wall in which it is imbedded, and continuing to grow, whilst the uterus itself partakes but slightly in the process of enlargement, will in time form a projection upon one or other surface of the organ. A further stage of growth will cause it to bulge more and more prominently, until it acquires a distinct polypoid form.

It is worthy of remark that fibrous tumours are very rarely found before the age of puberty; if they are, they remain passive until the period of activity of the generative system. After the child-bearing period, and the cessation of menstruation, fibrous tumours previously existing exhibit a marked tendency to recede. It is, I believe, a very rare occurrence to observe that any fresh tumours become developed after this period. This period of active growth of fibroid tumours and polypi is the period of functional activity of the generative organs. The periods of greatest activity of growth of these tumours are the periods when the generative organs exhibit the greatest activity. The periodical stimulus the uterus undergoes at the epochs of menstruation is shared by the tumours lodged within its walls. The

rapid enlargement of the uterus during pregnancy is attended by an at least commensurate growth of the tumours.

But, although it may be laid down as a general rule, that fibroid tumours do not continue to grow after the termination of the normal period of menstruation, it must be admitted that exceptions occur. I have even observed that the constitutional ferment which frequently attends this critical period of life seems to determine in the temporary exacerbation of any form of uterine disease existing at the time. The organic force which had hitherto been exerted in healthy physiological processes is now suddenly diverted, and expended in a pathological direction. The disposition to floodings under these circumstances is often greatly increased. The greatest care is necessary in order to carry the patient over this stage of peculiar danger, and to gain in safety that point when the commotions of the critical period shall have subsided.

The Degeneration of Polypi.—These muscular tumours and polypi not unfrequently undergo a process of retrocession, or even of degeneration. In the case of tumours which have enlarged simultaneously with the gravid womb, the retrocession that takes place is probably effected by the same process of fatty degeneration of the newly-developed fibres, which Mr. Rainey has described as taking place in the proper muscular structure of the uterus. It has been supposed that fibrous polypi are liable to become converted into scirrhus or cancer. It can hardly be admitted that the abnormal muscular growth of which they are composed is more liable to such a change than is the normal muscular structure of the womb. A muscular fibre cannot be changed into cancer. It may, however, give place to it. It is quite possible that the cancer element may be developed in the substance of uterine tumour, as it may be in the proper substance of the uterus; and that the activity of the new growth may cause the atrophy of the old, and the gradual substitution, not conversion, of a cancerous tumour for a benignant polypus.

In muscular tumours and polypi of long standing, the vessels often become very scanty, or disappear. Their entire structure sometimes undergoes an earthy or bony degeneration. In this condition, hemorrhages which had attended the earlier stages of their growth often cease. They seem to be removed by this change from the sphere of organic activity, and excite little or no irritation in the organs with which they are connected.—*Lancet, July* 1, 1854, *p.* 687.

120.—ON UTERINE POLYPI AS THE CAUSE OF HEMORRHAGE.

By Dr. Robert Barnes.

[In all cases of uterine hemorrhage where we cannot find out any constitutional cause, nor local disease, it seems evidently our duty to ascertain if polypus uteri does not exist. Out of thirty cases of uterine hemorrhage, when all other causes were excluded, Dr. Barnes found eight cases of detected polypus, eight cases of enlarged womb, and fourteen cases of tumours of the womb: so that the cause of the hemorrhage in all these cases was beyond tactile or visual examination. Dr. Barnes proceeds to say:]

Before considering the physical means of following up this search, I wish to point out one symptom which may furnish an useful indication. When the uterus contains a foreign body, even of small size, it rarely happens that contractions are not excited—that efforts are not made to expel it.

The occurrence, then, of spasmodic intermitting pains, resembling those attendant upon abortion or labour, together with globular enlargement of the womb and hemorrhage, point, under the exclusive conditions premised, with a high degree of probability, to the presence of a polypus.

It must, however, be admitted, that the general symptoms can furnish at best but presumptive evidence. In the case of pregnancy, which is attended by general signs of a marked character, no practitioner of experience undertakes to base an absolute opinion upon the general signs alone. He looks for the direct physical evidence afforded by *ballottement*, the placental rush, or the sound of the fœtal heart. In the case of polypus, the general symptoms can never rise to that point of distinctness which is usual in pregnancy; the necessity, therefore, of seeking for direct physical evidence is still more imperative.

The Physical Means of detecting Polypi.—It may be that the polypus has descended so low down as to be on the point of escaping through the os uteri. In this case it may, under favourable circumstances—that is, during the relaxation and forcing down attendant upon an attack of flooding—be perceived by the finger. Its detection is even more probable by the use of the bivalved speculum, which, by expanding, opens the os, and permits the inspection of a certain portion of the cervical canal. The uterine sound is also available in further opening the os. An observation of Dr. Locock's clearly expresses the importance of making the examination during the existence of the hemorrhage. He says—"I never discovered the polypus in these cases when I examined the uterus in the intervals between the attacks of hemorrhage, either by the finger or speculum. The os uteri closes in the intervals of the attacks. The tumour comes down during hemorrhage." It has occurred to me in several instances to have observed a polypus at one examination, which could not be detected on a subsequent occasion.

Another cause that may help to conceal a polypus in the cervix, is the congestion and swelling of the os, induced by the irritation caused by the polypus, or arising from independent circumstances. This condition may be removed by appropriate treatment, and when the os has been restored to its natural state, the polypus may be brought into view.

The Uterine Sound.—As a means of extending tactile perception beyond the point which it is possible to reach by the finger, the uterine sound is in some cases a most valuable instrument. By its means it will generally be possible, in cases where the os and cervix are sufficiently open, to determine whether or not a movable body be present in the uterus.

Should our conviction or our presumption, derived from the evidence of the uterine sound or from other sources, be so strong, that a polypus is present in the cavity of the womb, we possess means of bringing it into view. By causing contractions of the womb the contained body may be expelled. The ergot of rye has been used for this purpose, and no doubt sometimes with success; but I greatly prefer the agency of *galvanism*, as being both more safe and more efficacious. Dr. Tyler Smith has already applied galvanism in one instance for this purpose, with complete success.

Dupuytren recommended the practice of opening the cervix in these cases by *incisions*. Although I can conceive a case in which this operation may be justifiable, I believe that the necessity for resorting to it must very rarely occur, since we possess another means of effecting the object in view, with as much certainty and with less danger.

The plan of *dilating the os and cervix uteri* by means of sponge-tents, proposed by Professor Simpson, finds, in cases of this description, its most valuable application. The use of two or three graduated sponge-tents in succession will mostly succeed in opening up the canal of the cervix to an extent sufficient to admit of a perfect exploration by the eye and the uterine sound of the cavity of the uterus.

I am aware that the proposal to use sponge-tents, for the purpose of dilating the os uteri, has met with considerable objection. It has been urged that such a procedure may cause inflammation and other serious consequences. It does not appear to me that these objections are substantiated by experience, or by sufficient reason. Sponge-tents have been long employed for the purpose of inducing premature labour. Dr. Lee has sanctioned by his example their use for this purpose. The state of the cervix during early pregnancy does not differ widely from that of the cervix when an intra-uterine polypus exists. When a polypus is present, under the influence of the attendant hemorrhage, leucorrhœa, growth of the tumour, and action of the uterus, the cervix is already disposed to relax and to expand; and it does not seem reasonable to apprehend that the presence of a soft piece of sponge in the os uteri should be more hurtful or dangerous than the often-times far greater pressure exerted upon the cervix by a polypus. Under many circumstances, of not greater urgency, that occur in obstetric practice, those physicians who express so much dread of the possible effects of the sponge-tent, do not hesitate to enjoin the violent expansion of the uterine neck by the hand.

To exclude this application of an agent which experience has proved to be both safe and effectual, is, in effect, to pronounce—in the case of an obscure disease, situated higher up than the os uteri, however threatening to life—that the resources

of medicine are exhausted, and that the patient must be abandoned. To urge, as it has been urged, that if you wait long enough, the polypus, if present, will not fail to make its way into the vagina, is to advocate a delay that may be fatal. The treatment, by expectancy, is surely inappropriate when an uncontrollable flooding is fast destroying the patient.

M. Jobert de Lamballe has recently contrived an *intra-uterine speculum*, which may possibly be of service in assisting the visual examination of the uterine cavity and the cavity of the cervical canal. This instrument consists of a hollow cylinder, and a long stem to fit; the circumference is so small, that it can be passed into the uterine neck, the os uteri being previously brought into view by the ordinary speculum. By the withdrawal of a slide attached to the stem, the cylinder may be converted into a groove, so as to permit of an inspection of any part of the canal. [See page 204 of this volume.]

I cannot speak of this instrument from experience. Possibly its utility may fall short of the expectations of the inventor. Its employment would be facilitated by the previous expansion of the uterine neck by means of the sponge-tent.—*Lancet*, *July* 29, 1854, *p.* 79.

121.—CASE OF INTRA-UTERINE POLYPUS.

By Dr. Robert Barnes.

[The following case is a good one to illustrate some of Dr. Barnes' views of treatment of this disease.]

In the night of the 3d of August, my colleague, Dr. Ramskill, was called to see a young woman who was suffering from uterine hemorrhage. The history of the case is briefly this: the patient was twenty-six; she began to menstruate at the age of fourteen, and this function was performed very regularly until her marriage, eight months ago. From that time she has suffered almost perpetual hemorrhage. A month ago the flooding was so profuse that it was thought that she had miscarried. Since then there have been slight occasional intermissions, but her health was deeply impaired. When Dr. Ramskill was called the hemorrhage had returned. He observed strong bearing-down, expulsive efforts. The patient died the same night in convulsions, evidently from loss of blood.

The body was examined by Dr. Ramskill on the following day. The organs were all healthy. There was no abdominal inflammation. The os uteri was healthy, but flaccid: it was filled with a fresh clot. There was also blood in the cavity of the uterus. The larger portion of the uterus, with a body adhering to the inner wall, was forwarded to me by Dr. Ramskill.

I subjected the parts to a careful examination. The walls of the uterus were dense, pale, somewhat thicker than natural, and the whole size of the organ somewhat larger than the normal unimpregnated womb. There was no tumour or other abnormal condition of the muscular wall, but attached to the inner surface near the fundus, and altogether enclosed within the cavity of the uterus, was a tumour of the size of a small walnut. The tumour did not reach to the uterine neck. The mucous membrane of the cavity was stretched over it. It was connected by a broad basis to the uterus, but would have admitted of isolation by ligature. The apex, or most projecting part, had undergone partial disintegration; it was a little broken up, softened, and had evidently quite recently been the source of hemorrhage. Examined by the aid of the microscope, the substance was found to consist of nucleated fibres, the nuclei being large and distinct. Portions, especially those taken from near the apex, exhibited abundance of oily globules and numerous blood-globules. The structure of the tumour differed from that of the uterine wall in this respect only, that the fibres in the latter were longer, narrower, and more densely interwoven, and the nuclei less distinct. There was no evidence of fatty degeneration in the fibres of the uterine wall. There was no doubt greater developmental activity in the tumour than in the uterus.

The practical deductions from this case are of the highest interest and importance.

1. The condition of the uterine muscular walls leads me to conclude that the conjecture that the patient had aborted a month before her death was erroneous.

2. The comparative indolence of the tumour, and the absence of any remarkable amount of hemorrhage up to the period of marriage, and the constant floodings

following immediately upon that event and continuing until the death of the patient eight months afterwards, forcibly illustrate the influence of ovarian and uterine stimulation in developing the growth of uterine polypi.

3. The case is peculiarly one of that class to which I have pointed as strongly indicating the necessity of exploration beyond the os uteri. In this instance the os uteri was found healthy; there was no obvious sign of local disease to explain the hemorrhage; the bulk and weight of the uterus were rather above the normal standard; the figure of the organ was regular. Everything pointed to the uterine cavity as the seat of the cause of the hemorrhage. It is, in short, the type of the class of cases in which the dilatation of the uterine neck is indicated.

4. The operative proceeding which in my opinion would have been the means of rescuing this poor woman from bleeding to death, is the following: repeated doses of ergot of rye, and the application of galvanism to aid the natural efforts of the uterus to effect the scission and expulsion of the tumour; and the dilatation of the uterine neck by sponge-tents. By these means combined, no long period would have elapsed before the tumour would have been brought into view. If it had been then found difficult to encircle the neck of the tumour with a ligature, the operation might have been facilitated by bringing the tumour lower down by the aid of a tenaculum. In the present case, I believe the tumour admitted of being so removed. But we may imagine a case where ablation would not have been expedient or possible. In such a case the local application of the acid nitrate of mercury, potassa fusa, or the actual cautery might have been justifiably resorted to in order to arrest a hemorrhage that was fast hurrying the patient to the grave. Such an application might also cause the destruction of the tumour by disintegration or degeneration.

5. The softening and partial breaking up of the tumour observed after death suggests to me that the microscopical examination of the discharges might have detected some of the constituent nucleated fibres of the tumour, and have aided the diagnosis.

6. I would finally remark, in answer to those who deprecate any operative interference with polypi that have not yet made themselves visible at or below the os uteri, that the intervention of surgical aid is not indicated by the mere position of the polypus, which is altogether unimportant, but by the terrible attendant hemorrhage, no matter where the polypus may be. If this be so, and surely the proposition cannot be disputed, then it follows that the removal of an intra-uterine polypus may be as urgently called for as the removal of one that has descended into the vagina.

In the case of an abortion, or of the retention of a portion of placenta, no obstetric practitioner is ever satisfied as to his patient's safety until the offending substance is removed from the uterus. Why should the patient be abandoned when the offending substance is a polypus?—*Lancet, August* 26, 1854, *p.* 168.

122.—ON DIEFFENBACH'S LOOP SUTURE.

By T. Spencer Wells, Esq.

[In his practical essays on Plastic Surgery, Mr. Wells points out the different kinds of sutures which are used, such as the *quill* suture, the *clamp* suture, the *bead* suture, and the *lead* suture, but we wish more particularly to draw the reader's attention to Dieffenbach's *loop* suture, for the closure of fistulous openings.]

The *subcutaneous* or *loop* suture is an admirable contrivance of Dieffenbach, for the closure of circular fistulous openings. It is especially useful in cases of salivary fistula, fistula of the male urethra—vesico-vaginal, recto-vaginal, and vesico-rectal fistulæ—intestinal fistula, and preternatural anus, all of which have been healed by the skilful application of this suture after every other mode of treatment had proved unsuccessful. The edges of the fistula having been prepared for union by paring, cauterisation, or blistering, the patient is ready for the application of the suture, the description of which I shall quote from Dieffenbach:—

"A sewing needle very much curved, charged with a single thread, and held near the eye sideways by a pair of needle forceps, is pushed through the skin at some distance from the opening to be closed, the edges of which have been previously inflamed by cauterisation, or pared away. Then the needle is carried beneath the

skin in the subcutaneous cellular tissue for some extent, always keeping at an equal distance from the opening, so that a quarter or one-third of a circle is described. Then the point of the needle is directed outwards, the skin transfixed from within outwards, and drawn with the thread some inches out. Then the needle is returned through the same opening beneath the skin, another quarter of a circle is made below, the needle again brought out and carried a third or fourth time beneath the skin, until the whole opening is surrounded at an equal distance by the thread. At last

the needle comes out through the first point of puncture, and as the thread hangs out a finger's length from the opening, so by means of the other end, still in the eye of the needle, a tying together of the circular ligature can be effected. If the opening is small, and the thread lies near it, it may be tied moderately fast together by two knots, but much looser than when a large artery is tied. The knot lies upon the opening of the needle puncture, and generally sinks within it. The ends are now somewhat shortened, and the opening, if the edges are torpid, is smeared with tinct. cantharides; otherwise they are covered with strips of plaster, in order to assist the subcutaneous process of agglutination, and prevent the entrance of air by the needle punctures.

"When large openings or fistulæ have to be closed, for example in preternatural anus, into which a finger can be introduced, the same ligature is applied in an enlarged manner. In a narrow urinary fistula, I take a single fine thread, and a fine curved needle; introduce it at the most two lines from the border; but in the other case I use one of the largest sewing needles, and a very thick waxed silk cord, of four, six, to eight thick silk threads, and carry this an inch distant from the opening around it, until the thread is again brought out by the needle at the first point of puncture. In order to avoid rapid cutting through of the soft parts, to which no union would follow, it is advisable only to draw the ends of the thread so strongly that they gently press the walls of the opening. To do this, a round piece of cork, notched in the middle on one side, is laid between the two ends of the ligature, a knot is made in the notch, and then a bow, to be tightened afterwards. This is done after some days have passed, when the ligature has become loose.

"Small fistulous openings sometimes close after the first suture. The dressing is first examined on the fourth or fifth day. If the thread is firm, it is left for eight days, and it is better to withdraw it too late than too soon. This is done just as a common suture is withdrawn, the end of the threads being stretched, and the ring cut through with fine-pointed scissors in the enlarged needle puncture, and the ligature carefully drawn out.

"In large openings, every two days it is tied a little tighter, even if the closure has not followed, but always with great delicacy, so that the thread does not cut through for at least fourteen days or three weeks. In the most successful, or tolerably successful cases, the thread must not be removed too early. Even where it has cut through all the surrounding parts, the irritation set up in the edges and parts around produces considerable diminution in the opening, partly by the contracting cicatrices of the edge, partly by the constricting subcutaneous adhesion of the parts cut through by the thread.

To the application of this suture I owe many cures which resisted every other treatment. Treatment by this method is often a troublesome undertaking, requiring frequent repetition; but it has the advantage that each failure, not like the carrying away and suppurating through of the common or twisted suture, is not a true failure, but only a partial success, and a step forward towards the goal. Therefore the operation is repeated until the opening is perfectly closed."—*Medical Times and Gazette, July* 29, 1854, *p.* 109.

123.—*Painless Labour.*—Mr. Cousins was called, on October 13th, to a lady, aged twenty-six, eight months gone in her first pregnancy, who had been exerting herself during the day preparing for a dinner party. At half-past five p.m., a sudden gush of liquor amnii took place, carrying down the funis of a child; this was replaced, but again descended in ten minutes; it ceased to beat, and the os uteri being readily dilatable, Mr. Cousins turned the child. No sensation of pain had occurred until

this operation, which gave rise to some slight suffering: the child, a small one, was easily expelled by the expulsive efforts of the uterus: throughout this there was no pain besides that described above. When asked during the uterine contractions, she averred, without any falter of voice, that she felt no pain whatever. She gave directions which were required, and sustained an easy flow of conversation. Mr. Cousins watched the descent of a second child, a healthy, strong boy, the passage of whose shoulders *alone* caused pain: during the uterine contractions, she, now that her attention was aroused, "felt, as it were, a band pressing about her," but nothing more; and she recollected that, for twelve hours before she sent for him, she had more than once remarked to a servant, "how tight she felt all at once." Throughout the day the characteristic mucus had been abundant.—*Lancet*, *Nov.* 4, 1854, *p.* 387.

124.—ON SOME AFFECTIONS OF THE RECTUM CAUSED BY CERTAIN CONDITIONS OF THE WOMB.

By I. B. Brown, Esq., London.

Displacement forwards or backwards, and enlargement of the uterus, from whatever cause, whether pregnancy, hypertrophy, inflammatory engorgement, distension by fluid or by hydatids, polypi, or scirrhus, or any other disease, alike tend to injuriously affect the rectum.

As displacement may occur without enlargement of the uterus, it may operate singly in inducing rectal disease; but more often the two conditions concur, and it is then chiefly that the mischief is so considerable. The evils, too, will be greater when, with retroversion, engorgement of the body of the uterus, and with anteversion, that of its neck, go together. On the other hand, enlargement, without deviation of the womb forwards or backwards, may, and oftener does, act singly in provoking disease of the rectum, than either of these displacements does without it.

The conditions of the uterus under consideration act on the rectum injuriously in two ways; first, by mechanical pressure; and, second, by inducing vascular disturbance like that present in themselves. An enlarged uterus drags on its lateral ligaments, elongates them, subsides lower down in the pelvis, and so comes to press on the lower bowel, to interfere with its muscular action, and the circulation through its blood-vessels, and to irritate its mucous lining. At the same time any hyperæmic state of the uterine vessels causes an increased fulness of the hemorrhoidal, and a determination of blood to them. Thus, by reflecting on the anatomy of the parts, it will easily be understood why and how diseases of the rectum, such as hemorrhoids, prolapsus, fissure, stricture, fistula, as well as disordered functions of the bowel, as constipation, dysenteric irritation, &c., do sometimes result directly, either from the mechanical pressure of an enlarged uterus, or simply from the derangement of the hemorrhoidal circulation, resulting from uterine disease.

[The following case illustrates Mr. Brown's practice.]

Case.—L. C., aged 31, unmarried; had suffered for several years from three or four large piles, which, she stated, protruded in the act of walking, and also created great uneasiness in the sitting posture. Every three or four weeks she lost a considerable quantity of blood from them. Her bowels seldom opened more than once in three or four days, and then only from taking some aperient. After each defæcation, she had always increased inconvenience and pain. She suffered much from indigestion, headaches, and general lassitude. I first enquired as to the menstrual function, and found that, although she was generally regular every four weeks, the quantity was very small, and the discharge seldom continued more than one day, and that, at this time, she generally lost a considerable quantity of blood from the hemorrhoids. On making a digital examination per vaginam, I found the uterus enlarged and painful to the touch, and lower in the vagina than normal. I directed that three leeches should be applied to the os uteri, and that the bleeding should be encouraged by a hip-bath immediately on their removal; that the bowels should be freely opened by a saline aperient; and that this treatment should be pursued once a week. She was further directed, to take twice a day the sixteenth part of a grain of the bichloride of mercury, and her diet was ordered to be simple, nutritious, and unstimulating. The result of these measures was, that at the next menstrual epoch there was a more copious uterine discharge, and an absence of bleeding from the

hemorrhoidal vessels. The same treatment was continued for another month, and resulted in an improvement in menstruation, both in quantity and in the duration of its flow; the uterus had much decreased, being nearly of its normal size, and not painful to touch. I now considered it advisable to remove the hemorrhoids, which I did by passing a ligature of twine round the base of each. I then placed the patient in bed, giving her a grain of opium every four or six hours, for the purpose not only of relieving her from pain, but also of constipating the bowels till the ligatures should come away, which they did in five or six days. The case progressed satisfactorily, and the patient returned to the country perfectly recovered.

This case presents some striking practical facts, which serve to illustrate my preliminary remarks.

1. The bleeding from the hemorrhoids at the menstrual epoch. It is a fact that hemorrhoids are always more troublesome at the time of menstruation, because more blood is circulating through the blood-vessels of the rectum as well as of the uterus; and, if there be any obstruction in the uterine vessels to prevent the menstrual flux, then the hemorrhoids often bleed freely.

2. The enlarged and inflamed uterus, with the absence of the menstrual flux.

3. The great relief afforded by the antiphlogistic treatment.

4. In ligaturing the hemorrhoids I prefer *twine*. Its advantages over silk are very marked:—first, in procuring a quicker separation of the tumour; and, secondly, in causing less pain to the patient. It will also be observed, that I prefer the ligature to the scissors in the removal of these tumours, acting upon the golden rule laid down by my old and esteemed friend, Mr. Copeland, in all operations upon the rectum, "*to cut skin, and tie mucous membrane.*" I am also indebted to the same gentleman for the practical hint to prefer the use of the vegetable product—twine, to that of the animal product—silk.

I think it is not too much to assert, that had I at first devoted my attention to the hemorrhoids in this case, I should not have succeeded in restoring my patient to the condition of health.—*Monthly Journal of Medical Science, Nov.* 1854, *p.* 441.

MISCELLANEOUS SUBJECTS.

125.—ON THE CLIMATE OF EGYPT.

By the Rev. Thomas Barclay, D.D., Currie Manse.

[The following remarks by Dr. Barclay will be read with interest. Now that locomotion is so rapid and so comfortable, and likely to be so, even in Egypt itself, a journey to Thebes may be included in the treatment of pulmonary diseases. From the beginning of October to the end of April, is the time when the climate of Upper Egypt is most delightful.]

The malady for which I sought relief in a southern climate, was chronic bronchitis in its most aggravated form. All the usual remedies, both external and internal, had been resorted to, and steadily persevered in, under the ablest medical advice: but with little temporary and no permanent benefit. I had tried with the same unfavourable result those places on the south coast of England, which are usually recommended to invalids. The symptoms obstinately resisted every remedial measure. The chronic character of the disease was frequently exchanged for attacks of a subacute form. These always commenced with inflammation of the pharynx, creeping insidiously down the glottis and trachea to the bronchial tubes, which became gorged with mucus throughout their whole extent; and on every spot on which the stethoscope could be planted over the lungs, the mucous *rôle* was to be heard. Dyspnœa, accompanied with loud wheezing, was at all times distressing; but its nocturnal exacerbations, which invariably occurred after a short sleep, like fits of spasmodic asthma, were often so fearfully violent as to threaten suffocation. The digestive organs were deranged—I had no appetite for food—my frame was emaciated, and my strength prostrated.

Warm and equable as the winter temperature is at Cairo, the weather at that season is not free from frequent and sudden changes. It is in Upper Egypt that the invalid must seek entire exemption from these; and there he will not be disappointed.

While I was there, my register was kept on the Nile, and consequently it shows a lower temperature, at least in the night time, than would be denoted at a little distance from the river; while the dryness indicated by the hygrometer in the latter case, was many degrees greater than that registered on the river.

The following table contains the result of the observations noted between Thebes and Asouan, the uppermost town in Egypt, and from these an idea may be formed of the winter climate in that region:—

Thebes.

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
	°	°	°	°	°	
Jan. 17.	64	70	6	7	. .	Cloudless sky and bright sunshine every day; the firmament blazing with stars every night; no evening chills.
" 18.	63	68	5	7	. .	
" 19.	66	70	4	5	. .	
" 20.	64	69	5	12	. .	
" 21.	64	71	7	7	. .	
" 22.	69	72	3	6	. .	
" 23.	68	72	4	10	. .	

The benefit I derived from breathing the genial air of the Thebaid was very decided. The periodical night attacks, though still occurring, were less violent and of shorter duration; my breathing was greatly relieved; and my strength was so far recruited that I was able, without fatigue, to make daily excursions, sometimes of many miles, to the monuments with which this part of the valley of the Nile is studded.

At Thebes, which for several reasons should be made head-quarters in Upper Egypt, the day temperature from the middle of January to the middle of February, ranges from 68° to 78°, and after the latter date, the heat becomes rather too great to be borne with comfort. The invalid should then commence his downward voyage, and by the time he reaches the latitude of Cairo, he will find the climate there nearly as delightful as that which he left at Thebes. My register during the first week of March at Cairo was as follows:—

Cairo.

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
	°	°	°	°	°	Gent. brze., sunny.
March 1.	66	72	6	9	. .	Do. do.
" 2.	69	74	5	4	. .	Fog.
" 3.	68	75	7	0	0	Calm and sunny.
" 4.	68	66	8	6	. .	Haze.
" 5.	71	74	3	. .	2	Cloudy and sunny.
" 6.	69	72	3	6	. .	Drops of rain.
" 7.	65	69	4	6	. .	

In a city like Cairo, with a dense population of more than two hundred thousand people, and with narrow streets walled in by lofty houses, and constantly watered to lay the dust, the air can neither be so pure nor so dry as in the desert. Accordingly, the medical gentlemen of Cairo are in the habit of sending almost all their convalescent patients to reside for some time in the adjoining desert, to enjoy the benefit of its invigorating air. In it there is no water, either running or stagnant, to produce humidity by evaporation, nor is there any decaying matter, either vegetable or animal, to taint the atmosphere with noxious exhalations. It is in the desert, therefore, that the qualities of warmth, equability, dryness, and purity, which are characteristic of the Egyptian climate in general, are to be found in the most perfect union. The soothing influence of the desert air on delicate organs of respiration, and its invigorating influence on a debilitated frame, can be appreciated by those only who, like myself, have experienced its marvellous effects. It is at once balmy and bracing; and the invalid, while breathing it, feels as if he were drinking in health at every pore. I quitted Cairo for the desert of Gheezeh, on the 12th of March, and took up my abode in the neighbourhood of the Pyramids; and there a sudden change came over me, as if by magic. The second night I passed in the desert was marked by sound and uninterrupted sleep, and the absence of the periodical fit of dyspnœa—the first occasion on which I had enjoyed the one, or had been exempted from the other, for more than two years. My appetite soon became *excessive;* both the flesh and the strength I had lost during my illness were restored; every symptom of my complaint disappeared; and at the end of a month, I returned to Cairo in perfect health.

The only other place in Egypt whose meteorology it seems necessary to notice is Alexandria. When it is borne in mind that this city is surrounded on three sides by the Mediterranean Sea, and that on the land side it is enveloped by the Lake Mareotis, a moist atmosphere may be expected; and accordingly, the degree of humidity is excessive. In this respect the state of the atmosphere in Alexandria, during the early part of May, will be found to contrast remarkably with that of Cairo and its vicinity during the latter part of April; while the greater equability of temperature in the former city, both from day to day and during the twenty-four hours, is no less observable.

How far Alexandria, during the months of March and April, may be a more suitable residence than Madeira or Italy, for those whose complaints require a climate at once warm, equable, *and moist*, I leave it to gentlemen of the medical profession to judge; but I do think myself fully warranted to denounce it as a most unsuitable place for a bronchitic patient. During all the time I was there I felt as if inhaling steam; my breathing was excessively affected, and my whole system was languid and relaxed. These effects, however, by the time I had been twenty-four hours at sea, were completely dispelled, leaving no doubt whatever as to their cause.

I may add, that dyspepsia is very prevalent among the European residents in Alexandria; and I was informed that cases of pulmonary consumption, though not common, do occasionally occur among the natives of the whole northern sea-board of Egypt; but the inhabitants of Middle and Upper Egypt, as far as I could learn, are entirely exempted from that fatal disease. The prevailing maladies throughout all Egypt are dysentery and ophthalmia—both induced, it is believed, by exposure of the heated frame to currents of cold air.

These observations on the subject of Egyptian climate would be very incomplete if I failed to notice its influence in arresting hæmoptysis. Several instances of its efficacy in that respect were mentioned to me, and one very decided case fell under my own observation. A. B., a middle-aged gentleman, of a clear and florid complexion, had been for years afflicted with this complaint to an alarming extent. He had spent a winter in Italy without experiencing the smallest mitigation of his ailment. He had next been sent to Madeira, and there the malady was very greatly aggravated. He was at Malta when I went to Egypt; but finding no relief there, he came to Cairo in the end of December, and took up his abode at the hotel at which I lodged. The effect of the change of climate was immediate. The spitting of blood ceased at once, nor did it ever recur during his stay at Cairo, which was prolonged till the end of April. He then went to the South of Spain, and remained there till the month of June, when he returned to England, apparently in perfect health, and fully resolved to spend the whole of the following winter in Egypt, with the view of confirming his cure.

The transition from the climate of Egypt to that of Britain is too violent to be hazarded by one whose respiratory organs are in a delicate or dubious condition, especially as the season at which it is necessary to quit the one country is far from being genial in the other. I therefore stopped in Spain on my homeward passage, and spent the latter part of May and the greater part of June in that country and in Portugal.

Having heard that the air of Malaga was remarkable for its mildness, I repaired thither, and was both delighted and surprised to find in Europe a climate scarcely inferior in any respect to that of the latitude of Cairo. The register I kept while there, and from which I subjoin an extract, shows a temperature which was probably only a few degrees lower than that of Cairo at the same date, while its equability was greater than I had noted anywhere except at Alexandria; and what is still more remarkable, the dryness of its atmosphere exceeds that of Cairo, and contrasts surprisingly with the humidity with which the air of Alexandria is loaded.

Malaga.

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
	°	°	°	°	—	
May 20.	70	72	2	9	—	Bright sunshine.
„ 21.	70	73	3	12	—	Do.
„ 22.	71	73	2	12	—	Do.
„ 23.	69	72	3	11	—	Rain.
„ 24.	67	70	3	10	—	Do.
„ 25.	66	69	3	10	—	Do.
„ 26.	68	70	2	14	—	Bright sunshine.
„ 27.	69	72	3	16	—	Do.
„ 28.	69	73	4	15	—	Do.
„ 29.	69	73	4	15	—	Do.
„ 30.	69	72	3	16	—	Do.

The atmospheric conditions indicated by this table are doubtless to be referred, partly to the geological structure and the physical conformation of the country around Malaga, and partly to the latitude in which it is situated. I am persuaded that, in a therapeutic point of view, the climate in this part of Andalusia is deserving of more attention than it seems hitherto to have received. Those who take it for granted that the climate of Italy must be the mildest and warmest in Europe, seem to forget that Malaga is 248 miles further south than Naples, 318 miles further south than Rome, and 518 miles further south than Venice; and those who have not adverted to the fact, will propably be surprised to find that it is five miles further south than Algiers. Both Gibraltar and Cadiz are somewhat south of Malaga, but both have a great diurnal range of temperature, and are nearly equal to Alexandria in the humidity of their atmosphere; the one standing on an almost insulated rock in the Mediterranean Sea, and the other on a narrow spur of land projecting into the Atlantic Ocean.

In the beginning of June I moved northward to Lisbon; but on its climate, and that of delightful Cintra, no information is needed. In the end of June, 1853, I returned to Scotland; and though I was threatened soon after I reached home with a recurrence of bronchitic symptoms, these *now* yielded readily to medical treatment. I am most thankful to be able to add, that during the last twelvemonth I have enjoyed excellent health, nor have I been affected, during that time, by any of the changes of weather to which our variable climate is subject.—*Edinburgh Medical and Surgical Journal, Oct.* 1854, *p.* 656.

126.—OCCURRENCE OF DELIRIUM IN ACUTE RHEUMATISM.

By Dr. C. M. Durrant, Physician to the East Suffolk and Norwich Hospital.

The profession is deeply indebted to Dr. Watson for having insisted upon the importance of attending to this symptom, as an indication of the probable existence of inflammation of the pericardium.

Delirium occurring during the progress of acute rheumatism is always of grave import, and calls for immediate and frequently repeated examination of the condition of the heart. It is a symptom which is very likely to mislead the practitioner; and if the treatment be directed solely to the mitigation of the cerebral disturbance, the pericarditis, upon which the delirium depends, may silently advance, and ultimately destroy the patient.

In the fatal cases which have been reported of this complication, and in which an opportunity of examining the condition of the brain and heart after death has been afforded, the former, in many instances, has been found free from disease, while the latter has shown unequivocal results of recent but unsuspected inflammation. These cases of apparent inflammation of the brain, or its meninges, were formerly attributed to metastasis; the more probable cause is now considered to be uræmia, or it may be direct reflex action, excited by the pericardial inflammation. The latter is more especially the cause of the convulsive movements of the limbs, which sometimes take place under the circumstances which we are now considering.

The delirium may be active and violent, but it is more generally characterised by muttering and taciturnity. The pulse is usually very much accelerated. In the following case, the delirium occurred during the progress of the pericarditis; but the symptoms from the first were so severe, and the extension of the inflammation to both pleuræ so fully marked, that its brief detail may not be out of place.

Case.—A. M., aged 50, after having been out of health, was seized with acute rheumatism, to which, in a few days, was superadded pericarditis.

On examination, his countenance was anxious; the breathing was quick and catching; and he complained of considerable pain and oppression at the region of the heart. The tongue was coated with the white rheumatic fur; he had profuse sour perspiration; his bowels were regular; the urine scanty, with dense deposit of purpurates and lithates.

On applying the stethoscope, a friction sound was heard over the entire cardiac region, so loud as to mask considerably the sounds of the heart itself. The patient, prior to my visit, had been twice largely bled. Local depletion by leeches to the

cardiac region, with calomel and opium, in addition to salines with colchicum, were prescribed.

On the following day, the friction sound was less intense, but the patient now complained of pain beneath the right breast, where a friction sound was also audible. A blister was now applied to the region of the heart, and the mercury, &c., continued. On subsequent examination, the friction sound had disappeared, but the heart's sound had become distant; while the cardiac dulness had considerably increased, extending upwards towards the left clavicle. Signs of effusion also distinctly obtained in both pleural cavities. The mouth at this period was very sore from the mercury.

The subsequent treatment for some days consisted of repeated large blisters both in front and behind, with saline diuretics, supporting the strength with strong broths, and the occasional exhibition of a stimulant in the shape of wine.

The condition of this patient for more than a week was a most critical one; at this period delirium supervened, with constant talking and muttering, and occasional attempts to get out of bed. The evacuations and urine were now passed involuntarily. The pupils contracted naturally, and but slight heat of the scalp existed; the pulse was much accelerated. The mouth was still sore, and the absorbents acted slowly. The pericardial distension was still very considerable. The blistered surfaces were kept discharging freely, and a blister was also applied to the nape of the neck; while the quantity of wine allowed was increased. The delirium continued more or less for a fortnight, when the symptoms gradually abated; and, although I have not had an opportunity of ascertaining the precise condition of the heart and lungs after so formidable an attack, still the reports both from his medical attendant and friends have been satisfactory, inasmuch as he has been able to leave home and attend to business.

Remarks.—Dr. Stokes has rendered good service by recommending a more liberal allowance of wine in the latter stages of the treatment of, and during the convalescence from pericarditis. Attention to this advice will be more particularly called for, if venesection have been freely had recourse to at the onset of the attack. The nature of the disease, after the violence of the primary fever has been subdued, is essentially a depressant of the nervous power, more particularly if the brain be oppressed by uræmic poisoning.

Very many cases would, I believe, under the present asthenic character assumed by disease, have a better prospect of recovery, were the injunctions of Dr. Stokes in reference to the earlier exhibition of wine more generally attended to; and many a convalescence, I cannot but think, would be materially shortened were the lancet used still more sparingly, and when it is absolutely necessary that the quantity of blood be withdrawn, with a prospective regard to the ultimately depressing nature of the disease.—*Association Jour.*, *Oct.* 6, 1854, *p.* 903.

127.—ON CITRIC ACID.

By Dr. H. Bence Jones, F.R.S. Physician to St. George's Hospital.

The first point which immediately presents itself is the identity or difference between citric acid and lemon-juice. What other substances does the lemon-juice contain in addition to citric acid? Do any of the many substances which exist in lemon-juice so modify the action of the citric acid which it contains, that the medicinal effect of these two remedies cannot be considered as identical? The first question was a chemical one. Can lemon-juice be considered as a solution of citric acid in water, or is it an acid citrate of potassa, or does it contain some other active substances in solution?

At my request, Dr. Hoffmann undertook an analysis of the ash of lemon-juice, and this was made by Mr. Whytt, of the College of Chemistry. Some experiments which I have made on the degrees of acidity of fresh lemon-juice I shall also mention here.

The lemon-juice was obtained from fresh lemons. It contained no trace of sugar: it had specific gravity of 1036·0, 1037·9, and 1038·4. The acidity varied in different specimens. Thus—

1000 grains of lemon-juice required in one experiment a solution of caustic soda

containing 30·4 grains of soda. In a second experiment 1000 grains of lemon-juice required 29·4 grains of soda.

These quantities of caustic soda are equivalent to 62·2 and 60·8 grains of pure anhydrous citric acid. Hence each ounce of the lemon-juice examined contained about 28·1 to 27·5 grains of pure anhydrous citric acid.

1000 grains of the lemon-juice, when evaporated to dryness, and burnt, left 3·60 grains of ash. Hence an ounce of lemon-juice contains only 1·64 grains of ash.

[Dr. Jones then gives a table showing Mr. Whytt's analysis of the ash. He then says:—]

As it appears from these experiments that an ounce of lemon-juice contains 27 to 28 grains of citric acid, and only three-quarters of a grain of potash, which, at the very utmost, can only combine with one grain of citric acid, it follows that an ounce of lemon-juice does not contain two grains of citrate of potash, and for all practical purposes we may consider it as a solution of free citric acid, every ounce containing about 26 or 27 grains of free citric acid. With regard to the difference of the medicinal action of lemon-juice and citric acid, I instituted some experiments on the action of lemon-juice in increasing the acidity of the urine. The question I proposed to myself was to determine whether lemon-juice acted like a free acid and increased the acidity of the urine, and to what extent it did so; whether it caused the formation of free urea and crystals in the urine on standing.

The urine of a person in very good health was examined each time it was passed for twenty-four hours by taking a fixed quantity, and exactly neutralizing it by a solution of carbonate of soda containing for each degree the twelfth of a grain of carbonate of soda.

[After another long and interesting table, Dr. Jones proceeds to say:]

From these experiments it is perfectly evident that lemon-juice in twelve-ounce doses daily does temporarily cause an excessive acidity of the urine, and gives a deposit of free uric acid in the urine. Lemon-juice acts, then, as free acid on the urine, and does not at all resemble in its effects those produced by citrate of potassa and other vegetable acid salts.

Regarding the diuretic properties of lemon-juice, the quantity of urine passed each day during the four days shows that when taken pure, without water, by a healthy person, no strong or immediate diuretic action is observable.

The pulse also presented nothing remarkable in force or frequency when the lemon-juice was taken.

The results, then, of those experiments, and the knowledge which is obtained by chemical analysis, show that lemon-juice may be considered as a solution of citric acid. You will remember that citric acid is a poison having an action similar to that of oxalic acid. In half-drachm doses it killed a rabbit in twenty minutes, causing loss of sensation and muscular action, and stopping the action of the heart. On examination after death no inflammatory action was observed. The action of lemon-juice is similar, but less in degree, to that of oxalic acid, and other vegetable acids.

From the testimony of observers like Dr. Rees and Dr. Babington, there can be no doubt that its anti-febrile sedative action may be obtained in disease. In acute rheumatism and gout it is by extended observation certainly no specific. It does not even reduce fever so surely and quickly as many other remedies: but it is worthy of attention, that in opposition to the theory of excessive acidity as the cause of these diseases, it certainly does not aggravate the symptoms as might have been expected.

The account published in the *Révue Med. Chir. de Paris*, August, 1853, of cases of acute rheumatism, subjected only to the expectant treatment, shows how easily recovery may be attributed in acute rheumatism to the last remedy employed. Still, though lemon-juice is no specific for rheumatic inflammation, yet its anti-febrile action may occasionally be thought preferable to that of nitre, or many other saline medicines.

It ought never to be forgotten that it acts like a free acid on the urine, setting free uric acid cystals. In St. George's Hospital, I have found free uric acid in the urine twelve hours after it has been passed, in every case in which large doses of the lemon-juice were given.

Hence, in chronic rheumatism and gout, the remedy should be used with care, lest

red sand or uric acid calculus should be produced. A pint (20 ℥) and more of lemon-juice in twenty-four hours cannot be given for three, or four, or more weeks continuously, without risk of the formation of gravel.

In conclusion, I must again refer to the action of oxalic acid as being the type of the action of the vegetable acids on the system. It is worthy of remark, that the mineral acids differ altogether from this type. The poisonous action of the mineral acids differs in kind from the sedative action of oxalic acid, although they agree in the corrosive action which they possess.

If you inquire why does oxalic acid act as a poison depressing the system, no positive answer can yet be given. There is, however, a striking resemblance in the action of some other compounds of carbon, which I will venture for a moment to bring before you.

Carbonic acid CO_2 is a narcotic poison.

Oxalic acid C_2O_3 is a stronger poison.

Carbonic oxide CO is the most powerful of these three closely related substances.

The less oxygen the more poisonous the substance. Hence the activity of the substance would appear to depend on the carbon rather than on the oxygen. Some other simple compounds of carbon, free from oxygen, also possess poisonous properties.—*Medical Times and Gazette, Oct.* 21, 1854, *p.* 408.

128.—*On the Effects of Liquor Potassæ.* By Dr. E. C. Parkes.—It may be foretold of liquor potassæ, that if taken unneutralized into the blood (in cases in which no violent disturbance is going on in the body) it will cause an increased formation of sulphuric acid in all cases, of phosphoric acid in a minority, and of some other acid (probably an organic) now and then. It will increase also the organic constituents of the urine, especially the urea. Its main effect is, then, to hasten the metamorphosis of some of the albuminous structures of the body, but it may also act on the saccharine and fatty constituents of the body, though no evidence of this is given by the examination of the urine. Its utility in disease is evidently connected with its destructive and eliminating powers. Whether it is more powerful than other alkaline medicines in hastening the metamorphosis of tissue, can only be told by future experiment.—*British and Foreign Med. Chir. Review, Oct.* 1854, *p.* 506.

129.—ON A NEW METHOD OF URETHROTOMY FOR THE CURE OF STRICTURE.

By M. Maisonneuve, Surgeon to the Hôpital Cochin.

Until the very recent period when Mr. Syme, of Edinburgh, and M. Reybard, of Lyons, conceived the happy idea of employing free incisions in the treatment of strictures of the urethra, the radical cure of these affections was considered to be nearly impossible.

Cauterization, on which great hopes had been placed, had not fulfilled the expectations formed respecting it. Scarification, which was, however, considered rather as an adjuvant of dilatation than as a curative method, was attended with only transitory success. Finally, dilatation itself, notwithstanding the many improvements introduced into the modes of effecting it, most frequently succeeded only in palliating the evil, without removing its organic cause.

The treatment of strictures had consequently, in spite of the constant efforts of surgery, made scarcely any progress.

The system of employing free incisions is the only one which includes an idea really containing the elements of success, and which appears to attain the end so long sought for.

We can no longer, in fact, call in question the possibility of procuring a permanent enlargement of the urethra, by means of deep incisions made in the indurated tissue of which strictures are formed. The important works of MM. Reybard and Civiale

have incontestably established its truth, and the clinical observations which demonstrate it are now numerous enough to admit of every one having witnessed and verified them.

It is then a fact now secured to science that a deep incision, longitudinally made in the canal of the urethra, gives rise to a depressed cicatrix, and that this forms a sort of permanent trench, the width of which augments by so much the dimensions of the canal.

But if this fundamental principle of urethrotomy has received the assent of almost all surgeons, this is by no means the case with the methods and operative proceedings intended to realize its application.

These methods are three in number.

One, called Syme's method, urethrotomy from without inwards, consists in dividing successively with a bistoury the skin, the areolar tissue, and the walls of the urethra, corresponding to the stricture.

The second method, conceived by various surgeons, but applied rather to scarifications than to incisions, is called urethrotomy from before backwards, and is performed with instruments of different forms, the point of which, introduced into the opening of the stricture, serves as conductor to a cutting blade, which springs out in front of the obstacle, and is then pushed against it to divide it.

The third plan, invented by M. Reybard, under whose name it is known, consists in dividing from behind forwards the entire thickness of the urethral walls corresponding to the strictured part, by means of an instrument the blade of which, at first concealed, opens when it has passed the obstacle. This blade, thus opened at an obtuse angle, is drawn from behind forward, against the stricture, which it cuts through the entire thickness of the wall of the canal.

Each of these methods certainly fulfils its principal object, namely, the division of the strictured points; but their operative manipulation is usually so delicate and complex, and, above all, the accidents they may involve are so numerous and so serious, that the most experienced practitioners hesitate to adopt them.

The new method I have the honour of submitting to the Academy has the advantage of leading to the same results as the preceding, while it is attended with scarcely any of their inconveniences, and its execution especially is so simple, that it may, in this respect, be reckoned in the category of the most ordinary operations.

This method, which I shall designate by the name of urethrotomy from within outwards, consists in dividing the strictured walls by means of a concealed bistoury, the blade of which, contained in a sheath, is introduced into the opening of the obstacle, where, by a most simple mechanism, it becomes disengaged, presses from within outwards on the stricture, which it tends to dilate, and then on the least traction cuts the affected parts to the depth previously determined on.

Instrument.—The instrument of which I make use is not a new instrument. It is known to all practitioners; its mechanism is familiar to them: it is simply the lithotome of frère Côme. To convert it into a urethrotome, I have merely made its blade a little longer and bent on the flat, given its sheath a cylindrical form, especially at its extremity; marked on it the divisions of the mètre, and adopted an accurate regulator, so as to arrange its articulation in such a manner that when required the opening of the blade may be equal to its entire extent.

Description of the Operation.—Preliminary Precautions.—Before performing urethrotomy by my method, the surgeon should gradually bring the urethra to a degree of dilatation sufficient to admit of the urethrotome being easily introduced. He ought especially to accustom the canal to the contact of instruments, in order to avoid the febrile attacks which in this region so frequently complicate even the most simple operations. To this end the patient should, during at least a fortnight, be subjected to progressive dilatation by means of elastic bougies. He should also be kept on light diet, have frequent baths, emollient lavements, and cooling drinks.

This preliminary treatment is not, however, intended merely to accustom the canal to the contact of instruments, and to obtain the amount of dilatation necessary for the introduction of the urethrotome: it is also useful by enabling the surgeon to introduce exploring instruments, and so to ascertain exactly the number of the strictures, their position, extent, degree of resistance, &c.

All these objects having been accomplished, the operation may be proceeded with.

The patient should be placed upon his back, and kept in that position by assist-

ants. It is also advisable that he should be brought under the influence of chloroform. The surgeon, situated at his right side, again explores the urethra with a bougie (à boule); then, seizing the urethrotome, the degree of opening of which has been previously fixed at about 15 millimetres (0·59055 inch), he introduces it as he would an ordinary catheter to three centimetres (1·1811 inches) beyond the last stricture, and arranges it so that its concavity shall look upwards. Then, holding the penis on the instrument, he opens the latter by pressing on the lever, and by a movement of traction makes it traverse a space of two centimetres in extent (·7874 inch).

This movement is sufficient to cause the cutting blade to divide all the obstacles which opposed its expansion, without, however, involving the intervening sound parts. The pressure on the lever being then removed, the blade returns into its sheath, and the urethrotome is gently withdrawn.

Mode of Action of the Urethrotome on Strictures.—We have seen that at the moment the surgeon, in the manœuvre I have described, presses on the lever, the blade of the urethrotome driven out of its sheath, at once encounters the obstacle which opposes its separation: then, as soon as a slight degree of traction is given to the instrument, its edge divides the indurated portion without previously injuring the neighbouring sound parts.

This property, possessed by my urethrotome, of not being able to reach the sound tissues until after it has divided the strictured portions, is one of the essential characters of the method, and is not one of its least advantages. It is evident how much security it gives both the practitioner and the patient.

It is particularly in cases in which numerous strictures exist that this property is really valuable. When in fact we have a series of strictures separated by intervals of different lengths, urethrotomy by the ordinary plans is an operation so formidable, that its possibility is scarcely conceivable.

In Syme's method, for example, the surgeon would be obliged, in order to divide all obstacles, to slit the urethra in almost its entire length, and of this its author was so conscious that he confines his operation to those cases in which there is but a single stricture, and where the latter occupies the perineal or membraneous portion of the urethra.

The adoption of M. Reybard's mode is not, under the circumstances I have described, productive of much more advantage; for the blade of the instrument having to open behind each stricture, and having necessarily to divide a considerable portion of the sound tissue behind and before the obstacle, it frequently happens that all these incisions united constitute an enormous amount of mutilation, while, in addition, their execution requires a series of long and difficult manœuvres to open and close the instrument.

In my method, on the contrary, whatever be the number and position of the strictures, the surgeon is assured that with a single stroke all that opposes the evolution of the cutting blade shall be divided, while the sound parts cannot possibly be seriously compromised. By it we get rid of the complicated manœuvres and enormous incisions which have justly alarmed practitioners, and have made them shrink from urethrotomy.

Immediately after the incision, the surgeon introduces into the canal a large metallic bougie, five or six millimetres (·19685 or ·23622 inch) in diameter, in order to ascertain that all the obstacles are completely divided. He afterwards replaces this bougie by an elastic catheter of the same size, which is allowed to remain for, at least, twenty-four hours, and, at most, three days. The object of this is to spare the patient the acute pain caused by the urine in passing over the wound, as well as keep the lips of the incision apart, and to stop the flow of blood.

It is afterwards necessary to continue for about six weeks the daily introduction of a large metallic bougie, to maintain the dilatation of the canal.

Accidents consecutive to the operation.—In general the consequences of urethrotomy, performed according to my method, are very simple.

The hemorrhage stops in a few hours. The pain produced by the passage of the urine ceases after the eighth or tenth day, and the suppuration dries up towards the end of the sixth week. But, under some circumstances, we observe accidents of another kind.

1. *Engorgement of the Testicle.*—This scarcely ever occurs except about the

eighth day. It is of little consequence, and disappears under the influence of rest and the use of emollient cataplasms. Once, however, I have seen it assume the appearance of a very acute orchitis, which required the employment of incisions and the repeated application of leeches.

2. *Abscess in the Perineum.*—In the first trials I made of my method, when I performed the incision directly downwards in the median line, I had two cases of urinary abscess, followed by numerous fistulas in the perineum and the scrotum; but since I have adopted the lateral incision, nothing of the kind has occurred.

3. *Urethral Fever.*—Every practitioner knows how frequently catheterism is followed by febrile attacks of the remittent and intermittent types. These affections, sometimes very serious, may appear after urethrotomy, as after any operation performed on the urinary passages. Those I have hitherto seen have not been of any importance; they have merely consisted in a few transient rigors. But the surgeon should not forget that this is one of the most formidable dangers which attend the operation we have been considering; and he ought to take every care to prevent it. Frequent baths, mucilaginous drinks, rest, and, above all, extreme prudence in the use of the catheterism employed to procure dilatation, are the best means of doing so.—*Bulletin Général de Thérapeutique*, vol. xlvi., February, 1854, p. 169.—*Dublin Quarterly Journal of Medicine, Nov.* 1854, *p.* 472.

130.—*On Spongy Bougies.*—M. Alquie has constructed bougies of the following kind: Round a whalebone bougie is rolled a sponge; this is reduced by compression to a third of its volume; it is then covered with gold-beater's skin, and is covered with the salve used in ordinary bougies. The instrument when about to be used is dipped in water, and then, when its point has been touched with cerate, is introduced into the urethra. The sponge slowly imbibes the water, and swells so as to distend the urethra, supposing this to be the canal operated upon. Whether there is then any difficulty in withdrawing the instrument, or any danger of injuring the urethra in so doing, does not appear.

Ricord has used this instrument in two cases of stricture; it was introduced easily, but could not be borne more than an hour; it was withdrawn without difficulty, as the swelling of the sponge was not great.

M. Alquie points out various other possible applications of the sponge-bougies. He alludes to a case in which the fractured nasal bones were held in place easily by its means.—*British and Foreign Med.-Chir. Review, Oct.* 1854, *p.* 558.

131.—*Sore Nipples.*—M. Bourdel recommends the application of a piece of lint dipped in the tincture of benzoin placed over the part, then removed, wetted with the tincture, and replaced, so as to cover the ulcer with a layer of liquid. The first application is painful, but the pain seldom lasts more than fifteen minutes; the tincture forms a coating, which the action of sucking does not displace.—*Ibid, Oct.* 1854, *p.* 563.

132.—*Prurigo.*—Dr. Richart recommends in prurigo ani et vaginæ, which are often most obstinate complaints, the following "specific" treatment: Take equal parts of sulphate of zinc and of alum, roughly powder them, put them into a glazed earthenware vessel; put it on a slow fire, and leave it there till bubbles of air are no longer disengaged, and till the mixture acquires a stony hardness: then powder it, and throw it by small portions at a time into boiling water then filter, and apply to the parts with a sponge and on linen.—*Ibid, Oct.* 1854, *p.* 564.

133.—ON THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

By T. Spencer Wells, Esq.

[Professor Sigmond of Vienna, Rothmund of Munich, and Wützer of Bonn, have all succeeded by the following method in curing reducible hernia. Mr. Wells has himself operated with apparent success in two cases, adopting the plan of Wützer, which he described as follows.]

It consists of a wooden cylinder, a needle passing through it, an outer wooden case, and a screw to bind the case and cylinder together. After pushing a part of the skin of the scrotum before the forefinger, the cylinder takes the place of the finger, the needle is passed through it (through the sac and integument), and serves, with the screw, to fix and bind the cover and cylinder together, so that the invaginated skin, the walls of the sac, and the abdominal integuments, are pressed together with any degree of force that may appear safe and advisable. The instrument is left *in situ* from six to eight days, with the effect of producing adhesion of the whole of the sac, a firm, organized plug filling the inguinal canal. Very little pain is said to be produced. The patient is confined to a sofa until the needle puncture is healed, and he wears a truss for three months afterwards, to avoid the danger of the breaking up of new adhesions. Mr. Wells believes that there are grounds for admitting this operation within the province of legitimate surgery, and that the classes of cases in which it should be performed are—1. In strong persons, up to forty or forty-five years of age, when the hernia is of moderate size; in such persons a perfect cure may be expected. 2. In cases where the hernial protrusion is so large that it cannot be kept up by any mechanical contrivance. Here a radical cure is not to be hoped for, but the evil is so much diminished that a truss can be made to keep up the hernia effectually. He concluded by expressing his belief that, as by this new method the whole of the inguinal canal can be permanently closed, it is far superior to all others, and that the evidence he had adduced of its safety and success should lead to its further adoption in this country.—*Lancet, May* 20, 1854, *p.* 542.

134.—NEW PROCEEDING FOR THE CURE OF HERNIA.

Doubts were expressed in a former number of the 'Gazette Hebdomadaire,' as to the efficacy of a proceeding lately applied by Professor Jobert to the radical cure of inguinal hernia. Perhaps it should be said "re-applied," for the proceeding belongs to M. Velpeau; and it may be as well to observe, that this surgeon, instead of puncturing the sac blindly (*à l'aveuglette*) across the skin, exposes it by a previous incision, to be sure of making the injection penetrate; and that yet he has obtained only a moderately satisfactory result.—(See *Médicine Opératoire*, t. iv., p. 150.)

Now, the application of iodine will not at best effect more than the obliteration of the hernial sac; it will not extend to the inguinal canal; and the obliteration of the canal is an essential and indispensable condition to a radical cure. In effect, an obliterated sac can form a plug for some time to the external orifice of the canal; but, supposing that the hernia is old, and has acquired a right of domicile, it will easily drive down this feeble barrier, pushing the peritoneum before it. Supposing even that the walls of the sac rest, solid and firmly applied one to another, even in the interior of the canal, there is nothing to oppose the reprotrusion of the peritoneum by the viscera. In a word, the suppression of the hernial sac, in whatever manner it may be obtained, by ligature, by suture, by the introduction of filaments of gelatine, of strips of goldbeater's skin, by iodine injections, &c., cannot pretend to do more than neutralise, to a greater or less extent, the actual effects of the descent of the peritoneum; it is incapable of preventing a new protrusion. The success may be complete, provided the viscera exert no new efforts. Under the opposite condition, it will be only temporary. Thus it will be seen that no conclusions as to the result of the proceeding can be formed until after a considerable time.

What is really required, is either adhesion between the anterior or posterior wall

of the inguinal canal, when the sac will disappear by the fusion of these parts, or the introduction of a plug, which will not escape, into the canal. And, moreover, it is essential that the obliteration should extend to the level of the internal ring, without which, an interstitial hernia might form. M. Gerdy has the honour of proposing a plan by which these conditions are fulfilled. The proceeding consists in invaginating the skin of the scrotum in the inguinal canal as far as possible: in fixing the end of this inflected portion (*doigt de gond*) to the skin of the abdomen by a pegged suture; in cauterising with ammonia the internal surface of the cutaneous plug, to excite mutual adhesion of its walls, and to render it solid. Unfortunately this plug, which by its external surface has no means of attachment save to the subcutaneous cellular tissue, terminates, in many cases, in yielding before pressure of the intestines, and the hernia is re-produced more or less completely.

A completion of this operation has recently been conceived by M. Valette, a gentleman whose name is becoming extensively known and respected in the scientific world. He proposes to give fixed attachment to the invaginated skin, by making it adhere to the neighbouring parts above and in front, without prejudice to the obliteration of the infundibulum.

His surgical apparatus is composed of an invaginator,—a sort of plug in ebony or boxwood, about four inches long, rounded at the distal extremity, and furnished at the proximal with a metallic ring. This ring supports a handle turned so as to constitute a screw, which will receive two nuts (female screws) capable of being separated or approximated after being fixed upon the handle. The invaginator is bored by a curved canal, the entrance of which is the centre of its proximal extremity; the opening three-quarters of an inch from the distal extremity. A large curved needle can easily slide through the canal of the invaginator. An inch from the point of the needle is an opening or window, destined to receive a small quantity of the paste of chloride of zinc. (The chloride of zinc plays a great part in the surgery of Lyons.) At each extremity of the window, the needle presents a small opening to transmit a thread, by means of which the small bandalette of caustic is fixed in its box. As for the crosspiece which separates the opening into two, it is simply of steel, to give firmness to the fenestrated portion. A waistband of leather is necessary to maintain the invaginator fixed and immovable. On the anterior part of this band is a plate of steel, presenting an aperture for the passage of the long perpendicular handle fixed to the free extremity of the invaginator. Articulations permit to this plate movements to the right or left, torsion on itself, &c.; and female screws are prepared to maintain it in any position the surgeon may choose. The details of the operation are as follows:—

The skin having been pushed back into the canal by the index finger, a gorget is first introduced, then the invaginator is pushed along the groove of the gorget. The surgeon must be careful not to place the invaginator too obliquely, lest the external extremity press the urethra, and interfere with the escape of the urine. An assistant seizes the instrument, and holds it in place. The inferior screw of the handle of the invaginator is abandoned; the plate of the waistband is properly adjusted, and directed by means of its articulations, placed both at its attachment to the band, and about the middle of its length, in such a way as not to interfere with the handle of the invaginator; finally it is secured by proper screws. The superior screw of the handle is then applied, so that the instrument becomes immovable. Two threads may, for additional security, pass from the instrument to buttons fixed on the waistband. This having been done, the needle is thrust forward, which, having traversed the skin inflected by the invaginator, appears externally. It is made to pass completely through the soft parts, upon which the caustic acts, and the operation is complete. Should the cauterisation be sufficiently extensive (and the effect is usually obtained in ten or twelve hours) the needle is thrust further forward, so that the aperture for the caustic becomes quite disengaged. An eschar forms across the track of the needle. Should the cauterisation be insufficient, the needle must be charged again. Inflammation ensues about the disorganised parts, and adhesions form of considerable strength. When the effect is obtained, the instrument is easily removed. First, the needle is withdrawn, then the invaginator. The extremity of the infundibulum may be, lastly, excoriated, if the instrument have not already done so.

M. Valette has performed this operation eight times. He publishes only two

cases in detail. In one the patient suffered from two large inguinal herniæ since birth. He was discharged cured five months after the first operation, and three months after the second. A year afterwards he was examined and found well. The second case was that of a man who had an inguinal hernia as large as two fists. He went from the hospital three months after the operation, but wrote four months after his discharge to say that the cure was perfect.

M. Valette gives the results only of the five remaining operations. All the herniæ were large; four times the cure was complete. In one a relapse was becoming apparent a month after his discharge from the hospital.

In no instance were there any serious complications; there was no peritonitis, even in the slightest degree. We hope for further experience to test the value of an operation so important as one for the radical cure of hernia.—*Gazette Hebdomadaire*, Aug. 24, 1854.—*Med. Times and Gazette*, *Oct.* 7, 1854, *p.* 376.

135.—*On the Use of Ice combined with Compression in the Reduction of Hernia.*—In sixteen cases of strangulated hernia, in which all the ordinary means of reduction had been unsuccessfully employed, M. Baudens has effected the return of the bowel by the application of ice associated with permanent local pressure. The degree of refrigeration is to be regulated in proportion to the amount of inflammation in the hernial tumour, and the sensations of the patient. M. Baudens commences by the application of a simple compress, which is soaked from time to time in water, whose coldness is gradually increased. Fragments of ice are afterwards placed on the compress, and the cold thus produced may be augmented, if necessary, to a very considerable intensity by the addition of increasing quantities of common salt. Sometimes the refrigeration alone is sufficient to effect the reduction, as M. Baudens has witnessed on three occasions; but when this does not take place, an elastic bandage is applied over the ice, by means of which a continual steady pressure is exercised on the tumour. Whenever the rupture is so painful that no pressure can be tolerated, ice alone must be employed at the outset, and the elastic bandage may be added as soon as the sensibility of the swelling is sufficiently diminished. In order to facilitate the action of these measures, the pelvis should be kept in an elevated position.

M. Baudens ascribes the efficacy of this treatment to the powerful influence of cold in diminishing the size of the hernial tumour, by overcoming the capillary congestion, and extinguishing the inflammation in the strangulated bowel. Hitherto, says he, the treatment by cold has been condemned, because its therapeutical effects have been neglected or misappreciated. He regards the idea that the application of cold to a strangulated rupture is likely to produce mortification in the bowel as a groundless apprehension, believing that so large an amount of heat is developed in a part during the continuance of inflammation, as to enable it to resist with facility long and powerful refrigeration, without at all endangering its vitality. He admits, however, that it is quite possible to continue the application of cold too long, but maintains that the feelings of the patient furnish the best index as to the advantageous or prejudicial operation of the remedy. As long as the patient feels that the action of the refrigerant is grateful, and productive of comfort, its application should be continued; but, as soon as he begins to experience a disagreeable sensation of coldness and moisture it should be immediately removed.—*Gaz. des Hôpitaux.*—*Med. Times and Gazette*, *Sept.* 30, 1854, *p.* 349.

136.—MODIFICATION OF SYME'S AMPUTATION.

[Professor Pirozoff of St. Petersburg, has attempted an improvement on Mr. Syme's operation of exarticulation at the ankle-joint.

A correspondent of the 'Medical Times and Gazette' writes from Dantzig that this improvement has been adopted at the Dantzig Hospital. He says:]

The point in which Pirozoff's operation differs from that of Mr. Syme, is the preservation of the posterior portion of the os calcis, which is left to fill up the heel-

flap. The proceeding is obvious, and easy of execution. The incisions are made exactly as directed by Mr. Syme; after practising the first from one malleolus across to the other, the operator need only prepare the integuments about a line or two backwards from the inferior surface of the os calcis; he then proceeds to the execution of the second incision across the front of the joint; the astragalus is then exarticulated, and the os calcis divided by the saw behind the posterior extremity of the astragalus. The posterior extremity of the os calcis is preserved by this proceeding, instead of the whole being peeled out, as in Mr. Syme's operation; the heel-flap is thus completely filled out by the bone. The saw is then applied to separate the malleoli, and a thin slice of the articular surface of the tibia; the cut surface of the last-named bone is then brought into apposition with the cut surface of the os calcis, and the skin-flaps accurately united by sutures. The advantages of this operation are obvious:—1st. We obtain a longer stump than by Mr. Syme's operation, and the extremity of the stump is firmer, and far better adapted to bear pressure with impunity. 2ndly. The healing process takes place quicker, and is less likely to be retarded and disturbed, as after Mr. Syme's operation, the cavity of the heel-flap being filled up by the bone. 3rdly. There is no danger of the heel-flap being deprived of the necessary supply of blood, as it will be easy to avoid dividing the posterior tibial artery high up in the wound, as often happens during the execution of Mr. Syme's operation, even by the most skilful hand.

I am bound to add here, that I have seen a case of Syme's operation, in Langenbeck's Hospital, recover remarkably well, where the posterior tibial artery was divided immediately below the malleolus internus; whereas, in another case that came in shortly afterwards, where the artery was preserved during the operation in its whole length, extensive sloughing of the flap supervened.

Of course, I do not mean to imply that the preservation of the arterial branch was the cause of the accident; and am of opinion, as every man of clear sense must be, that the preservation of the artery must be aimed at as much as possible.

Pirozoff's operation is easier of execution than that of Mr. Syme. This is a point I do not particularly urge, as, by diligent practice on the subject, anybody who is able to handle a knife can acquire great dexterity in performing Mr. Syme's operation. A button-hole, or a "window," as we call it here, will certainly now and then make its appearance, just when you are priding yourself upon performing the operation with the utmost elegance: it is a consolation that the hole injures the pride of the operator more than the patient. Of course, this operation will not supersede Mr. Syme's proceeding, as a perfectly healthy state of the os calcis, in its posterior part, forms a *conditio sine quâ non;* but I think it well deserves the attention of the profession in cases adapted to its execution.—*Medical Times and Gazette, May* 6, 1854, *p.* 468.

137.—ON MORBID STATES OF THE URINE CONNECTED WITH INDIGESTION.

By Dr. H. Bence Jones, F.R.S., Physician to St. George's Hospital.

[It has long been known what intimate connection exists between indigestion and morbid states of the urine, and in numerous articles in this 'Retrospect' we have pointed it out in the writings of our most eminent modern physicians. On a former occasion Dr. Bence Jones pointed out the connection between the stomach and acidity and alkalescence of the urine. In the following article we give his views of indigestion, when the urine is comparatively neutral.

Sometimes digestion may appear to be good, and yet myriads of microscopical octohedral crystals may be seen: and even with the naked eye a cloud of these crystals may often be observed. In other cases severe dyspepsia may exist with urine of a neutral character. If this urine, however, be kept for six or twelve hours, and a little of the lower urine from the bottom of the vessel be examined by the microscope, innumerable octohedral crystals of oxalate of lime will be seen. Whatever depresses the general health disposes to this form of dyspepsia. Now, what is the treatment of these cases? Dr. Jones says:]

The best rule which I can give is this: take the oxalate of lime only as one symptom of indigestion, and treat the indigestion only, using the oxalate of lime as a test of the effect of the treatment of the stomach and general system.

A solid animal diet, with little bread and vegetables, and no fruit, and with little sugar or vegetable acid, most quickly removes the flatulence; but even an entirely animal diet will not cause the oxalate of lime entirely to disappear. I have seen the octohedral crystals after three days of total abstinence from every kind of vegetable food. The moderate use of vegetable food is not to be forbidden; and, even regarding sugar, I do not advise you to prohibit its use entirely, for this reason, that it is impossible that your prohibition can be enforced, unless you stop all bread and vegetables. As long as any starch is taken as food, sugar will be formed by the action of the salivary and pancreatic fluids. What, then, is gained, for example, by forbidding sugar, but advising that rice should be eaten? If the rice cannot remain in the mouth for two minutes without giving rise to sugar, how manifestly either too much or too little must be enjoined when sugar is forbidden and starch allowed?

It is very certain, that, if vegetables are eaten, starch cannot be prohibited; in other words, sugar cannot be altogether stopped; and it ought to be considered, whether we should order no sugar, when some must be taken. It should also be remembered, that probably starch sugar is more injurious than cane sugar. It ferments more readily. It becomes converted into acid more quickly, and therefore more care should be taken to avoid starch than to prohibit ready-formed sugar. Lessen the quantity, then, of starch, as well as sugar, which your patient is taking.

There can be no question as to the benefit which frequently follows the use of the mineral acids, for the oxalate of lime will often entirely disappear during the use of the acid. Without doubt this sometimes follows in consequence of the removal of the cause; but you will find the oxalate of lime frequently return when the acid is discontinued, and you will often be disappointed at the frequent relapses of your patient. This may result from a return of the indigestion; but it very probably is also caused by the mineral acid passing off in the urine, and hindering the appearance of the oxalate of lime. Some easy experiments, which you can repeat, will, I think, explain to you the return of the oxalate of lime on the omission of the mineral acid.

If you take the fresh urine of a patient who is passing oxalate of lime; if you divide it into five portions, leaving one portion to itself, and adding different acids to the other four portions—as, for example, a small quantity of acetic acid to one, nitric acid to another, hydrochloric acid to a third, and sulphuric acid to the fourth—and leave them for twenty-four hours before you examine them, you will find that the natural urine will give octohedral crystals. The urine, acidulated with acetic acid, will also give plenty of crystals; but the other three portions, acidulated with the mineral acids, will give no oxalate-of-lime crystals. The crystallisation, if not altogether prevented, will be much retarded.

It is highly probable, that the same thing happens when the mineral acids are taken as medicines. After a time, they pass off in the urine, and thus hinder the oxalate of lime from making its appearance until the acids are stopped, when the crystals may reappear. But, even in this point of view, the mineral acids are valuable palliatives, playing the same part, as regards the deposit of oxalate of lime, that the alkalies play in the uric acid deposits. But, in addition to this palliative action, the mineral acids have a curative action acting directly on the stomach. It will be well shortly to take each acid separately, and not altogether, although they are sometimes so prescribed. Thus, you may occasionally meet with sulphuric, nitric, and hydrochloric acids in one recipe; and constantly you will find two of them in the same prescription. In fact, nitro-muriatic acid has come into more general use than any single acid.

1st. We will take *Sulphuric Acid.*—You see the best illustration of its use in colliquative perspiration, in menorrhagia, in hæmoptysis, and, more lately, in diarrhœa. It acts as an astringent. It is said to suppress the excessive secretion of pus from an open abscess. When you require an astringent action and a mineral acid, use dilute sulphuric acid. It should be taken when the stomach is empty, an hour or two before food.

2ndly. *Hydrochloric Acid.*—All the most satisfactory experiments show, that the stomach liberates this acid for the purposes of digestion, and therefore, if you want a mineral acid to assist digestion, give hydrochloric acid. In the convalescence from many diseases you will find the advantage of giving this acid just before each meal.

3rdly. *Nitric Acid.*—This acid in large doses was formerly used as a remedy for

syphilis. It was considered to increase secretion; to act on the liver like a mercurial medicine; even it was said to excite salivation. It is believed by some to act on the bowels, even when administered in baths. Hence, nitric acid is the opposite of sulphuric acid in its action.

The rule, then, which I have formed is, sulphuric acid to astringe, hydrochloric acid to promote digestion, nitric acid to promote secretion.

In India, nitric acid was much used as an external application in baths for disease of the liver. Dr. Scott thought that its efficacy was increased by the accidental presence of hydrochloric acid. The combination of the two acids was therefore tried, and a more energetic action was said to have been obtained than by nitric acid alone. Mr. Martin has given the following formula for the nitro-muriatic acid bath: Three parts of hydrochloric acid, two parts of nitric acid, mixed with five parts of water. Three ounces of this mixture to each gallon of water in the bath. Usually five or six gallons are requisite. The bath is usually taken at 96°, twice daily. The acid is to be renewed every week. Now, the positive proof of the benefit of this treatment is still wanting. At first it was employed in the general hospital, (there is even a formula for it in our Hospital Pharmacopœia) but now you rarely if ever see it employed. Many individual cases have been mentioned to me by Mr. Martin as conclusive, but still the question of its efficacy is not proved. Far less is it determined which substance is the active ingredient. Mr. Wallace, of Dublin, used chlorine baths, instead of the nitro-muriatic acid, and maintained that his results were more constantly favourable. Certainly, this use of a single ingredient is more likely to lead to accuracy than the method of using many substances internally or externally at the same time. The composition of the nitro-muriatic acid varies according to the temperature at which the acids are mixed or kept. There are, at least, four substances present—nitric acid ($NO_5\ HO_2$), muriatic acid (H Cl), chlorine (Cl), hypo-chloro-nitric acid ($NO_2\ Cl_2$); and perhaps, a fifth substance, chloro-nitrous acid (NO_2 Cl). The proportions of these different substances vary according to the temperature at which the nitro-muriatic acid is made and kept. I am far from being able to tell you what is the action of each of these different acids. Though the highest authority may be quoted for their conjoint use, yet, if you wish to prescribe rationally and not empirically, you will not use nitro-muriatic acid. For to which of its ingredients will you ascribe its efficacy? Or, as the proportion of its ingredients is so liable to vary with the mode of preparation, how can you tell what you are prescribing? If we shall ever gain any knowledge of the action of medicines, it will be by giving one substance, and only one, at a time. If our therapeutics are ever to advance, the first step will leave such combinations behind. Give nitric acid, give hydrochloric acid, give chlorine, or give hypo-chloro-nitric acid; but do not give all four together, until the separate action of each is well known.

In the use of any of the mineral acids, some care must be taken regarding the action of the acid on the teeth. I have known patients, who, for years had been free from toothache, suffer again in consequence of the action of an acid ordered for the treatment of the oxalate of lime in the urine. The acid may be taken through a straw or tube, but the simpler way is to wash the mouth with a teaspoonful of sp. ammoniæ arom. in a glass of water immediately after the medicine is taken.

If taken when food is present in the stomach, the mineral acids probably all assist only in dissolving the albuminous food, and have no medicinal action whatever. For the relief of indigestion they should always be given on an empty stomach, and so diluted that they cause no pain or uneasiness. The primary action of all the mineral acids is probably to lessen the irritability of the stomach; to render it less sensitive of the irritation which may be caused by the food. Perhaps, also, the quantity of acid thrown out by the stomach itself is thereby lessened. If an overdose of the mineral acids is given, the opposite effects are produced. Still, if the excessively acid taste permitted, far larger doses might be given than are usually prescribed. In my experiments ('Philosophical Transactions,' 1850) on the effect of sulphuric acid, I constantly gave a drachm of dilute acid, sp. gr. 1108, in an ounce and a half of water, thrice daily, without the slightest uneasiness; but on the other hand, ten minims of the same acid in the same quantity of water have caused uneasiness to some patients.

In many cases of oxalate of lime in the urine, I have given carbonate of ammonia

when the mineral acids have not agreed, and in doses of from five to ten grains. I have been well satisfied with its effect; it relieves the flatulence, and prevents its return. The great nervous depression which exists in some patients with oxalate of lime has also been an indication for the use of this remedy. Quinine and steel, sea-bathing and the shower-bath, bracing air and healthy exercise—in short, everything which can improve the general health, will be found beneficial for the indigestion which leads to the formation of oxalate of lime in the urine.—*Med. Times and Gazette, May* 6, 1854, *p.* 454.

138.—MODES OF EXHIBITING COD-LIVER OIL.

[The practitioner constantly meets with difficulties in exhibiting this remedy. The following hints are valuable:]

Cases in which difficulty occurs may be divided into the following classes:—1. *Those in which the Nauseous Taste of the Oil forms the obstacle.*—In these the use of the pale oil will generally obviate the difficulty; it is, however, four times the expense of the brown, and is more liable to be adulterated, which are great objections. The taste of the brown oil may often be concealed by taking it floating on some bitter menstruum. A wine-glassful of strong coffee, of ginger wine, of infusion of quassia, or perhaps, best of all, a quinine draught, containing a drachm of the tincture of orange-peel, may serve this purpose. The oil may be stirred up in a little hot milk, and swallowed so warm that the sensation of heat overpowers the taste. Should these expedients fail, the patient may be instructed to put into the mouth a teaspoonful of marmalade or of black currant preserve; and, having well lubricated all parts with the sweetmeat, so as to fully absorb the attention of the gustatory nerve, then swallow the oil. Advantage frequently results from closing the nostrils when taking the dose. 2. *Those in which the Oil excites Sickness, and is quickly rejected by Vomiting.*—Many of the expedients mentioned above will obviate also this source of difficulty, more especially the use of bitters. Very few cases indeed will resist the influence of hydrocyanic acid and bismuth exhibited three times daily for a few days preparatory to the trial of the oil, and continued during its employment. Patients should be directed to eat a little dry biscuit or bread-crust before the oil, and then to take it floating on a cup of the coldest spring water. If these fail, as a last resource, the dose should be given in the recumbent posture, that is, in the morning, an hour or two before getting up, and in the evening after going to bed. This last suggestion is one emanating, we believe, from Dr. Birkett, of the City Chest Hospital, and often answers very satisfactorily. 3. *Cases in which the Oil cannot be Digested*—This is a large and very important class. Patients complain that they have a great repugnance to the oil, that it makes them feel sick for hours afterwards, though seldom causing actual vomiting; that everything they take after it seems to taste of it, and that thus all relish for food is destroyed; that the oil "rises," either oily or with a most nauseous acid flavour. They frequently have bilious attacks; and, for a day or two in each week, probably the repugnance to the remedy is so great, that they are quite unable to overcome it. If pushed under these circumstances, the oil does more harm than good; there are, however, extremely few such cases in which careful attention to the digestive organs will not enable its administration to be successfully conducted.

We copy the following formula from the Pharmacopœia of the City Hospital for Diseases of the Chest, where it is in general use for the relief of the condition alluded to:—

> "℞. Rad. rhei ʒiij.; rad. zingiberis ʒij.; rad. gentian. ℥iss.; sod. carbon. ℥iij.; aquæ pur. lb. viij."

The roots having been cut into small pieces, the infusion is made with cold water, and, after standing in a cool place for about twelve hours, is strained. The mixture is clear and bright, and not in the least disagreeable to take. In cold weather it keeps well; but in summer the addition of some tincture is necessary to prevent it from becoming thick. If cough be present, it is usual to combine with each dose from half a drachm to a drachm of paregoric; if sickness, from two to three minims of the hydrocyanic acid; and if the signs of atony, a small quantity of the tincture of gentian, calumba, or hop.

By the use of this mild stomachic (a wine glassful thrice daily) for a week or two before commencing the oil, and then continuing it during the exhibition of the latter, many patients have been induced to bear it with advantage who had failed in other prior attempts to do so. The mixture is so mildly aperient that it almost never purges, and it may be continued for months together with the effect of improving most markedly both the appetite and the digestion. 4. *Cases in which the Oil, although taken easily, cannot be borne in full Quantity, and does not appear to produce much Benefit.*—In many cases of phthisis coming under this head, the combination of tonic medication with the use of the oil often answers well. So generally, in fact, is advantage derived from such combination, that at the Hospital for Chest Diseases there are very few patients indeed who take the oil alone. In almost all it is exhibited together with either the stomachic mixture above noticed, or some form of tonic. The favourite tonics are the sulphates of quinine and iron (gr. j. with gr. ij. ter die), or the sesquichloride of iron. The latter is extensively prescribed with the mineral acids in infusion of quassia, or, if the stomach be delicate, in the following more elegant form, which is a prescription of Dr. Risdon Bennett's:—

℞. Tinct. ferri sesquichl. ℥x.; acid. nitrici dil. ℥x.; syr. zingib. ʒss.; aq. menthæ viridis ℥j. Ft. haust.

Notice has been made repeatedly in our hospital records during the last two years of the practice of combining the use of cod-liver oil with that of mercury in small doses long continued, which prevails extensively at the hospital for Skin Diseases in cases of lupus and cutaneous struma, and at several other Institutions, in the treatment of chronic disease of the joints. This kind of treatment is becoming increasingly prevalent, and is certainly very applicable to many forms of strumous inflammation. The plan of giving the oil to counteract the depressing effects of a mercurial course for the cure of syphilis in cachectic states of the constitution, has also been previously mentioned, and is well worthy of being borne in mind.—*Med. Times and Gazette, May* 13, 1854, *p.* 487.

139.—*How to Test the Genuineness of Cod-liver Oil.*—We possess in sulphuric acid a simple re-agent by which genuine cod-liver oil is easily distinguished from most other kinds of oil: a few drops of this acid produce, in olive oil, a dirty grey colour; in poppy-oil, a deep yellow colour, approaching brown; and, in ordinary fish-oil, a deep brown colour; while poured drop by drop into cod-liver oil, sulphuric acid produces in it a centrifugal movement, particularly where the drops fall, and, at the same time, a beautiful violet colour, which changes to purple the moment the mixture is agitated, and, ultimately, to a rich sienna brown. This colour is no doubt due to the action of the sulphuric acid on the constituents of the bile contained in the oil. The action of the re-agent is best seen by adding one or two drops of concentrated sulphuric acid to about half a teaspoonful of the oil, spread out upon a white porcelain plate.—*Lancet, July* 29, 1854, *p.* 83.

140.—*Urticaria treated by Quinine.* By James Startin, Esq.—[This gentleman has found but few cases to resist the use of quinine. The following case will illustrate his treatment.]

A severe case, in which the eruption had come out daily for the last six months, presented itself a few days ago. The patient was a married woman, of middle age, and of nervous temperament. The following was the prescription ordered:—

℞. Quin. disulph. gr. xij; am. sesquicarb. ʒj.; magnes. carbon. ℥ss.; aq. pur. ℥viij. Ft. mist. ℥ss. ter. die sumend.

The quinine in this formula is of course undissolved, and the magnesia is added in order to suspend it. To relieve the itching of the eruption, a lotion containing the dilute nitric acid was ordered to be used. With regard to the latter, it may be worth remarking, that Mr. Startin believes it quite as useful as the hydrocyanic acid for the relief of tingling, &c., while it is very much less expensive.—*Medical Times and Gazette, May* 13, 1854, *p.* 487.

141.—*Novel mode of applying Chloride of Zinc.* By E. Cock, Esq.—In many cases, it is of great importance, in the use of chloride of zinc as a caustic, that it should not run. Such a one is at present under the care of Mr. Cock, in Guy's Hospital, in which it is being used to destroy a polypoid growth in the nose, part of which has been extracted. The spot to which the application is made is deeply placed in an enormously dilated nostril, and it is absolutely necessary to limit its action to the part affected, and, at the same time, desirable that it should there act freely. A paste made with flour or bread crumb was first tried, but this was very inconveniently moist, and liable to run. Mr. Cock now uses plaster of Paris, which answers admirably. A paste may be made of suitable consistence, and which does not afterwards become in any degree more liquid. It is laid on a piece of lint cut to the wished-for size, which is then carried into the nostril, pressed on to the diseased part, and left in. It should be remembered, that the long-continued pain which often follows the use of escharotics, is from their gradual extension to parts not at first involved. The duration of the pain, after the use of chloride of zinc in this way, is generally much less than if it be used in a paste more liable to liquefy. —*Med. Times and Gazette, May* 13, 1854, *p.* 487.

142.—ON NEURALGIC HEADACHE.

By Dr. P. J. Murphy.

Neuralgic headache is synonymous with those headaches described by some old authors as hemicrania, by others as clavus hystericus, and by Dr. Graves as hysterical congestion. It is *peculiar* to females, and to females during a certain period of their existence only—from puberty until the final cessation of the menstrual secretion. Dr. Graves gives a graphic description of the symptoms and of the injurious effects of the usual routine treatment. He calls it hysterical congestion; but he seems not to have understood its true pathology. There is no doubt of its being hysterical, but there is no congestion, for the seat of the pain is in one of the nerves of the scalp, which can be easily proved by a slight examination, and it is therefore an external headache. The error may have arisen from his having met with cases where this headache was in combination with the anæmic headache. The proper name which should be bestowed on this headache, in order to facilitate the diagnosis, is spinal irritation of the sub-occipital nerve. Spinal irritation is beginning to be understood in this country; we are indebted to a French physician, M. Valleix, for the discovery. Since then many other disorders, such as irritable mammæ, pleurodyne, and neuralgic headache, are discovered to originate in functional derangement of the spinal cord; and I believe whoever will carefully compare these disorders with cases related by Dr. Tilt must come to the conclusion that they are nothing more than symptoms of subacute ovaritis. They are hysterical disorders, and *hysteria is subacute ovaritis*, which displays its phenomena on the sensitive and motive nerves of the spinal column.

On comparing the neuralgic headache with the phenomena of spinal irritation in other parts, we find how exactly they coincide. Like spinal irritation, it is a form of hysteria, and therefore peculiar to females. It is not only peculiar to females, but attacks them only during the menstruating period of their existence—that is, from about the thirteenth to the fiftieth year. It is exacerbated just previous to menstruation, makes its first attack on the left side, and rarely passes over to the right side.

Cause.—As this form of headache is peculiar to the female sex, it must therefore have its origin in some organ peculiar to them; and as it is felt during a certain period of existence only, the organ must have the performance of its functions limited to that period. As there is no organ by which these two facts are explicable, unless the ovarium, it is unphilosophical not to conclude that the disorder proceeds from the ovarium. There is certainly also the uterus, but the functions of this viscus cease on the removal of the ovaria. We daily meet with the uterus inflamed, ulcerated from cancer or cauliflower excrescences, distended by hydatids or pregnancy, producing moles and polypi, but none of the phenomena of spinal irritation at present. In the married female who bears children regularly it is scarcely ever known. Before the commencement of menstruation, or after its ter-

mination, it is equally rare. What is the state of the ovarium I do not pretend to affirm. If inflammation, yet it has often yielded to tonics; it may depend on moral causes, but such an explanation has never satisfied me. An accumulation of fæces in the rectum has appeared to me as occasionally the source of irritation; in a few cases I think it was traceable to ascarides in the rectum. We witness the action of cold in paralysing the trunk of a motor nerve, the portio dura, as it escapes from its cranial foramen, but cold cannot be a cause of this headache, otherwise why should not the male sex equally suffer.

Occasionally spinal irritation, in other parts, has been observed earlier in life, but I have not met with the headache; and, as the headache has occurred some years before the appearance of the menses, so I believe it possible it may arise a few years after their total cessation. The headache resembles spinal irritation, also, in a curious and hitherto unexplained phenomena: *commencing* on the left half of the body, we occasionally meet with it also on the other side; but I have never discovered that it began there, nor is it ever restricted solely to that side. When both sides are attacked they are unequally so, the left being by far the more painful. As another proof of its being spinal irritation, if further proof be necessary, we find it under two distinct forms, and these forms are easily distinguishable by the nature and extent of the pain. In the one it is confined to the exact tract of the sub-occipital nerve, it is lancinating or shooting, intermitting, and chiefly felt at its termination in the integuments of the temporal region; when severe in this spot it is the *clavus hystericus*. When the whole course of the nerve and its branches are implicated the entire left side of the scalp is very tender, sometimes exquisitely so; this is the hemicrania. It is singular how much this disease is confined to the left side of the head; we find such to be the fact in ninety-nine cases out of a hundred. It seldom reaches the aggravated form of clavus hystericus without being accompanied with other well-known hysterical symptoms, which, of course, facilitate the diagnosis.

Diagnosis.—This headache attacks females exclusively. I have never heard nor read of men suffering from this kind of headache. It is only during the menstruating period of life that even females are liable. The pain is referred to the left side of the head; it is worse on the approach of the menstrual flow; it is found in the track of the sub-occipital nerve. The course of this nerve is well known; it accompanies the sub-occipital artery, emerging from the spinal canal; it passes along the back of the head, midway between the mastoid process and the mesial line, sending branches to the integuments which cover the parietal protuberance, and terminating in the temporal region. Its course, from its exit to its termination, can oftentimes be accurately ascertained, from the pain induced by pressing upon it. Although the head suffers, pressure may not always produce the pain, for it is intermitting. In general, however, pain may be thus detected in one of three places: on the left side of the neck, where the head and vertebræ join, at the parietal protuberance, or in the temporal region; when concentrated in the last spot it is the well-known clavus. It is sometimes painful in all three, and sometimes in its whole track. It is, however, rare that the tenderness is absent in the occipital region. The part suffers more when pinched than when pressed. When the branches as well as trunk suffer, we then have hemicrania—a most painful form, less intermitting than the other, and preventing the unfortunate girl from lying on the affected side. It is more commonly met in the unmarried female, from the twenty-second to the thirty-fifth year, but the married females who are childless do not escape.

This headache is chronic, intermitting, may continue for days, weeks, or months, then subside, and return after the lapse of months, or even years. A first attack is seldom felt before the twentieth year, nor after the thirty-fifth. The pain is generally of a shooting kind, darting from the neck towards the temple, and never towards the neck, by which it is easily distinguished from odontalgic pain. Neuralgia of the left mamma (irritable breast), or of the seventh or eighth intercostal (pleurodyne), frequently co-exists. It is sometimes found in combination with the anæmic, but more rarely with the congestive headache. From caries of the body of a vertebra it is easily distinguished by the pain being superficial, being confined to the left side of the spine, by its not becoming worse when the head is flexed on the chest, nor by jumping, nor by pressing the head against the spinal column.

This neuralgic pain sometimes accompanies the rotated spine. It is singular how often toothache is mistaken for headache, especially for this form. In both the pain

is described as shooting in the course of the nerves, but in toothache the pain shoots towards the neck and ear, leaves no tenderness of scalp, never goes so high as the parietal protuberance, and is more correctly discovered by learning that a paroxysm is brought on by food, sometimes when warm, at other times when cold.

Treatment.—If the disease be not complicated, we can promise relief. The bowels should be kept open by regulated diet, and by aperients, such as castor oil, olive oil, lenitive electuary, powdered rhubarb, soluble tartar, or the compound rhubarb pill. If the bowels are obstinate, an enema of a pint of cold water daily answers the double purpose of removing the contents which may irritate the ovary, and as a local application to the organ chiefly in fault. The cold hip-bath is a valuable remedy when the constitution is vigorous, but all these things are inferior to sea-bathing. Stimulants should be abstained from, employment should be found for mind or body, but physical efforts are preferable. The sedentary position required by the needle, especially in solitude, is very injurious. A sinapism over the exit of the nerve gives great temporary relief; a vesicating plaster of cantharides is better, but it oftentimes leaves a mark, and therefore, on account of sex, age, and position in life, may be objectionable. A croton-oil liniment, made with one drachm of oil and one ounce of camphorated tincture of opium, and rubbed until pustules appear, is preferable, as it leaves no permanent blemish. The belladonna plaster, mixed with powdered opium, or a liniment of extract of belladonna, rubbed with mucilage, are useful and unobjectionable remedies. Speedy relief is occasionally afforded by veratrine or aconitine ointment, made with from four to six grains to half an ounce of spermaceti ointment. The finger used in rubbing should have a piece of bladder interposed.

One ounce of tincture of aconite with seven ounces of rose-water, is a safer remedy to trust to inexperienced hands than the veratria. The internal medicines are not so easily chosen. Tonics are frequently required, and they may be combined with anti-hysteric remedies. The disulphate of quinine may be exhibited in a strong infusion of valerian; compound iron pill, with assafœtida in large doses, is very beneficial. If there be irritability of stomach co-existing with profuse menstruation and leucorrhœa, pills of valerianate of zinc, half a grain three times a day, with one drop of creasote, answer many intentions. If there be much debility, the sulphate of iron may be combined with infusion of valerian and ammonia, or the ammoniated tincture of valerian may be prescribed. The pain is sometimes so acute that some relief is quickly demanded, and half a grain of morphine will lessen the pain for a while until other remedies have time to act. For the leucorrhœa one drachm of acetate of zinc to one pound of distilled water is useful as a lotion. But we are sometimes perplexed, for the tonic treatment is not the best for a full plethoric female; leeching or even general bleeding is required, but the cases are rare which require general bleeding.

If the patient be not very weak, and there is much leucorrhœa and menorrhagia, the treatment laid down by Dr. Tilt for subacute ovaritis should be adopted. He leeches in the menstrual interval, and then blisters the iliac regions: but as his work is universally read, the treatment is well known. Sea-bathing, when practicable, should never be omitted.—*Lancet, May* 20, 1854, *p.* 541.

143.—ON CHRONIC AND PERIODICAL HEADACHE.

By Dr. E. H. Sieveking. (Read before the Harveian Society.)

Occasionally it almost appears as if an inverse ratio existed between the danger threatened by a given disease and the pain accompanying it. Certain it is that many of the most fatal maladies are not marked by a proportionate amount of suffering, and that many which are ushered in by bodily anguish lose this feature in a great measure as the "day of nothingness" approaches. Pain is a valuable indicator to the physician of hidden processes; but, at the same time, it is one which in its turn requires to be measured, in order to be appreciated by the observer at its real value, and for which, unfortunately, we possess no standard, except that afforded to us by an intimate knowledge of the individual before us, or a vague, empirical acquaintance with human nature at large. An "odynometer" would be of vast service in reading the history of disease; but we must advance much beyond our present

understanding of the nervous system before we can expect to meet with an invention of this kind. Whether, however, the pain be represented by a known quantity of greater or less magnitude, it is invariably sufficiently unpleasant to the sufferer to desire some means of speedy relief, and this very natural wish is readily responded to by sympathizing friends or more scientific Æsculapii. But the apparent urgency of the symptom is the very feature which often induces the patient to employ his remedies inconsiderately and at random, while it places him at the mercy of persons who are intent upon their own advantage rather than upon the other's relief.

Pain is the sentinel of health. Pain, to employ Romberg's oriental imagery, is the prayer uttered by the nerve for healthy blood. Pain was placed by our Maker as the beneficent guardian of this mortal fabric, a warning friend more often than an avenging angel. But none of our feelings or our emotions are isolated; nor is there one which is not, more or less, under the domination of the higher powers of the mind. Though pain be but a physical manifestation, the actual suffering may be much exalted or diminished by the intellectual and moral force brought to resist its encroachment. This is an element that we practically regard every day, in estimating the importance of the symptom in this or that case; but as we are commonly free from suffering at the time we have to judge our patient's condition, we are rather apt to demand a greater exercise of self-command and resolution from him than may be quite just; nor is it always the case that those who show least sympathy to others have themselves the greatest powers of endurance in the time of need. The significance of pain, then, varies in different individuals, as it varies in different classes, or in the same individuals at different ages, no less than its expression. The susceptibility to pain is probably greatest immediately after birth, though the actual suffering cannot have attained its climax until the mental powers are so far developed as to allow of the feeling of conscious apprehension. During the first years of life, spasmodic action appears to be the form in which the nervous system, as it were, responds to the tax made upon it. As life advances, the capability of endurance increases; but an excess of pain is met by a temporary arrest of the powers of perception, and the sufferer is relieved by syncope. I need not now enlarge upon the difference displayed in this respect by the two sexes. We all know the fortitude exhibited by those who, though more fragile in form and constitution, appear, in times demanding a moral effort, to bear pain better than the sons of Adam; and yet these sons, using their prerogative of power, have made it a by-word to bear pain like a woman. It would almost seem as if she were trained, by the many minor aches that she is subject to, to the endurance of great suffering; they certainly contribute not a little towards rendering her the sympathising comforter, whose very look is a healing balm in the dark day of weariness and pain.

I have ventured to make these preliminary remarks as an excuse for devoting such consideration as the necessary limits of the occasion will permit, to some of those morbid states which are characterised by pain, confined to one—the most important—part of the body, the head. Headache, like pain experienced elsewhere, is not a disease, but a symptom of disease; and yet it is so commonly the only sign recognised by the patient, that it is constantly exalted to the rank of disease itself. The one great difficulty already alluded to, in appreciating the value of pain as a symptom, is, that it belongs entirely to the class of subjective phenomena; it is in no way physically tangible. The difficulty in the present instance is much enhanced by the inaccessible character of the organ or organs belonging to the head. While its presence in the abdomen, thorax, or extremities, may be more closely investigated, and its accompanying symptoms more accurately defined by means of direct appliances, we are much more limited to what the lawyer terms circumstantial evidence, in order to establish its intensity, its character, its relations to other morbid phenomena, when affecting the head. Much of what we regard as acknowledged fact in this domain of pathology is yet but hypothetical; there is still more than any one has yet attempted to withdraw from the range of mere conjecture. But while this is only an argument in favour of our making the subject one of earnest consideration, it warns me not to increase the obscurities already surrounding it by increasing the hypothetical structure. I have carefully gone through the numerous cases which I have recorded in my own practice, in which headache was the

prominent symptom, and to some of the conclusions at which I have thus arrived I shall beg your considerate attention.

A question that meets us on the threshold of the subject is one that fortunately no longer admits of an ambiguous answer. It is, whether the absolute amount of blood contained within the cranium varies. The practice of all ages negatived the supposition, that the quantity of blood within the skull was always the same, until the apple of discord was thrown in by some experimenters, among whom Dr. Kellies' name stands foremost. He has, however, been completely refuted by Dr. Burrows, who meets Dr. Kellies' views by experiments and arguments of a most convincing character. Dr. Burrows' summary is "that experiments and physiological considerations lead us to the conclusion that the quantity of the blood within the cranium is extremely variable at different times and under different circumstances." This statement is no way opposed by the fact, that the total contents of the cranium must at all times be a nearly constant quantity; for there are various mechanical provisions which are evidently destined to keep up the balance when a change in the amount of one or the other constituents takes place. One of these is the meningeal serosity which may freely pass from the spinal to the cerebral cavity, and is immediately under the influence of atmospheric pressure in the former. Another is the venous *rete mirabile* contained in each lateral ventricle, known to you as the choroid plexus, and capable of very considerable distension or contraction, according to the amount of blood contained,—a safety-valve of very remarkable construction, and capable of aiding the system, not merely as a receptacle of blood, but also by the secretion of various matters, the accumulation of which would be injurious to the constitution. We allude not merely to serum, but to the concentric bodies containing phosphate of lime, and to epithelial growths that are so frequently found in or about the plexuses. Another provision consists in the extreme elasticity of the white matter of the brain, secured by its canalicular structure, no less than by what my researches have led me to regard as the peculiar conformation of the blood-vessels of this portion of the brain. This last point is not one for present consideration; but in the case of any one having turned his attention to the circulation within the brain, I would request him specially to attend to the arrangement of the minute vessels, with a view to determining whether or not they present enlargements resembling reservoirs, fed and emptied by numerous conduits of much smaller dimensions than themselves.

Nothing is of more importance in reference to the pathology and therapeutics of the head than clear and well-defined notions on the physiological subject of the circulation within the cranium; for, among the various sources of medical scepticism, no one is more puzzling or more destructive of logical practice than a contradiction between the doctrines of physiology and the daily practice of medicine.

What Hippocrates said of spasm—that it results either from fulness or emptiness, or to use more modern terms, from hyperæmia or anæmia,—applies equally to headache; but, to embrace all the causes of this affection, we must add a third element, which, though most commonly complicating one of the above circumstances, is not necessarily included in them, viz., a change in the constitution of the blood. Dr. Bennett is of opinion, that "headache is always occasioned by pressure, which probably arises from local congestions of blood, although the causes tending to these, or the means by which they are produced, are yet unknown." This view, if correct, would simplify our pathology of the subject, as well as our treatment; but, while it applies to a majority of the cases of headache that come under our notice, it is far from universal, and, unless received with considerable qualifications, would perpetuate the exclusively antiphlogistic proceedings from which therapeutics appear to be emerging. This remark is borne out by the experience of Sir Henry Holland, who, in speaking of headache, says, that "many of its varieties are as little defined by our knowledge of their causes as by the terms used in describing them; though probably the greater number depend on different conditions of the circulation through the brain; of which conditions, that of congestion or retarded circulation, varying in degree and in the vessels in which it occurs, is presumedly the most frequent." The same author, adverting to vertigo as a frequent concomitant of cerebral affections, speaks of it "as a symptom which often conveys alarm, from the idea of pressure on the brain, and leads to immediate depletion under this view. In some cases," he continues, "the inference and practice are just and beneficial; but there are others,

(and these, as I think, of frequent occurrence), where vertigo is the effect of causes wholly remote from fulness or over-action, being, in fact, a step towards syncope, and a proof of exhaustion of the nervous power; here we require, not bleeding, but cordials, to restore the brain; and a wrong practice may lead into serious danger."

The three essential points upon which we would insist, as requiring our first consideration, in forming an opinion upon an individual case of cephalalgia, are, whether the *causa proxima* consists in an increased afflux of blood, in a diminished amount of blood, or in a specific dyscrasia. None of these matters can be discussed otherwise than hypothetically, without some previous history of the case; the external appearance of the patient, though a guide, is often fallacious; and though we might, in marked instances, determine, by the light of empirical experience, whether we have to deal with hyperæmia or anæmia, the frequency with which dyscrasic conditions of the blood constitute an essential element in the causation of headache is so great, as to greatly influence the results of our practice, according as we attend to it or neglect it. Romberg, in speaking of the difficulties attending the physical exploration of the brain in cephalalgia, which he treats of simply as hyperæsthesia, states, that "one means of approach has been overlooked which we ought certainly to avail ourselves of: it is a fact, that during every vigorous and long-continued act of expiration the brain is elevated, the cerebellum being pressed against the tentorium, the cerebrum against the cranial bones; the old surgeons, acting upon a knowledge of this circumstance, recommended their patients who suffered from penetrating wounds of the cranium to cough violently, or sneeze, in order to promote the discharge of blood or pus. We may therefore employ continued expiration, or holding the breath during expiration, in cerebral diseases, especially if it affects the surface, as a sort of substitute for the external pressure, which we so frequently have recourse to in the exploration of the abdomen or thorax. The patients alluded to generally complain of the head-ache being brought on by straining or defecation. For the purposes of diagnosis, we cause the patient to imitate this effort, by holding the breath for some time during expiration, while the abdominal muscles are contracted. This at once brings on the pain, or if it was present increases it to the utmost. The same occurs in screaming, coughing, and vomiting. Similar experiments may be instituted during inspiration, during which the brain falls and approaches the base of the skull; we may thus obtain some information on the diseases affecting the base of the cerebrum and cerebellum.

The form of headache that probably occurs most frequently to every practitioner is that which affects the female in connexion with some disturbance of the uterine and ovarian functions. It is so common, and the prevailing tendency to set down the complaints of the softer sex under the list of hysterical affections is so general, that I would at once warn against a too hasty assumption of the sexual origin of the pain; for an error in diagnosis entails a serious error in treatment; and even in the event of the sexual influence being established, a very different proceeding may be advisable in different cases.

Although I am not prepared with a classification of the various forms of cephalalgia that shall embrace every feature and complication that presents itself to us in practice, I think, that by keeping in view the *causa proxima* of the increased, diminished, or altered blood, and engrafting thereon whatever our enquiries may establish regarding the exciting causes, we may arrive at a tolerably correct view of the nature of the malady in the individual instance. As I have before observed, several conditions may be complicated with one another, and thus obscure our view at first; but, by care, we may always succeed in laying hold of a leading feature, round which the minor traits are grouped, and having done so, the principle both of diagnosis and of treatment is established. Accordingly it appears convenient to divide the exciting causes of cephalalgia into three classes,—those directly affecting the head, those proceeding from the chylopoietic viscera, or the organs of nutrition, and those taking their origin in the sexual apparatus. Among the causes of cephalalgia residing as it were in the cranial contents, or affecting them without the intervention of other organs, the foremost are mental and intellectual excitement carried beyond the normal limits of healthy stimulation; it may act by a shock, as in a child that is frightened or suddenly carried into a brilliant light, or made the gazing-stock of pseudo-admiring friends at an hour when it ought to be in the land of dreams: it may act by the continued strain upon the reproductive energies of the earnest student diving into

the secrets of nature that are revealed to none but such as are prepared to sacrifice themselves on her altars; it may, after days and nights of unremitting toil, prostrate the poor milliner, who, with a weary heart and aching eyes, uses her very life-strings to earn bread for the invalid parents. A direct physical stimulus is anything that, through the organs of smell, of hearing, or of sight, gains admission to the perceptive faculties. The excitement may produce but a temporary result, commensurate with the powers of the individual; or it may find a favourable nidus, and the deranged action once set up may be propagated until broken by some greater countervailing power. Many forms of epilepsy are nothing but the spasmodic expression of this derangement; and for private use, I constantly employ the term of cephalalgia epileptica, as indicative at once of what I regard as a cause and a tendency, occurring possibly in a subject in whom the epileptic paroxysm has been manifested merely by slight vertigonous attacks, by a single attack in former times, or by some spasmodic action that alone would not be regarded as of an epileptiform character. The pain in this form of headache may affect any part of the head, but it is frequently limited to a spot at the vertex; and, where that is the case, I have found marked benefit from making the attack directly upon the apparent seat of injury. Thus, in the case of a needlewoman in whom intense chronic cephalalgia and partial blindness has been produced by overwork, nothing has afforded so much relief as the application of powerful counter-irritation to the vertex, where the pain is limited to a space the size of half-a-crown, and the hair has fallen out. Her constitution is otherwise good, and no medicine avails beyond the occasional exhibition of a warm purgative. The constant action of the cause prevents even the chance of a cure in her instance. In many cases the avoidance of the cause is sufficient to remove the pain. In some, speedy assistance is demanded, and we commonly fly to counter-irritation and purging. But these remedies are not always indicated. The condition of the brain may be solely a state of hyperæsthesia, or, in other words, a state of extreme sensitiveness, unaccompanied, as we should suppose it to be, in cases indicating a derivative procedure, by vascular repletion. Thus, in the case of a young authoress, who, for a year and a half, had been subject to frequent paroxysmal headache, with pains spreading along the distribution of the fifth pair, unaccompanied by any visceral or uterine disorder, what certainly looked like a permanent cure was effected by a brief exhibition of the citrate of iron and quina. But where the nature of the case is not clearly apparent, we cannot be too careful to solve the problem to the best of our ability, before adopting a definite line of treatment. The presence or absence of vertigo in younger people, and the state of the heart and circulating organs, the previous occurrence of epileptiform seizures in the young and middle-aged, the previous occurrence of apoplectic symptoms in those advanced in life, must be attended to, and their bearing well marked. Our conclusion may determine the life or death of the individual.

Among the causes that act by direct irritation of the brain, to omit external lesions as not immediately bearing upon our subject, we find congestive states produced by previous disease of the lungs, as hooping cough, or emphysema. Catarrhs, which are frequently accompanied by severe throbbing frontal headache with a stillicidium oculorum et nasi, act partly by the implication of the frontal and sphenoidal sinuses, partly by the general febrile state of the blood acting as a special irritant to the meninges; the value of the mustard plaster, or in severer cases, the blister, applied to the nape of the neck, and accompanied by such other treatment as the special case may demand, sufficiently shows that here the pain is not merely of a hyperæsthetic character.

Of the alterative or dyscrasic diseases, giving rise to headaches, by inducing direct irritation through the vascular current, or by exciting a local morbid action, which in its turn proves a source of irritation, the most marked are rheumatism and syphilis. Rheumatism affecting the head is frequently regarded solely as a source of pericranial pain, characterised by external tenderness and puffiness of the scalp. Romberg describes this form of headache as presenting a hemicranial and paroxysmal character, marked by a peculiar sensibility of single parts, especially of the forehead and vertex, the pain radiating from these in various directions. I do not find that authors allude to the possibility of an intracranial rheumatic affection; but I have repeatedly met with cases of cephalalgia in which no such external indications presented themselves, and in which the concomitant symptoms, though but feebly

marked, the history of the case, and the anti-rheumatic treatment adopted, appeared to justify the conclusion that the dura mater, the fibrous envelope of the brain, was the seat of the disease.

To determine the question, the whole complex of symptoms must be taken into consideration. It is always unsatisfactory to establish our diagnosis by the result of our treatment; still, until we have very definite data for the former, we must be allowed occasionally to argue from the *post hoc*. Nor is it quite unreasonable to conclude from what we know of the action of certain medicines, that their successful application is, *à tanto*, evidence of a definite derangement of the blood. Thus if, as in the case of a man, aged 34, who for six weeks had suffered from violent pain, affecting the entire of the head, attributed to sleeping in a damp bed and aggravated by the warmth of the bed, and who had been previously treated without benefit, we find guaiacum and iodide of potassium give speedy and permanent relief, few practitioners would question the reasonableness of the inference that his cephalalgia had been due to the rheumatic poison. Less difficulty is presented in determining the nature of syphilitic headache, though these cases are occasionally obscure, and do not always present such unequivocal signs as to enable us at first sight to determine their character. This is more particularly the case when there is an apparent history of previous rheumatism. When we see the syphilitic taint on the surface, the diagnosis is easily established; but this is not necessarily visible, and, without some palpable indication of lues, the admission of infection is not readily obtained. The following abridged case illustrates these remarks:—M. A. W., aged 29, the wife of a plasterer, stated she had had rheumatism six or seven years previously, and had for several years been subject to severe headache, the pain being mostly at the vertex, and accompanied by a sense of something falling in the head from back to front, or of bursting. From the absence of other cause, and the previous history, I set it down to rheumatism, and treated it without iod. pot. This gave partial relief, and I continued the treatment on the assumption of its being rheumatic disease for about six weeks. I then, however, clearly established the existence of syphilitic secondary symptoms, and after a course of mercurial inunction, followed by iod. potass. and tonics, the cephalalgia entirely disappeared, with the syphilitic ulceration. I forbear multiplying cases, as an extensive range of subjects bearing upon the main question is yet before me, too large in fact to allow of more than a cursory notice.

I proceed to consider the varieties of headache in which the condition of the chylopoietic viscera, and the organs of nutrition, is more particularly at fault. The terms cephalalgic, biliosa, crapulosa, saburralis, gastrica, and others, are used to designate this form of cephalalgia; while the vernacular, bilious, or sick headache, is commonly, though often erroneously, applied to headache taking its origin in the abdominal organs destined for the support of the individual. The antecedent excesses, the derangement of the appetite, the furred tongue, and altered alvine and urinary secretions, generally suffice to determine cases of recent dyspeptic headache in the child and in the adult; and an emetic or a purge commonly at once rectifies the abnormal condition. Such cases may be regarded as partaking of the congestive type, both from the throbbing character of the pain, the flushing, and the immediate physical influence exerted by the overloaded and distended gastro-intestinal to act upon the circulating organs. Headaches, connected with derangements of the chylopoietic viscera of longer standing, and involving deeper-seated lesions of the organic power, are not so easily combated, and require some discrimination.

There is a form which is more particularly connected with the state of the portal circulation, occurring in subjects leading a sedentary life, though not necessarily given to any excesses in diet, or in persons not intemperate, but habituated to the ordinary luxurious living of the higher classes, and especially to the unphysiological custom of heavy, late dinners; or, again, in females after the grand climacteric, where a species of vicarious action is often set up between the hemorrhoidal and menstrual discharge, and the portal system becomes surcharged by these new duties. This form of headache may not inaptly be designated cephalalgia hemorrhoidalis, not that I wish to add to the complexity of the subject by multiplying names, but that in my mind the term is associated with a definite lesion, implying a distinct procedure. The subjects are ordinarily persons of middle age or advanced life, with a sallow, dry complexion, more or less subject to hemorrhagic discharges per anum, and

occasional severe attacks of inflammatory hemorrhoidal swellings. A peculiar irritability of temper is often characteristic of such individuals. The headache is sudden, violent, and vertigonous; they see muscæ volitantes or strange figures; a black veil is occasionally drawn over their eyes, and the threatened loss of sight excites the greatest alarm. There is no vascular excitement, the tongue may exhibit some gastric derangement, but is often perfectly clean; there is a general sense of lassitude and disinclination to mental or bodily labour, with an indescribable sensation of restlessness and discomfort. Such cases are not benefited by active treatment, in the shape of powerful counter-irritation or purging. Gentle warm laxatives, followed by the employment of quinine and accompanied by a suitable diet and regimen, ordinarily suffice to restore the balance of the circulation and to relieve the head. Mild mercurials may be indicated where the liver is manifestly deranged, but they should not be given without satisfactory evidence to that effect, as they too often increase the prostration, and thus render a protracted course of tonics necessary. Where an habitual hemorrhoidal discharge has been arrested, I have obtained marked benefit from the application of from two to four leeches to the verge of the anus, while I am inclined to regard a combination of flores sulphuris and magnesia, with the occasional addition of a portion of rhubarb and some aromatic, as almost a specific in this species of portal derangement. The flowers of sulphur operate mildly on the bowels, and they relieve instead of increasing the tenesmus, which in hemorrhoidal subjects is a source of much suffering. The following is a formula, such as I frequently employ:—

℞. Flor. sulphuris, magn. carbon., aa, 3iiiss.; pulv. rhei 3j.; ol. cinnam. ℥viii.

Misce, a. cochleare minimum horâ somni ex lacte sumendum.

Taraxacum is likewise a valuable cholagogue applicable to these cases, but in almost all the cure will be promoted and rendered more or less permanent by the exhibition of bitters, and especially of small doses of quina.

Both sexes are liable to the hemorrhoidal form of headache, but the female sex are necessarily alone liable to that form which arises from the excessive tax laid upon the organs of nutrition in undue lactation. This is accompanied by vascular and nervous symptoms of great prostration, manifested by palpitation, dyspnœa, formication, a sense of treading upon nuts, and such like evidence of hyperæsthesia. Moderation in nursing, especially separation of the mother from the infant at night, tonic diet and regimen, change of air, and the exhibition of steel, gentian, or bark, may enable the mother to continue lactation, if circumstances, and particularly the age of the child, may render this desirable; but in many instances no advantages are to be obtained by any procedure short of weaning. There are other cases in which the whole system of nutrition is so utterly deranged, by previous severe illness or excessive medication, that without any apparent local disorder in the chylopoietic viscera, and without any symptoms indicating organic mischief in the brain, we are utterly baffled in our attempts to relieve the sufferer from the constant and harassing headache complained of. A young woman, aged 19, has long been, and still continues under my care, who, up to the age of seven years was, according to the statement of the mother, a robust, hearty child. At that time she had typhus fever, during which she was salivated. She has recovered from the effects of this treatment, and she has now long been subject to pain at the vertex, manifestly of the most distressing character, and for which I regret to say, I have entirely failed in discovering the remedy.

Lead poisoning, which produces such marked derangements in the nervous system, does not appear often to induce cephalic pains; the neuralgic affections resulting from this cause present a more superficial character. There are other poisons which give rise to cephalalgia, among which I would more particularly advert to the poison of urea circulating in the blood from degeneration of the kidneys. I dare not, however, important and interesting as the subject is, do more than allude to it, if it be not to urge the necessity, in obscure cases, as they sometimes occur, to pay especial attention to the state of the urinary organs. Not long since, the *post-mortem* of a man was held in St. Mary's Hospital, who had been an out-patient under my care and that of Dr. Markham, for epilepsy and intense cephalalgia, which I regarded from the antecedents, as a rheumatic affection of the dura mater. He was, as my friend and colleague can testify, much relieved by counter-irritation, calomel, and purging. Dr. Markham, under whose care he subsequently came, found that there

was so intimate a relation between the cessation of the mercurial action and the disease, that, on a relapse taking place, he resumed the previous plan of treatment. He then became an in-patient, on account of the appearance of an enormous carbuncle, to which his death was attributed. The only lesions discovered adequate to account for the symptoms, though not for the apparent success of treatment, were some congestion of the superficial meningeal vessels; a marked development of the pacchionian bodies, which had actually perforated the dura mater; and extreme atrophy of the kidneys, rendering them totally unfit to perform their functions.

There are forms of headache which, while manifestly originating in the gastro-intestinal tract, seem to be rather the result of reflex irritation than of any influence upon the vascular current. Such are cases in which the only palpable lesion is discoverable, the presence of worms. Ascarides may act in this way; and tenia also gives rise to cephalalgia. The indication here naturally is to remove the parasites; when the reflex irritation of the brain, will, if owing to that cause, necessarily at once disappear.

The third class of headaches, that mainly connected with derangement of the sexual apparatus, now claims our attention.

The patients whose cases belong to this category are mainly if not exclusively females. We find epileptiform seizures occurring in youths at the period of puberty, and apparently connected with an irregular development, or an abuse of the sexual organs; but, though that fact may justify the assumption, that cephalalgia may in them, at times, originate in the same cause, experience does not frequently bring instances to our notice. My memory does not recal one of the kind. The controlling influence which the sexual apparatus exerts upon the mind and the body of the female is so universal, that we are more prone to exaggerate than to underrate its power; and though an undoubted connection is traceable in a majority of cases between derangement in the ovarian and uterine functions and the occurrence of headache, we must be cautious to avoid the error of overlooking many other conditions which affect the two sexes almost equally. The danger of over-estimating this one item in the causation of morbid conditions in the female re-acts injuriously upon the medical man, as well as upon his patient; in the former it may induce a disregard to a more enlarged view of pathological relations, and it will cause him to concentrate his therapeutic proceedings upon one part of the system alone; in the latter, the very tendency to regard all her maladies, as springing from one source alone, may induce a pseudo-morbid or morbid condition, which otherwise might not have occurred.

The subdivisions of the *causa proxima* of headache, to which I have more than once alluded, apply in the present instance with as much force as in other species of cephalalgia. It may present a congestive or an anemic type, or the blood may materially deviate from its normal constitution. It will not therefore suffice, when we have to deal with a case of cephalalgia, to determine, for instance, that it is concomitant with amenorrhœa; but to render the diagnosis complete, and the consequent treatment satisfactory, the cause and character of the menstrual arrest must be ascertained. Moreover, there are cases in which the two disturbances appear to co-exist without a definite relation of cause and effect; and where the empirical adoption of remedies requisite to restore the healthy functions of the reproductive system may be imperatively counter-indicated by the deeper-seated and more dangerous malady that has fastened on the brain.

Some data by which to estimate the relation existing between disorder of the sexual system and the occurrence of cephalalgia are contained in the following numbers:—I have examined the records of 172 cases of disease occurring in females, chiefly in the prime of life, who have been under my care during the years 1850—1853, at the female Invalid Asylum, at Stoke Newington, the patients being mainly servants, or persons in easy circumstances. In 46 I find that there was either disordered menstruation or some other manifest lesion of the sexual organs, or cephalalgia. They may be arranged as follows:—

There was evidence of uterine or ovarian disturbance in 39; in 7 there was no such evidence. The number of cases in which cephalalgia is spoken of as a prominent symptom is 26; while in 20 no mention is made of this symptom. Among the 39 cases of disordered function, 16 were cases of amenorrhœa of greater or less duration. Of these 16, 9 were accompanied by headache; in 7 no mention is

made of headache. I am quite aware that the numbers are not sufficient to establish a law, but they serve in a definite manner to corroborate my general impressions as to the relation between the sexual system and the painful symptoms to which I have ventured thus cursorily to solicit your attention. Much yet remains to be said, both in regard to diagnosis and to treatment, not only of the third class but of the previous ones alone. The more we direct our attention to a subject, the more we become conscious of the difficulties that surround it, and of numerous points of view under which it may be regarded. I feel that I have already trespassed too far on your kind forbearance, and while I should be happy to receive your encouragement at some future meeting again to treat of individual parts of this and allied subjects, I entreat that you will not criticise my present performance as one claiming the merit of completeness; or in fact any merit but the one which I should indeed be sorry not to be conscious of possessing in common with all members of this Society, earnestness of purpose and candour.—*Med. Times and Gazette, Aug.* 12 *and* 26, 1854, *pp.* 156, 208.

144.—ON THE REMEDIAL AND ANÆSTHETIC USES OF INTENSE COLD.

By Dr. James Arnott, London.

[The following paper on this subject is one of the best we have seen, and shows the various cases to which this remedy is applicable.]

As no remedy has been longer in use, and few are more valued than the local application of moderate degrees of cold, or a temperature ranging from that of dissolving ice to about 70° of Fahrenheit, it may at first appear singular that a greater or more powerful remedial effect should not have been sought by increasing the dose of the agent, or employing a lower temperature, in the same manner as we have sought and found much greater remedial benefit in many cases by using mercury, antimony, quinine, and other drugs, in larger doses than had been customary. The reason is that medical men were under a most erroneous impression respecting the effects of very low temperatures on the body. Because a temperature of zero stops the circulation, and because the vitality of a part has been lost by its *long-continued* congelation, whether caused by exposure to severe cold in winter, or by the incautious use of ice in hernia and other diseases, it was hastily and erroneously inferred that there was danger of loss of vitality from *short-continued* congelation. The mistake would not be greater to infer from the fact, because a long-continued stoppage of the circulation through a limb from an improper application of a bandage has occasioned gangrene, that it would be dangerous to use the tourniquet in operations.

The correction of this error will be deemed of no little importance when it is considered that in short-continued congelation, judiciously applied, we have an unfailing means of immediately arresting inflammation wherever it can be reached by the remedy; of not only giving speedy relief from pain in many diseases, but in consequence of the organic changes produced by it, of obviating the return of pain; and in malignant disease, of producing an amount of benefit much exceeding that yet accomplished by other means. Although much inferior in importance to these results, it is yet another great benefit conferred by intense cold, that the pain which would be otherwise caused by the greater number of surgical operations can be prevented by it with perfect safety; and not only can pain be prevented, but the inflammation proceeding from the surgeon's knife, that so often proves fatal, may also be obviated by the same means, and with almost equal certainty. It will be proper to consider the remedial and anæsthetic effects of intense cold separately; but before doing so, it is necessary to mention how this degree of cold is produced and applied, as well as to attempt an explanation of its mode of operation.

That degree of cold may be called intense which immediately benumbs the part to which it is applied, speedily stops the circulation through it, and congeals the adipose matter. I have usually produced these effects by placing what are termed frigorific mixtures, either immediately in contact with the skin or mucous membrane, by means of a net of thin gauze containing them, or by allowing them to act through thin bladders, or metallic vessels of appropriate form; but there are various other

ways of effecting the same object, some of which are preferable for certain purposes. Substances passing rapidly from the solid to the fluid, or from the fluid to the aeriform state, strongly abstract caloric from other bodies in contact with them; and substances, either solid, fluid, or aeriform, already sufficiently cooled by artificial means, may be placed in contact with the part; the first, as solid metallic balls of appropriate shape; the latter two, when forming strong currents. When cold is produced by the common frigorific mixture of ice and salt, and applied by means of a gauze bag or net, the following is a convenient mode of proceeding:—If the congelation is not to be extensive or long-continued, a piece of ice of the size of a large orange will be sufficient. This is well pounded in a coarse cloth or bag, and the powder being placed on a large sheet of paper, is thoroughly mixed, by means of a paper-folder, with about half its weight of common salt. The mixture is then put into a net of about four inches diameter, and as soon as it begins to dissolve, it is ready to be applied. The net is not kept motionless on the part, but is frequently raised in order that fresh particles of the mixture may be brought into contact with the skin; and the water that escapes from it may be absorbed by a sponge, or allowed to fall into a basin placed underneath. If the surface to be acted upon is of small extent, a very thin and large copper spoon containing the mixture, or a solid brass ball of about a pound weight, which has been immersed in ice and salt, will often answer, and be a neater mode than the net.

The moment a gauze net, or a thin metallic vessel containing ice and salt, is applied to the skin, it is benumbed. There is hardly a sensation of cold produced, and no tingling or smarting. If the contact of the frigorific be continued a few seconds longer, the surface becomes suddenly white, in consequence, doubtless, of the arrest of the circulation; and this change of colour is attended with a slight smarting like that produced by mustard. There is now complete anæsthesia, which, if the frigorific were removed, would remain complete for several minutes. But if the frigorific be allowed to act, another change is produced—the adipose matter under the skin is congealed, and the part becomes hard as well as white. The depth to which the benumbing influence of cold will extend depends upon a variety of circumstances, as the degree of cold, the duration of the application, the vascularity of the part, whether pressure is used or the circulation is suspended, &c., &c. After the usual application of cold for anæsthesia, the circulation soon returns to the part, and the skin assumes a red colour which lasts for several hours. If the congelation has been considerable, there is now some smarting felt, unless the natural heat be more gradually restored by pouring cold water on the part, or by placing on it a little pounded ice, or a bladder containing iced water. If the application has not exceeded the first stages, there is no smarting, and no necessity, therefore, for such precautions.

The redness produced does not, as might at first sight be supposed, indicate an inflammatory condition, but the very reverse. The tonicity of the small arteries appears to be lessened or suspended for a time, and instead of being inflamed, the part is rendered unsusceptible of inflammation. Parts cut after congelation heal by adhesion or the first intention more quickly than they otherwise would; and, as has already been said, we possess in this expedient a certain and prompt remedy for every inflammation accessible to its complete influence.

1. *Remedial Uses of Intense Cold.*—The remedial qualities of intense cold may be described as antiphlogistic, anodyne or sedative, and specific; and it is useful in the diseases for which other remedies possessing these qualities have been employed, viz., in inflammatory, painful or irritative, and malignant diseases. The circumstance which limits its application in these, is the impossibility of extending its influence beyond a certain extent or depth, although it is certain, from its effects in a deep seated disease, that this influence, whether it be direct or sympathetic, is more extensive than would at first be supposed. It may be laid down as a rule, that in every case in which the local application of moderate degrees of cold has been found of service, the use of well-regulated congelation would prove much more useful; and in those diseases of similar character, in which moderate cold has not been employed from the idea that their seat was beyond its reach, congelation might be tried with reasonable hope of success. Intense cold has this immense advantage over other powerful remedies of the same class, that it may be used with impunity—if it does no good it will do no harm. Who will venture to affirm this of bleed-

ing, mercury, antimony, opium, chloroform, arsenic? Neither in my own practice, nor (so far as I can learn) in the practice of others, has there been any untoward result from the use of congelation. Its action being confined to the diseased part, and not uselessly expended on the rest of the system, affords the explanation. Other topical remedies have much the same character for safety, but what other expedient of this class has a tenth part of the power of intense cold?

Instead of enumerating the diseases in which this agent has been employed according to the above classification, I shall mention, first, those in which it has been more or less successful; and, secondly, those in which it might, reasoning from analogy, be tried with hope of advantage. In administering intense cold as a remedy, the common or a more powerful frigorific has been generally applied directly to the part, or with the intervention only of the thin gauze containing it; and the duration of the congelation has been from one to ten minutes.

In the spring of the year 1849, I requested the house-surgeon of the Brighton dispensary to apprise me of every case of acute lumbago that came under his notice, and, in all of those, amounting to nine, I employed congelation with perfect and permanent success. The net containing the ice and salt was passed to and fro for five minutes, over a surface of about eight by four inches, the skin being blanched during the whole of this period. In only two or three cases was it necessary to apply the remedy twice. Several of the patients rose immediately afterwards from their beds to which they had long been confined. In most cases of chronic rheumatism, the remedy has been equally successful; and this, on account of the frequency of the disease, is one of its most valuable applications. Sciatica has generally yielded to it, but by no means so easily. In acute rheumatism, the local inflammation of the joints is, by this means, invariably and completely relieved, and that portion of the accompanying fever thence arising, is consequently removed. The disease, thus treated, will run a painless course of about a week's duration. In no case, of about a dozen, in which congelation was almost exclusively employed, was there extension of inflammation to the heart; and I am persuaded that the best plan of preventing this, is to subdue the inflammation of the joints from which it generally originates. I did not use the remedy in cases where the heart was already affected, though I have since learned that congelation is employed in the hospital at Vienna (where it was introduced some years ago by Dr. Waters of Chester), as an application to the chest in rheumatic carditis. That this affection of the heart would occasionally occur during the treatment of acute rheumatism by congelation is very probable, because it often arises, as the same affection of the joint does, from a morbid condition of the blood over which the remedy can have no control; and that such an occurrence, in the present feeling on the subject, would be called metastasis from cold, is very certain; but I am convinced that it will yet be acknowledged, though probably after many years, that this affection would be much decreased in frequency by the adoption of any means capable of quickly subduing the accompanying arthritis. When it is considered what an immense amount of eventual mischief arises from the organic disease of the heart that occurs under the common modes of treating rheumatic fever, to say nothing of the patient's present sufferings and tedious confinement, it is to be lamented that prejudice should oppose any measure of greater promise. In rheumatic gout, the relief has been as marked from congelation as in lumbago. In ordinary inflammation of the joints it has also been exceedingly useful. Ophthalmia has been immediately cured by keeping the frigorific in contact with the gently closed eyelid for three or four minutes. Glandular inflammation in the neck and groin, yield to a high degree of cold with equal facility. I have been told that in orchitis, its beneficial operation is immediate; and I have little doubt that, from its closeness to the surface, the urethral inflammation causing orchitis, would be quickly suppressed. Congelation has often at once converted an irritable into a healing ulcer, though sometimes the patient has complained of the pain of the operation; it is probable that had the salt in the mixture been prevented coming in contact with the irritable surface, this would have been in a great degree prevented. Certain acute inflammatory affections of the skin are equally under its influence, as erysipelas, eczema, impetigo. It has not often failed in prurigo, but in only one case of psoriasis has it appeared to be of service. Painful nodes are at once relieved by this means, and the inflammation subdued. I have only used congelation in carbuncle as an anæsthetic previously to cutting it, but it is probable

(judging from its effect in severe boils) that the incision might have been dispensed with. It has been mentioned to me that severe cold has been employed with the same view in whitlow, of which it is certainly a sufficient cure. The inflammation following sprains, contusions, and other similar injuries, is perfectly under its influence; and the same may be said of burns. In one of my publications on the subject, I have related the excellent and speedy effect of congelation in a case of meningitis, and also in a case of peritonitis; I have not had the opportunity of trying it in other affections of this description. Headache of various kinds has at once yielded to the application, for a minute, of a frigorific over the painful part; and in neuralgia affecting the side, it has generally proved efficacious. In neuralgia attacking the face and other parts, it has often succeeded and often failed. If the seat of the disease be deep in the brain, little can be hoped from this remedy, although there are few obstinate cases of neuralgia in which it does not deserve a trial. Toothache is generally at once relieved by it if properly applied; and there is no remedy for the painful affection of the mouth caused by mercury comparable to congelation. A spoonful of dissolving ice and salt is repeatedly put into the mouth, until it becomes benumbed. In one case of severe scurvy of the gums, where I feared a loss of the teeth, extensive congelation of the gums immediately arrested the disease.

In many of the diseases just enumerated, the promptness of the cure is as remarkable as its certainty. In military and hospital practice this advantage is very prominent.

In cancer the effects of congelation have been various. From my own experience, and that of others, I think that in its early stages, and when from its size the tumour can be thoroughly brought under the influence of the remedy, it will be cured by it. In all stages the progress of cancer will be arrested or retarded, and the pain accompanying it assuaged. The difficulty in advanced cases is to cause a sufficient degree of cold to pervade the tumour. The French translator of a recent paper of mine on the subject ('L'Union Medicale' for May), thinks that the frequent occurrence of cysts in cancerous tumours may facilitate this. But if layer after layer is acted upon, it may be enough. In cancer of the womb the frigorific is applied by means of a speculum, and one stronger than ice and common salt will generally be required. The opinions of Dr. Hughes Bennett respecting the nature of cancer have much influenced the mode in which I have used congelation in its treatment. M. Velpeau states in his recent elaborate work on diseases of the breast, that he has employed *long-continued* congelation as a substitute for caustic in cancer; but of this effect of the agent I have no knowledge.

There are other diseases in the treatment of which severe cold would probably be very useful. It might be applied with such a hope to the spine in tetanus, or to the scalp in certain varieties of mania. After gunshot, and other severe wounds, it would prove a powerful preventive and cure of inflammation. Even in pleuritis, and other deep-seated inflammation of the chest, as well as in various uterine affections, benefit might rationally be expected from it. In two cases of epidemic cholera, I administered a succession of draughts of a temperature of about 25° Fahr., with apparently excellent effect; and I cannot doubt that the application of cold to the interior of the stomach which, as appears by the recently published report of the College of Physicians, is the only treatment of cholera which has been unanimously approved of, has not been carried far enough. If the irritation of the mucous membrane be considerable (as it must be to account for the exhausting and fatal discharges), the temperature of ice merely, is not sufficient to subdue it.—*Monthly Journal of Medical Science, July*, 1854, *p.* 33.

145.—TREATMENT OF CANCER BY CONGELATION.

By Dr. James Arnott, London.

[Some years ago Dr. Arnott directed our attention to this subject, so that it is by no means a novelty; indeed, Dr. Arnott himself confessed that he has only been a means of bringing this remedy into more general notice.]

Having ascertained the important facts that the circulation of blood in a morbid part may be temporarily suspended by intense cold, without in the slightest degree

endangering the vitality of the part, and that such a suspension and other concomitant effects of this degree of cold, are highly curative in inflammatory and neuralgic affections, I merely applied it in cancer to arrest the inflammation accompanying the disease, on which the rapidity of its progress, and many of the most distressing consequences, depend, and at the same time to assuage the pain by its permanently benumbing or narcotic property. I at first expected only to find a substitute for the very inefficient and otherwise objectionable remedies of inflammation and pain in common use in cancer; and had congelation only fulfilled these indications, it would have been very valuable; but experience has shown that it has still more powerful effects in this disease, although, from the unknown nature of cancer, it is as difficult to account for these as it is to explain how the exhibition of bark or quinine cures an ague. Professor Bennett, of Edinburgh, expresses (in his able work on Cancer) the opinion, that "were it possible to bring down the temperature of an entire cancerous growth below the vegetating point, we must inevitably kill it;" and it is not improbable that to such destruction of the vitality of the cancer-cells—to the killing of these parasitic animalcules—the curative influence of congelation may be chiefly due. But however satisfactory it might be to ascertain the mode of operation of the remedy—whether it acts in this manner, or by some unknown change produced in the functions of the vessels or nerves of the part, in addition to its obvious power of suppressing inflammation and assuaging pain—the chief point is to know whether it has great control over cancer, and this can be ascertained only by experience.

[Insensibility to pain cannot be expected to be caused by Dr. Arnott's frigorific mixture beyond a certain depth of texture; but it is supposed the effects of the cold will extend much deeper. *Short* congelation may be quite sufficient, in many cases, to destroy the vitality of diseased parts without endangering the life of contiguous structures. Dr. Arnott brings forward the following case of scirrhous cancer of the breast to illustrate his practice. There seems to be no doubt that it was a case of cancer. Mr. Lawrence himself had so called it.]

On examining the breast, I found a hard, flattened hemispherical swelling, of about three inches diameter, knotted on its surface, contiguous with, but not adhering to the skin, excepting at the nipple, which was retracted and slightly ulcerated. There was at times a lancinating pain. The disease had existed more than two years, and although the usual routine had been had recourse to, no kind of treatment had appeared to be of any service. The disease gradually but steadily progressed.

The frigorific mixture of ice and salt was applied for about four minutes, the usual precautions being taken to prevent the smarting that would otherwise take place on the return of sensibility to the parts which had been congealed. A similar application was repeated about every month by her medical attendant in the country; and after about six such applications I again received a visit. The tumour appeared to be smaller than when I first saw it, the decrease being chiefly in its thickness; and in other respects there was great improvement. She continued the same plan of treatment, and the principal results are recorded in the following extracts from a letter which I have lately received from her, dated April 6th:—

The substance of this letter is, that the tumour continues of nearly the same dimensions; though it appears to be a little longer, it is less thick. She has not "for the last four months, known what a bad night is, being always free from pain;" though during the day there are, "at times, three or four, or perhaps more, transient pains, while, at other times, she passes some days without any pain." Her "general health is very good, and is kept good by regular exercise in the open air." As I had expressed the opinion that she should make longer intervals between the applications than a month, in order to ascertain whether the tumour was not now merely a lifeless mass, like a bullet in the flesh, which might give occasional uneasiness, particularly when the mind was intent on the subject, she states in reply—"Five weeks have intervened between the last applications; I have these renewed, because, while I feel there is life in the tumour, I think they are necessary." She concludes a letter written a month previously (March 4th) by the expression of a wish "that every sufferer from the same disease were as happily delivered from the effects of cancer as she has been by this remedy."

As in almost every case which I have treated by congelation, a certain degree of hardness and swelling remained after the disappearance of other symptoms, it is important to investigate the cause of this. On the supposition that cancer is essen-

tially a congeries of living cells, we may reasonably think that the absorption of these, after their vitality has been destroyed, must be slow, if it take place at all; and perhaps the irritation that has induced patients to apply for the frigorific application afresh, may have proceeded from the presence of this inert mass of dead cancer-cells. I should be sorry, however, to think that the absorption of these never takes place, because in a voluminous congeries of cells it were difficult to understand how the cold could reach the inner surface of the mass without the absorption of the more superficial layers having previously taken place after the extinction of the life of the cells constituting them; unless, indeed, the layers of dead cells were to form so good a conductor as scarcely to resist its passage. But in their living state, the tumour in which they are interspersed is so dense and so little intermixed with bloodvessels, as to form a substance easily permeable by cold—as easily, perhaps, as cystic tumours are, the fluid contents of which I have congealed in applying cold to them, as an anæsthetic, previous to their excision.

If it be objected to the cases which I have now related that they do not furnish satisfactory evidence that cancer is completely curable by cold, they show, at least, that it will produce much more benefit in this disease than any other remedy. If an unequivocal cancerous tumour subjected to this treatment, instead of increasing in size, gradually decreases, while the patient becomes free from pain (excepting such uneasiness as a mass of dead cancer-cells might excite), and the general health improves,—if no glandular enlargement takes place in the armpit, or extension of the disease to any other part appears, and if this condition continues without change for a long period, it is surely a very satisfactory change in the patient's condition. I question much if it be possible entirely to remove the hardness in the majority of such cases, but this hardness is no proof of the presence of malignant affection, and no source of inconvenience to the patient. What other remedy yet employed in cancer will effect as much? Too many of the measures employed as remedies only precipitate the unfortunate patient's fate.

Before concluding this paper, I must advert to a statement which appeared some time ago in a medical journal, that a patient had been seriously injured by a prolonged application of ice to her cancerous breast of upwards of twenty-four hours. As every one has not yet learned the essential difference between the application of ice and congelation in disease, it may be advisable now briefly to point it out; and as the subduing of the inflammation usually accompanying cancer is one of the objects in using either of these methods of treatment, it may simplify the matter to speak of this difference as respects their use in preventing or removing the inflammation of wounds after other mechanical injuries. Ice, or a temperature of 32° Fahr., acts as minor degrees of cold do, in lessening the increased vascular action which accompanies (if it does not constitute the chief element of) the morbid condition called inflammation; but congelation, by causing a temporary suspension of the circulation and sensibility of a part, does this and a great deal more. It effects such a change in the vital actions of the part, perhaps by removing for a time the tonicity of the small arteries, as not only immediately to arrest inflammation, but to render the part unsusceptible of this condition for some time afterwards. If this suspension of vitality be of short duration, there is not the least hazard of the part being permanently injured, and when congelation is purposely effected by a powerful frigorific, its duration and extent can be exactly limited. But if it should happen that owing to a debilitated state of the body, or other circumstances, a prolonged application of ice should at length congeal the part, as the surgeon may not be aware of this unintended effect until after it has lasted for a long time (as has happened in applying ice to gun-shot wounds and in strangulated hernia), much injury may be the consequence. Fortunately, the aching usually caused by a continued application of ice is a preventive of such mischief, and another reason why it has been so little employed in surgery.—*Lancet, April* 15 *and May* 6, 1854, *pp.* 415 *and* 489.

146.—*Cold as an Anæsthetic.*—[This agent has been lately tried in several cases in St. Bartholomew's, to prevent the pain in the incisions through the skin, which are often, indeed, the most painful part of the operation. In one case, in private practice,]

Mr. PAGET excised a fatty tumour from over the shoulder of a lady, the skin having previously been frozen; and although the incision required was four inches long, yet no pain was complained of. In proof that congelation does not hinder the subsequent healing, it may be mentioned, that in that instance a considerable part of the wound united by the first intention, and the rest of it soon closed. The mixture used was about equal parts of pounded ice and salt, enclosed in a coarse muslin bag. This was by degrees applied to the surface to be operated on, and, as the patient got used to the sensation, allowed to remain on it. The process occupied from four to six minutes, and caused no pain. Operators who make use of this plan, must recollect that the skin does not cut so crisp as natural when frozen, but like a tough soap, requiring a little modification in the handling of the scalpel. The apparatus recommended by Dr. Arnott, a gauze bag, a large brass ball, a spoon, &c., is now kept by the instrument-makers; but it is very simple, and may easily be extemporized without cost. It is much to be desired, that the extent of practical use which may be made of cold as a local anæsthetic should at once be tested, since should it prove to be generally applicable, it would prevent, on a large scale, the risks incident to the inhalation of chloroform. In a very great majority of the cases for which the latter agent is now used, the cutting of the skin is almost the only painful part of the operation.—*Med. Times and Gazette, July* 1, 1854, *p.* 11.

[The following case, by NATHANIEL WARD, Esq., illustrates the value of Dr. Arnott's views on the effect of this anæsthetic agent:]

I was consulted a few days back by a gentleman between 30 and 40 years of age, of a highly nervous temperament, concerning a tumour situated over the right clavicle, and which required removal. It was just one of those cases in which a surgeon, on the one hand, would not have sanctioned the use of chloroform, and on the other, in which the patient would have protested against anything being done unless it could be accomplished without pain. The tumour was of a sebaceous character, as large as a walnut, had been gradually increasing for two or three years, and gave him inconvenience during every movement of the arm. On informing my patient that chloroform (to which he was much averse) would be attended with risk, but that the removal of the tumour could be effected with safety and without pain by the previous application of cold, his nervous anxiety subsided, and he consented to the operation.

I mixed together two parts of pounded Wenham-lake ice and one part of salt, and put them in a common white pocket-handkerchief, and kept the mixture pressed on and around the tumour during the space of one minute by the watch. The integument that was submitted directly to the action of the cold became remarkably corrugated. It was then cut into, and the tumour removed without the slightest sensation of pain, and much to the astonishment and delight of the patient, who said that the only thing that annoyed him, and that not much, was the burning sensation of the application. No vessel required ligature, the bleeding, in fact, being very trivial, and the wound had healed at the end of the week.—*Med. Times and Gazette, Sept.* 2, 1854, *p.* 248.

[It appears that cold does not always answer the expectation. Two cases of failure are recorded below. The first case was under the care of Mr. WALTON, at St. Mary's Hospital.]

The tumour was subcutaneous, and felt quite as loose as such tumours generally are; it had a size of about an adult fist, somewhat flattened. Nearly an hour was wasted in unsuccessful attempts to freeze the skin, but as this was due, of course, to mistakes in manipulation, it should not be charged against the process. At length a mixture, properly made, was applied, and in about four minutes the requisite area of skin was frozen, as white and hard as could be wished. Without the loss of a moment's time, Mr. Walton made a deep incision through the whole required extent of skin into the tumour. This gave no pain. The tumour was seized at once, and forcible enucleation attempted. It could not, however, be extracted so easily as had been expected, and adhesions, both to the skin and to the deeper parts, required to be divided by the knife. At one part, where it appeared to have been pressed upon by the edge of the woman's stays, the adhesions between the tumour and the skin were very close, and a careful division was needed. The operation lasted perhaps altogether about four minutes, and during the whole of that time,

excepting the first cut in the skin, the patient was making loud cries and protestations of pain. It should be stated, that she was a remarkably quiet person, and one who did not complain for little.

The above operation took place on Wednesday last; and on the Friday following we witnessed an almost similar one in the theatre of the London Hospital. Mr. CRITCHETT's patient was a man of middle age, and the tumour was a fatty one, about the size of a large fist, and situated beneath the skin in the upper part of the front of the thigh. The freezing of the skin was very complete, nearly five minutes had been occupied in the process, and the incision into it appeared to be quite painless. The tumour had, however, rather intimate adhesions, especially to the integuments; and the man complained much of almost every touch of the knife excepting the first.

We had witnessed before the above several cases of partial failure in the case of cold, but were inclined to attribute them somewhat to timorousness in its use; in these, however, it was fairly and sufficiently used. Their evidence seems clear to the effect, that, unless the tumour be so loose, that almost instantaneous enucleation can be performed, a painless operation must not be expected. The anæsthesia does not extend at all deeper than the skin; and even in it recovery of sensibility is so rapid during the manipulations, that the division of adhesions to its under surface will not be painless unless made without a minute's delay. There are, doubtless, large numbers of cases in which, despite these drawbacks, anæsthesia by cold may be made very useful; but the surgeon must always be careful not to promise to his patient a painless operation. As it regards the excision of tumours, it will probably, in a few instances, be completely successful, and in many others sufficiently so to afford a good pretext for avoiding the use of chloroform. It is, perhaps, adapted best of all for use in the very painful operations which it is so frequently necessary to perform on the fingers and toes. Here it can be applied from several sides at once, and a more complete and less transitory degree of anæsthesia produced.—*Med. Times and Gazette, Sept.* 30, 1854, *p.* 342.

[Dr Arnott justly remarks that it has never been asserted that the effects of cold could extend very deeply; but that as the division of the skin is one very painful part of every operation, this pain may be saved in almost all cases, and in some operations this is all that is required.—*Med. Times and Gazette, Oct.* 7, 1854, *p.* 379.

147.—*Local Anæsthesia by means of Dr. Hardy's Instrument.*—M. NELATON employed this instrument at the Clinique, upon a girl who was suffering from abscess in the axilla, and from a small wound upon the back of the wrist, both extremely painful. The first application of the chloroform vapour on the tumour in the axilla produced an insensibility which lasted three hours, during which time the part could be handled and examined with impunity. After the second application, the abscess was opened; the patient declared she was not conscious of any pain from the operation, and became aware of the fact that it was over only by touching the part with her hand. From this time she had no more pain. The little wound on the wrist fumigated in the same manner, remained quite insensible.—*Gaz. des Hôpitaux.*—*Dublin Hospital Gazette, April* 1, 1854, *p.* 80.

148.—ON THE PATHOLOGY AND TREATMENT OF CHOLERA.

By DR. J. ROSE CORMACK.

[One of the most excellent papers on this subject is by Dr. Cormack, Editor of the 'Association Medical Journal,' from which we extract the following.]

1. Cholera is a fever, intimately related to those fevers which depend on malaria.
2. The intermittent or remittent type can be generally recognised in the milder, and also not unfrequently (though less distinctly) in the severer cases.
3. The stage of collapse ought to be considered as an aggravated cold stage of the paroxysm of a pernicious fever which may spontaneously terminate in death or reaction.

4. The least dense portion of the blood has an excessive tendency to exude through the capillaries of the stomach and bowels, and pass from the body by vomit and stool.
5. The inspissated residual blood being unable to pass through the small pulmonary vessels, causes congestion of the lungs; and as speedy consequences of this condition, paralysis of the right side of the heart from over-distension, asphyxia, and other subordinate derangements of the vital actions.
6. Death may take place from
   a. Asphyxia.
   b. Necræmia, *with* loss of the least dense portion of the blood by stool and vomit.
   c. Necræmia *without* such loss of the least dense portion of blood as can be discovered during life—the exudation remaining within the stomach and intestines.
   d. Toxæmia from absence or deficiency of sanguineous depuration.
   e. Inflammation of lungs or other organs supervening in convalescence.
   f. Debility.
   g. Gastro-enteritis.
   h. Two or more of the above causes combined.
7. The anatomical lesions found on dissection vary with the causes of, and circumstances attending, death.

Treatment of Cholera.—The advocacy, by many, of a uniform and empirical system of treatment of cholera, has greatly tended to obstruct the progress of rational inquiry. Several plans, and particular remedies have been found useful; and practitioners, impressed by the published account of this success, have too often contented themselves with empirically repeating the instructions of others, without carefully analysing the facts and seeking for a rational explanation of the *modus operandi* of the therapeutical agents which they employ. The stage of the disease has not been sufficiently noted in relation to the therapeutic means employed; and recoveries have been too largely designated cures. With one, capsicum has been the specific; with another, camphor; with another, sulphuric acid; with another, acetate of lead; with another, quinine; and so on might the list be indefinitely extended. Now, that all of these and other medicines are *par excellence* cholera remedies, I perfectly believe; and it seems to me that a more successful treatment is to be discovered by a judicious application of the means which we possess, than by searching for some new specific.

It seems to be of primary importance to bear in mind that we have a poison-disease to deal with, resembling, if not indeed identical with a pernicious ague; that it has a course to run, which may be modified and curtailed, but which, even when let alone, shews a disposition to terminate in recovery. We have, therefore, in the first stage, in which chills and other premonitory symptoms of discomfort may be present, to adopt measures which may be regarded as prophylactic rather than curative. It is in this stage that the pernitrate, or some other preparation of iron, and the disulphate of quinine, are of signal benefit. Here, however, we must not follow a blind empiricism. We must, if the digestive system is at fault, combine with the use of these special medicines a judicious alterative system; and, should there be any tendency to copious watery evacuations—the serum of the blood—we must be prompt in our administration of those remedies which are generally termed hæmostatics; among which may be particularly mentioned quinine, sulphuric acid, nitrous acid, acetate of lead, creasote, and nitrate of silver. If the case should proceed a little farther, in spite of our endeavours to arrest its progress, or should the case, from its inherent intensity, or from neglect, not present itself for medical treatment till the secondary effects of the loss of serum have become apparent, we may probably find it useless to think of the quinine, and be obliged to treat the cramps and collapse which threaten speedy dissolution. The necessity for diffusible stimulants is now apparent: and, of them all, as a general rule, camphor will be found the most useful. It can be conveniently administered in large doses by dissolving it in chloroform; and the solution combines so well with creasote, that it will often be found prudent to confine our administration of medicine to frequent doses of a mixture of camphor, chloroform, and creasote. The chloroform is useful as affording facilities in dispensing: and it cannot, in the small quantities administered, do any harm. The camphor acts quickly as a diffusible stimulant;

and the creasote has a powerful effect in restraining the serous discharge. Indeed, I am inclined to think that its beneficial effects are not much, if at all inferior in this respect to those of sulphuric and nitrous acids. External warmth, and the use of stimulating embrocations, are very beneficial in conjunction with the internal use of camphor. The suppression of urine is not a symptom which is to be relieved by the administration of diuretics; it is a necessary consequence of the congestive paroxysm of the disease, and its removal is likely to follow the cessation of that paroxysm.

Enormous doses of opium, of calomel, and of other powerful drugs, have been given to cholera patients; and there can be no doubt that such substances, when introduced into the system when in a state of collapse, are not likely to produce any very suddenly appalling effects. And indeed, in a vast number of cases, they seem to lie as quietly in the stomach of the cold cholera patient, and to produce as little effect as if they were deposited in a glass bottle. Should the patient, however, emerge from the state of collapse, he runs as great a risk from the poisonous doses of these medicines which have been placed in his interior, as he has just escaped from the pestilence. Large quantities may certainly pass off by the bowels, unacted upon; but it cannot be questioned that in a very great number of cases which have been treated in the way mentioned, patients have had their convalescence abruptly arrested by fatal narcotism or exhausting salivation. The use of opium and calomel, in ordinary doses, is often necessary: but the inordinate doses of these medicines formerly, and perhaps still, employed by some, cannot be too much condemned.

The importance of arresting the serous discharge, and of maintaining the warmth of the body by the application of heat externally, cannot be over-estimated. In fact, the judicious carrying out of these intentions constitutes the essence of the treatment of cholera. Other therapeutic measures may be regarded as liable to considerable variety, according to circumstances, and as valuable and auxiliary rather than as always indispensable. The prompt arrest of the serous discharge, by creasote, sulphuric acid, nitrate of silver, or other remedy, has undoubtedly saved innumerable lives; and from amid the chaos of contradiction, depreciation, and laudation, in which the merits of special modes of treatening cholera are involved, the diligent application of warmth to the surface can be extricated as a measure of established value.

Mr. Barwell, in a little work just published, says: "It is a grand essential to keep up the temperature of the patient, since the tendency to become cold is certainly a great characteristic of this disease. Hot bottles should be placed to the feet, and inside of the thighs; and India-rubber-bags filled with hot water to the loins and abdomen. The bed-clothes must be ample, and should be so arranged, by means of an extra blanket wrapped round the shoulders, or passing round the chest and under the arms, that he does not bare that part to the cold in his restless jactitations, nor in rising on the elbow to vomit, as he may perhaps frequently be obliged to do." These recommendations of Mr. Barwell are sound and practical. They are, moreover, firmly base upon facts which came under his observation in St. Thomas's Hospital. In the following passage, he states very clearly, and, I think, very correctly, the relative value of internal stimulants and external warmth in cholera; and he also refers to the hospital experience already referred to, upon which his opinions are founded.

"Stimulants," says Mr. Barwell, "such as brandy, ammonia, or wine, though decidedly useful in their place, have not such effect in restoring circulation, and exciting the system to greater action, as in collapse from other disease; indeed, considering the difference of its cause in this and other maladies, it is not to be expected that they would be as beneficial; for prostration usually occurs in consequence of nervous shock, and consequent loss of nervous power; therefore stimuli which act upon that system are naturally, in those cases, such as would benefit. But in this disease there is comparatively little loss of nervous power; in fact, with so great disturbance of the circulation, the retention of nervous power is marvellous. Our remedies ought not, therefore, to be directed through that system, but we should, if possible, find some means of acting on and recalling the circulation, without exciting the nervous centres; and the best mode of doing this is by external heat. This principle of combating the deadly cold collapse was not found or recognised at St.

Thomas's until after several cases had been treated at the hospital, and the general inefficacy of medicines or of stimulants proved. Though a certain number under the treatment then adopted recovered, still the whole result was unsatisfactory; thus, of twenty-eight cases of perfect collapse, before external heat was used, seven only recovered—a very small proportion; but, after this was employed, sixty-one patients were treated by some mode in which this formed an essential part; and of these, twenty-seven recovered, or not very far from half the whole number."

Did space permit, I could adduce much evidence of the same description, in addition to that which has now been quoted. In fact, I could show that the success which many have ascribed to favourite pharmaceutical nostrums, ought, with much greater probability, if not with absolute certainty, to be ascribed to the external warmth employed along with the drugs.

Having now glanced at the general principles upon which medicines ought to be employed in the treatment of cholera, I would now remark, that I have not enumerated every drug which may be usefully employed, and have selected those with the operation of which I am personally most familiar. This much, however, may be added; that they are good types of the respective classes of remedies to which they belong.

The formulæ to be adopted must of course be varied in accordance with the circumstances of each case; and it is by a ready power of modifying these formulæ, that the skill of the practitioner is displayed. The formulæ cannot be too simple. No therapeutic advantages flow from the multiplicity of ingredients; and by administering remedies for the purposes of clinical study, as well as of cure, it is obviously necessary not to give more than one medicine, or one class of medicines at the same time.

The following medicines possess in a high degree the power of arresting the serous diarrhœa, which generally precedes collapse by a good many hours, and which is the immediate cause of that collapse as well as of the cramps:

Creasote;
Turpentine;
Sulphuric acid;
Nitrous acid and nitro-sulphuric acid;
Nitrate of silver;
Quinine;
Gallic acid;
Alum; and
Acetate of lead.

1. *Creasote.* Some patients refuse to take creasote, from a dislike to its odour; but if one or two doses of two or three drops can be taken every hour or every two hours, in mucilage, I prefer it to any other means of cure in serous purging. It hardly ever fails.

2. *Turpentine* may be used in place of creasote, in doses of ten minims. I have in several cases of diarrhœa found it quite successful.

3. *Sulphuric Acid.* The use of this acid in diarrhœa is by no means new. The late Dr. Anthony Todd Thompson, in his 'Dispensatory' (Edit. 1837, p. 762), wrote as follows:—"When combined with mucilages, it has been beneficially given in passive diarrhœa, operating on the relaxed nervous coat of the intestine as an astringent. The usual dose is from ten to thirty minims, *but this dose may be very often repeated.*" In later years, its use has been revived: and several writers in the various medical periodicals have spoken in high terms of its efficacy. Among those I may mention Mr. W. I. Cox, of Kensal Town, who has also employed it in cholera ('Lancet,' for August, September, and October, 1849, &c.); Mr. W. Griffith, of Eaton Square ('Ibid.,' Oct. 4th, 1851); Dr. G. B. Payne; Dr. Miller, of Stoke Newington; Mr. Edgar Sheppard, of Enfield ('Provincial Medical and Surgical Journal,' for September 15th, 1852, and 'Association Journal' for March 18th, 1853); and several other practitioners. To Mr. W. I. Cox is due, as far as I am aware, the merit of having first employed sulphuric acid in the treatment of *cholera.* In a tabular view of the result of treatment of ninety cases given by him in the 'Lancet' for January 26th, 1850, five are mentioned as having been treated with sulphuric acid, generally in combination with Dr. Ayre's method. Four of the cases

so treated recovered; but we can scarcely draw a conclusion from so small a number.

Dr. Fuller, of St. George's Hospital, in a paper lately published, see *Retrospect*, vol. xxviii., p. 405, speaks in high terms of the efficacy of sulphuric acid in arresting diarrhœa. In bilious diarrhœa, and in certain chronic diarrhœas, he says it is of little or no avail; but in epidemic diarrhœa, in "acute autumnal diarrhœa," and in more decided choleraic diarrhœa, he has known no single instance of its failure. He gives it in doses of half a drachm mixed with water, every twenty minutes or oftener; and the effects produced are described as remarkable; heat returns to the extremities; the nausea and vomiting cease; the purging is stayed; the cramps subside; perspirations generally break out; the tongue becomes moist and slightly coated; the intestinal evacuations become healthy; and the pulse regains its normal steadiness.

In epidemic cholera, Dr. Fuller has had no experience in the use of this remedy. He would give it in doses of ℥ xl. or ʒj. five or six times in an hour, simply mixed with water; and he is opposed to the employment at the same time of brandy or any strong flavoured stimulant or carminative. Calomel, if it is thought necessary, may be given at the same time with, or after the acid. Mustard, bran, or other warm poultices or fomentations may be applied; they usually afford relief. From the prompt action of sulphuric acid in arresting diarrhœa, Dr. Fuller expresses great hopes as to its superiority in cholera over other medicines which require a longer time for their action.

4. *Nitrous Acid and Nitro-Sulphuric Acid.* Mr. W. J. Anderson, in the 'Association Journal' of November 4, 1853, recommends a combination of nitrous acid and sulphuric acid. He says: "Can a remedy be found which will readily yield up its oxygen, and supply that element to the impure blood: and at the same time, by its astringent properties, tend to check the enormous exudation which takes place from the mucous surface of the intestinal canal? In our present state of knowledge, some of the mineral acids appear to be the best adapted to this purpose; and, for certain reasons about to be explained, a combination of nitric with sulphuric acid seems to me to be preferable to any other. The acid should be administered in tolerably full doses, and repeated at intervals varying according to the nature and urgency of the case. For an adult, we may give acidi sulphurici diluti f. ʒij, acidi nitrici diluti f. ʒj in a six ounce mixture, one ounce being the dose for an adult.

Mr. Hope, of Chatham, in the 'Edin. Med. and Surg. Journal' of July 1826, recommended nitrous acid in cholera. He spoke strongly of the efficacy of the following mixture:

℞. Acidi nitrosi ʒj.: misturæ camphoræ ℥ viij. Misce, et adde tincturæ opii ♍ij.

Of this one-fourth is to be taken every three or four hours. Mr. Whiteman, of Putney, informs me that he finds nitrous acid so prompt and so satisfactory a remedy in epidemic diarrhœa, that he trusts to it in preference to every other medicine. He often combines with it a little laudanum, but in the majority of cases, he uses the acid alone. I have not used nitrous acid in diarrhœa or cholera; but I have no doubt as to its value in arresting the discharges.

5. *Nitrate of Silver*, I have not given in cholera and diarrhœa, except in the form of enema. It is valuable in this form. My experience is too limited to enable me to compare the efficiency of enemata of nitrate of silver with enemata of acetate of lead, and of alum.

Dr. Lever and Dr. Aitkin are the principal English writers who advocate the use of nitrate of silver in cholera and diarrhœa. On the continent, the authorities by whom it is recommended are Hirsch, Canstatt, Boudin, Bouchardat, and Trousseau. Dr. Charles Lever, in 1832, extolled the nitrate of silver as a remedy in cholera. In a forlorn case, he administered thirty grains dissolved in three ounces of distilled water, which the patient swallowed at once. She lay quiet for six minutes, when she vomited a small quantity of whitish turbid fluid. She had no return of the vomiting, fell asleep, and recovered ('Johnson's Medico-Chirurgical Review,' Oct., 1834, p. 444). I give ten grains in half a pint of water as an enema. Hirsch prescribed it in the form of mixture; and I am sure that it is a safe medicine in this form, as I have given it internally in other diseases, dissolved in abundance of water. The danger lies in giving the medicine in too concentrated a form.

6. *Quinine.* Of the power of quinine to check epidemic diarrhœa, and to thus arrest cholera in what may be regarded as its first stage, I feel well assured, from an extensive use of the remedy. It is true that I have generally combined it with sulphuric acid, and with iron, and sometimes with both; so that my facts are not available for the purpose of accurately determining the value of quinine given alone. Dr. James Bird and others have, however, from ampler data, arrived at the same conclusion. Dr. Mandl, in the 'Gazette Medicale,' of Oct. 29th, p. 682, speaking of the importance of checking the diarrhœa, which he has generally found in the initiatory stage of cholera, says, "the most powerful means of arresting epidemic diarrhœa, is by administering disulphate of quinine in doses of ten centigrammes (a grain and a half) every two hours." He says that he has seen cases of cholerine, in which opium and injections had failed to arrest the discharge from the bowels, cured by quinine in twenty-four hours. Dr. Charles Bell, who has written so ably in favour of the doctrine that cholera is a fever of a remittent or intermittent type, naturally advises quinine. He says, "If asked what I should do in a case of cholera, I should answer, that that depended very much on the stage in which I found my patient; but, in all cases and in all stages, I should certainly give a largely diluted solution of some salts of iron and quinine, with a view to counteract the morbid inactivity of capillary congestion, and repeat it as often as the patient would drink of it, till warmth was restored." The practice is, there can be little doubt, correct; but it is equally probable that it is a method well calculated to stop the diarrhœa, which Dr. Bell strangely considers salutary. He makes this dangerous announcement:—"Diarrhœa is a natural mode of relief to congestion, and thereby tends to the prolongation of life." The hemorrhage from the stomach thrown up as black vomit in yellow fever, is nature's mode of relieving congestion; but it is not a salutary hemorrhage. Within certain limits, a serous or a sero-sanguineous discharge may be harmless, or even useful; but inasmuch as it is physically impossible for the blood to circulate when it has been deprived of its serum, so is it imperative upon the practitioner to restrain the serous evacuations of cholera.

7 and 8. *Gallic Acid and Alum.* I have often used these remedies in the diarrhœa of phthisis, and occasionally in epidemic cholerine. They are less to be relied on as means of arresting serous diarrhœa than creasote and the mineral acids. They are not superior, and hardly equal to logwood and catechu, as mere astringents; but I know that some authors regard them as especially the astringents to be used in cholera and serous diarrhœa.

9. *Acetate of Lead* may be used internally alone, or in combination with opium. It is generally a prompt astringent when used in the form of enema.

I have not space to enter upon a consideration of all of several reasonable methods of treatment which have attracted the attention of the profession, and deserve to be spoken of with respect. I can only notice one of them at present.

The *Sulphur* plan, as advocated by Mr. Grove, of Wandsworth, has evidence in its favour, though I do not think that the theoretical ground—the fungus theory of cholera—upon which it is advocated, is tenable. Mr. Grove uses the following formula:—

℞ Sulphuris precipitati 3j; sodæ bicarbonatis 3j.; sp. lavandulæ compositi 3vj; aquæ q. s. ut fiat mistura ℥ iij.

A teaspoonful of this is taken every half hour or every quarter of an hour. Mr. Grove informs me that the effect of the medicine is to restore warmth, and promptly to check the serous discharges.

I believe Mr. Blacklock was the first author who recommended sulphur as a remedy for cholera. Although I have no personal experience of its virtues, I may quote, in connexion with this subject, the following passage from a letter dated Sept. 22, 1849, which I received from a non-medical friend residing in Edinburgh:—

"I saw a man to-day from the south muirs, who told me that his wife and he, as well as many of the people of his remote landward parish, had been very unwell; and, from the symptoms which he mentioned, it was clear that they had suffered from cholera, and had been saved from it. They had no medical man near them, and got no medical attendance; yet there were no deaths. As soon as they found cramps come over them, they took a teaspoonful of powdered brimstone, or sometimes the flowers of sulphur, mixed with a little whiskey, to which was added water, if the sick could not otherwise swallow the dose. The man described the cure as

certain and very rapid. Try this on the London folks; it may serve the afflicted, and do you much good."

Mitchell says that, "the sulphureous localities of the sickly island of St. Lucia are its only salubrious places. Cities, too, which abound in sulphur products ..... enjoy an immunity from ague, for which they are everywhere noted. Immediately around the sulphur works, and factories for making gunpowder and sulphuric acid, the vegetation and the ague disappear together." To this we may add, that, in 1849, during the bombardment of Rome, that city suffered much less than usual from malarious fevers—a fact which induced Mr. Walker to recommend cannonading, and the discharge of fireworks, as measures likely to ward off, or prevent the spread of, cholera. The rushing of columns of air, caused by the production of a vacuum, is, however, the more probable explanation of the benefits (if any) which resulted from the cannonade. Mr. Blacklock (Madras, 1848) says; "I hope yet to see the day when sulphur, in small quantities, will be regularly issued to every soldier in the field, in India, say forty grains per day, while actually marching, and twenty grains per day, during halts, as a sure way of warding off this terrible disease; and I have a firm belief that sulphur, so employed, will be as effectual in banishing cholera from our armies, as lime-juice has been in eradicating scurvy from our fleets."

Mr. Grove quotes the following curious passage from the 'London Practice of Physic'—a work published in 1692: "In the year 1670, about the autumnal equinox, a world of people here were seized with a most dangerous flux (though without blood), and joined with a cruel vomiting, which presently caused great faintings and a total decay of strength. For the cure of this disease, no evacuation did good; nay, bleeding, vomiting, and purging did hurt; only cordials, and those of the hottest nature, to wit, such as abounded with *spirit and sulphur*, did good." (p. 22.) From this, it would appear that the whiskey and sulphur treatment of the south muirs of Scotland is an old tradition.

Manec administered sulphur in all possible forms, in his treatment of Cholera in the Salpêtrière in 1849.

Sulphur is a stimulant of the capillary circulation, and so is camphor—one of our best remedies against collapse. Perhaps the combination of the sulphur with hydrogen is the cause of the augmented heat in the surface of those who take the former in repeated doses. The characteristically offensive smell of sulphuretted hydrogen gas is sufficient proof that this chemical action does take place. The therapeutic action of sulphur is, however, yet open to investigation.

The treatment of cholera cannot be reduced to any routine formulary, but ought to be adapted to the particular condition of each patient in each stage of the disease.

The principal indications of treatment may be thus summed up:

1. The "rice-water" vomit and purging require to be energetically subdued by quinine, sulphuric and other acids, creasote, nitrate of silver, and such like remedies.
2. In actual and threatened collapse, external warmth, stimulant embrocations, and those internal stimulants which act on the capillaries, are of signal benefit.
3. In reaction, and during convalescence, local inflammations and congestions require to be guarded against or subdued; and rational means must be adopted to restore the secretions of the liver, kidneys, and skin, but particularly of the two former.
4. Lastly, though not of less importance, the character of the fever should be modified, and a repetition of the paroxysm guarded against, by change of air, or by the administration of quinine, which, in the majority of cases, from the existence of anæmia, ought to be conjoined with iron.—*Association Medical Journal, Nov.* 11, 1853, *p.* 990.

149.—*Treatment of Cholera by Galvanism.* By C. Mehnig, Esq.—[Mr. Mehnig calls attention to the remarkable influence of mild continuous currents of electricity in cases of choleraic diarrhœa.]

The symptoms were—violent diarrhœa, pains and strong movements in the bowels; in two cases vomiting, and in one case cramping sensations in the legs accompanying the diarrhœa.

The positive disc of one of my portable Electro-Generators was applied to the spine between the shoulders, the negative pole was placed on the abdominal region, and moved higher up and lower down according to the varying seat of the pains and coldness in the bowels. The effect of the current was perceived at once, in a feeling of warmth and tone in the bowels, and the pains disappeared in about half an hour entirely; but the diarrhœa and vomiting subsided only gradually, in the course of from two to five hours, when proportionately slight weakness was left. The urine re-appeared shortly after the application, probably through the action of the current on the urinary organs.

I have witnessed these results, in eight cases, one of them in my own person, in which case I purposely abstained from employing any other remedy whatever. It was found necessary to communicate the current to the abdomen by means of a metallic plate, of sufficient size to cover the whole abdominal region, as the patients complained that the warmth was limited to the exact position of the small disc, and did not sufficiently pervade the whole abdomen. In one or two cases, I observed the remarkable circumstance, that while the diarrhœa lasted the pricking sensation at the negative pole was almost nil, whereas after the disappearance of the complaint, the skin was blistered in about a quarter of an hour by the power of the current.

Now, Sir, I certainly look upon these observations as of sufficient importance at the present juncture to induce physicians to test their validity; the more so, as it can be done without trouble, without precluding the use of other remedies, and as the successful results here stated may very easily be accounted for.

Whatever may be the difference of opinion as to the cause of cholera, and the nature of its symptoms, and consequent treatment, two points will be conceded by most thinking men:—

1. That it is of the utmost importance to keep up the power of the central as well as of the digestive system, so that, if diarrhœa there must be, the other functions are disturbed as little as possible.

2. That in this disease that remedy is the most preferable and certain of effect, which is the most directly applicable, and the most independent of the disturbed digestive functions.

In sending a continuous mild stream of electricity (no interrupted or shock current) from the spine to the abdomen, I vitalize—keep up the power of the whole central and digestive systems, and by keeping in action all functions, I prevent the diarrhœa from taking a virulent and passive character. I do this instantly, independently of digestive functions, strongly or mildly, continuously or intermittingly, at option.

All the effects purporting to be produced by the means generally employed, are produced much more perfectly and instantaneously by this simple application.

However, volumes of reasoning will never supply the test of experience; therefore, let it be tried, and stand upon its own merits; this is all that I call for, and I will, with the greatest pleasure, supply any number of apparatus deemed necessary to decide the question.

With regard to cases of collapse, I cannot speak from experience, never having witnessed such a case. It is, however, a fact, witnessed by me dozens of times, and very easily ascertained, that spasms and cramps of the ordinary description cease in a few minutes under the influence of strong, but continuous (not shock) currents along the spine and contracted limbs; and with regard to the maintenance of irritation in any given part of the body, it will be obvious to every one that no other means at command can bear the most distant comparison with electricity for such purpose.

In cases of collapse, I would fix a generator, one disc on the spine, the other on the abdomen, as above stated, and, in addition, I would send the strong continuous current from a battery, first along the spine, and from the spine along the contracted parts, also through the liver, stomach, and bowels; a few shocks to be used only where it was necessary momentarily to stir up the whole system—in fact to keep up life.—*Med. Times and Gazette, Sept.* 23, 1854, *p.* 324.

150.—*Treatment of Cholera by small and frequent doses of Calomel.* By Dr. AYRE, Hull.—[Dr. Ayre remonstrates with Dr. Gull for his report on cholera drawn up at the request of the College of Physicians. Dr. Gull states his belief that no remedy has yet been found, and that the calomel treatment has failed like the rest. We believe this to be in some measure true; and yet, after all, it is one of the best modes of treatment which we have seen, especially as adopted by Dr. Ayre of Hull. This gentleman gives, in the stage of collapse, one or two grains of calomel every five or ten minutes, with one or two drops of laudanum with the first few doses. He continues the calomel until the collapse materially abates. This treatment, however, must not be classed with the calomel treatment in larger doses and longer intervals. Dr. Ayre never gives stimulants in any form.]—*Lancet, May* 20, 1854, *p.* 536.

151.—*Treatment of Cholera by Yeast and Turpentine.* By L. F. CRUMMEY, Esq., Middlesbro'-on-Tees.—To the congregation of certain symptoms we give the name of typhus; to others, measles; to others, small-pox, &c.; but we know nothing concerning the agent producing them. Just the same may be said of cholera. We believe that the poison which occasions this fearful disease emanates from certain conditions of matter, but beyond this we are totally ignorant. The opinion which I formed of the *nature* of cholera in 1832, all subsequent experience has tended to establish—that it is a *febrile* disease, occurs sporadically as well as epidemically, and is essentially contagious. The *rationale* of *treatment* then adopted I have no reason to relinquish. Experience has induced variety in the means; but the purpose—the expulsion or counteraction of the poison—constitutes the main object of my endeavours to this day. The present alarming outbreak of the disease in this town has supplied the opportunity for so testing the practice as to warrant its recommendation to the profession.

It is a serious question for the medical profession to determine whether the premonitory diarrhœa of cholera should be indiscriminately arrested; and it becomes no less an important public inquiry, seeing the universal administration of what are called "astringents," from the comfortable *ad libitum* brandy and water to the most popular quack specifics. In the belief that the vomiting and purging, so characteristic to cholera, do not constitute the *disease*, but are simply an effort of nature to throw off the poison, I should as soon think of trying to arrest these indiscriminately by astringents as to suppress the skin eruption in measles, small-pox, &c. In all cases of bowel irritation, during the prevalence or apprehension of cholera, I prescribe castor oil, combined with any of the preparations of ammonia. In every stage of real cholera, from the first onset of diarrhœa to the very verge of collapse, I give either castor oil and oil of turpentine, or the latter with yeast. I never saw an instance where immediate relief to the symptoms did not follow, and scarcely one where the stage of collapse supervened—nay, even the last fatal prostration has in some remarkable instances during the present epidemic been changed into reaction and recovery by the turpentine and yeast. But from a knowledge of the potent effects of yeast in malignant typhus, small-pox, &c., I was first induced to try it in cholera, combined with the turpentine; and certainly, if fresh brewer's yeast could always be obtained, I should use no other remedy than the following combination: —Yeast, a table-spoonful; oil of turpentine, a teaspoonful; cold water or barley-water, two tablespoonfuls: the dose to be repeated every second, third, or fourth hour, according to circumstances.—*Lancet, Aug.* 26, 1854, *p.* 178.

152.—*Treatment of Cholera by Oxygen Gas.* By Dr. THOMAS O'BRIEN, Calcutta.—[Dr. O'Brien strongly recommends the use of oxygen gas in this disease. Writing from Calcutta, he says:]—This awful scourge is always here, but the people do not at all think it contagious, and natives attacked by it are received into any of the hospitals, and placed with all classes of patients, either medical or surgical. The saline treatment seems to have obtained here, but I cannot say much for its success. I never give opium. I give calomel freely, combined with camphor. The mixture I give is, sp. vin. camphorat., creasote, brandy, and soda-water. I have always given oxygen gas, which I did when in the Galway union, where this treatment was equally

successful, as you can see by my reports to the Board of Health. This gas is easily given by shaking powdered chlorate of potash on a heated surface, and making the patient inhale the fumes. When the vomiting is severe, I have found firing over the stomach and abdomen avert it, and produce heat; but the oxygen gas is that upon which I would place most reliance. Transfusion has been often tried, but without success. Should you have an opportunity of trying the gas, will you do so? and let me know the result. I always give as much cold water as can be swallowed, and where the evacuations are copious, I have injected chicken broth with O'Beirne's tube; this has sometimes been retained for a considerable time.

[Oxygen gas may be readily generated by intimately mixingc hlorate of potash with one-sixth its weight of black oxide of manganese, and throwing the mixture on an iron shovel heated to dull redness.—*Ed. Dublin Hos. Gaz.*]—*Dublin Hospital Gazette, Feb.* 1, 1854, *p.* 14.

153.—*Treatment of Cholera by Eupatorium Villosum and Nervosum.* By Dr. W. E. Stulo, Dublin.—[The following is from the letter of a friend of Dr. Stulo in Jamaica. It refers to a novel remedy for cholera.]

"I have put up a box of the bitter bush, *Eupatorium*, for this packet; and, should an opportunity offer, I hope it will be tested, as I am satisfied it will be found a safe and sure remedy in cholera. I have given it, with remarkable success, in every stage of the disease, even after collapse, and have not heard of a single death when the remedy has been administered in time, and persevered in. It is used by making a strong decoction by boiling about two drachms in a pint of water, and giving a small teacupful, cold, every half-hour until the symptoms abate. In severe cases it is given alternately with the saline powders recommended by the Board of Health. In simple diarrhœa, one dose is generally found sufficient. The first dose often produces vomiting; in such cases repeat the dose immediately. It is certainly a wonderful remedy, and the effect of it sometimes appears to be magical. I have used two species of it, which appear to be *Eupatorium Villosum* and *E. Nervosum*. That used in St. Ann's is, I believe, the *E. Rigidum*. We have several species here, all of which appear to possess the same active properties, but those I have named appear the best. The box I have sent contains the two species I have used; they are very similar, except that one (which I think the best, *E. Nervosum*,) is inodorous, and that the other possesses a strong fragrance, which we think to be *E. Villosum*. The *E. Odoratum* is also common here."

It is right to state that the writer of the foregoing is not in the medical profession; still his observations on this plant should not on that account be disregarded. We know that several of the *Eupatoria* possess very active properties. Some are emetic and purgative, as our native *E. Cannabinum*; while others are powerfully astringent, or even styptic, as the *Matico E. Glutinosum*. Probably the *E. Nervosum* and *E. Villosum*, and others whose efficacy has thus been stated in cholera, are powerfully astringent, and on that account may have a decided effect in restraining the choleraic diarrhœa, and thus give time for using the other remedies for restoring the heat and vital powers, which, from the continued drain of the fluid part of the blood, would otherwise inevitably end fatally.—*Lancet, Aug.* 5, 1854, *p.* 113.

154.—*Treatment of Cholera by Sulphuric Acid, Strychnia, &c.* By W. Wilkinson, Esq., Sydenham.

Cholera may be divided into three stages—

The first, or preliminary stage, being diarrhœa, painless or otherwise.

The second, diarrhœa continued, motions extremely thin, and devoid of fæcal smell, cramps, &c.

The third being the stage of collapse, with unconsciousness, &c.

The first stage may be generally relieved by the exhibition of a *stimulant* emetic which, by evacuating the stomach, prevents any accumulation of irritative matter there, and by its revulsive power produces a general alterative effect. This is to be followed by the following pills:—Chloride of mercury, five grains; ipecacuanha powder, half a grain; soap and opium pill, ten grains; divided into three pills.

The calomel as well as the ipecacuanha to act upon the secretions, and thus relieve the bowel of any viscid irritative matter; the opium to relieve or prevent pain, nervous irritation, and excitement (so constantly followed by depression); and with the ipecacuanha to produce an effect upon the capillaries of the skin, and thus cause a sympathetic derivative action. This may be followed by an evacuant, consisting of rhubarb and magnesia, with compound cinnamon powder and water.

This, even in the early part of the second stage, will be sufficient sometimes to cure the patient; but if the serous fluid lost be considerable, or if the disease should run into its third stage, it is of importance to administer some astringent which can be depended upon, together with some medicine, so to uphold or stimulate the nervous power, as to prevent entire loss of control over the capillaries of the intestinal mucous membrane.

From my own experience in diarrhœa and some few cases of cholera, I can corroborate the opinions of several members of our profession as to the efficacy of sulphuric acid; therefore I should—in every case in the second stage, after the preliminary treatment, or immediately in the third stage, when from the copious action of the bowels, the preliminary treatment would be unnecessary—prescribe, dilute sulphuric acid, half a fluid drachm; strychnia in acid phosphoric solution, one-sixteenth of a grain. This medicine to be taken as frequently as the urgency of the symptoms might render it necessary. The sulphuric acid may be depended on, I think, as an astringent, and the strychnia would also be of the greatest benefit to uphold or stimulate the nervous power, and thus prevent that loss of control over the capillaries, which seems productive of such infinite mischief.—*Lancet, August 5*, 1854, *p.* 100.

155.—ON THE PATHOLOGY AND TREATMENT OF LARYNGO-TRACHEAL INFLAMMATION.

By Dr. Robert Turner, Keith.

Observation and reflection have impressed me with the belief that a common pathogenic condition pervades all inflammatory affections of the laryngo-tracheal mucous membrane; and that to this condition their leading characteristic, stridulous respiration, as well as their tendency to a fatal termination, is chiefly due; in other words, that the essential element of this class of diseases—of *laryngitis*, acute and chronic, and of *croup*, in all its varieties—is the same. My object in the present communication is to develope those views, and to point out what I conceive to be their application to practice.

The following positions embody a statement of some points in the physiology of the larynx; which, although well ascertained facts in vital dynamics, have not, I believe, obtained a due practical recognition in pathology and therapeutics.

I. *The terminal branches of the superior laryngeal or excitor nerve of the larynx being distributed principally to the mucous membrane of the glottis, a higher degree of sensibility resides there than in any other part of the respiratory tube.*

II. *This sensibility is evinced by closure of the glottis, effected by the muscles in which the motor nerves of the larynx (the recurrent laryngeal and the crico-thyroid branch of the superior laryngeal) terminate.*

III. *It is by this act alone that apnœa, or even stridulous respiration, depending on contraction, can take place—the absence, below the glottis, of muscular fibres capable of acting in such a direction as to diminish calibre, and the unyielding cartilaginous rings in the walls of the tube rendering constriction in that situation physically impossible.*

The vigilance of the *sentinel perdu*, stationed at the approach to the respiratory organs, and the promptitude with which its warnings are responded to, are facts so familiar as to need no illustration; but such an opportunity as occurred to me in the instance related below (Case 1) of estimating the degree of force which may be exerted in this conservative act, is not, I believe, often enjoyed.

Under the term *spasm* the participation of the glottidean contraction in the phenomena of inflammatory affections of the laryngo-tracheal mucous membrane and submucous tissue has been recognised by many systematic writers, from the days of Cullen downwards. It will, however, be my endeavour to show that its true semeiological and pathological import, in these circumstances, has been misappre-

bended or underrated by all. It is unnecessary to notice every allusion that has been made by authors to the (so called) *spasmodic element* of the diseases under consideration, as the sentiments expressed by our latest and most esteemed authorities fairly represent the general state of opinion on the point. Dr. C. J. B. Williams ('Library of Medicine,' art. *Tracheitis*) refers to it as arising from the increased sensibility of the inflamed mucous membrane, from interstitial effusion in the lining of the trachea and larynx, and from the presence of false membrane within the tube. Dr. Cheyne ascribes the difficulty of breathing in laryngitis "partly to spasm caused by inflammation of the membrane" ('Cyclopædia of Practical Medicine,' art. *Laryngitis*); Dr. West ('Lectures on the Diseases of Infancy and Childhood,' 2nd ed., pp. 249-53 *passim*), in speaking of croup, alludes to "that spasmodic condition of the muscles of the glottis which endangers the patient's life more than the mere extent of false membrane in the air-passages," and again to "that spasm of the glottis which the inflammation occasions;" and Dr. Maunsell (Evanson and Maunsell 'On the Management and Diseases of Children,' 5th ed., p. 329) observes, "Every affection of the larynx is subject to exacerbations which partake much of a spasmodic character; and a paroxysm of this nature may occur at an early period of true croup, and destroy the patient's life before there has been time for any very important results of inflammation to be produced."

Other modern authorities of equal mark ignore, or by implication deny the occurrence of this spasmodic condition in the same circumstances; Dr. Copland discussing the value of tracheotomy in croup, without adverting to the spasmodic element at all ('Dictionary of Practical Medicine,' art. *Croup*); and Dr. Watson ('Lectures on the Principles and Practice of Physic,' 3rd ed., vol. 1, pp. 844–6) referring the dyspnœa in this affection to the mechanical obstruction of the air-tube by "preternatural membrane, or serous or mucous or puriform matter," in one class of cases; and, in another, "mainly to thickening of the mucous membrane."

It will, however, be generally conceded that the broad fact of the presence of what has been designated *spasm*, in inflammatory disease of the larynx and trachea, rests on as certain grounds as any other article of belief in pathology not absolutely demonstrable by the researches of the morbid anatomist. A convincing argument in support of it is thus stated by Dr. Williams (*op. citat.*, p. 56): "The share which spasm has in causing the dyspnœa (of croup) may be inferred from the fact that in no case have the air-passages been found so much blocked up by the albuminous secretion as to account for the amount of the obstruction; and in many cases the constriction has appeared greatest where little or no exudation has been found after death." The same inference is plainly deducible from Dr. Cheyne's statement to the effect that, in fatal cases of croup, a space of *three-eighths of an inch* usually exists in the larynx for the transmission of air, although this result of his extensive observation has received a different explanation from Dr. Cheyne himself.

Inflammation of the mucous membrane, then, singly or combined with any or both of its consequences, tumefaction of the tissue of the glottis from interstitial effusion, and fibrinous deposit within the tube, are the morbid conditions held to be capable of inducing contraction of the *rima glottidis*—an opinion in which all who are conversant with the clinical history and the revelations of the scalpel after death, in the diseases under consideration, will readily concur; and this doctrine, viewed in connection with the anatomico-physiological considerations above stated, inevitably leads to the following conclusions:—

I. *In every form, case, and stage of laryngo-tracheal inflammation, when a degree of severity of which stridulous respiration is the token has been attained, there exists a structural lesion which will excite to actim the constrictor muscles of the glottis.*

II. *This glottidean contraction must greatly preponderate over, if it do not entirely supplant, all other pathological conditions, in these diseases, as a direct cause of impediment to the access of air.*

For the reasons already assigned, it is evident that any influence which the muscular fibres of the trachea can exert in occasioning dyspnœa, by lessening the calibre of the tube, will be of a very subsidiary kind, when co-existing with the glottidean contraction. The opinion, therefore, of Drs. Cheyne and Williams, who attribute an equal share to "spasm" in both situations in the production of "the croupy inspiration and cough, and the hoarseness" ('Library of Medicine,' vol. iii., p. 56, and 'Cyclopædia of Practical Medicine,' vol. iii., p. 22), is manifestly untena-

ble. On the same grounds, the estimate of the last-named authority (*loc. cit.*) respecting the agency of "effusion in the lining of the trachea and larynx," and of the pseudo-membranous tubular deposit in their interior obstructing the passage of air "directly by their bulk," must also be regarded as erroneous. The anatomical characters of laryngo-tracheal inflammation, in short, are of less pathogenic importance *per se* than by reason of their indirect tendency in inducing the excito-motor act of constriction of the glottis.

III. *When the inflammatory action is once excited, it will, if continuing unabated, maintain the glottidean contraction throughout the attack, whilst the function of the true spinal system of nerves remains unimpaired.*

This deduction, so far as I have been able to ascertain, is opposed to all views hitherto advanced on this subject, with the single exception of those of Dr. West, which I shall afterwards have occasion to notice more particularly. The two writers last quoted are, I believe, those who have spoken most definitely on the point; both refer to the glottidean contraction in inflammation of the laryngo-tracheal mucous membrane, as a transient condition, a "temporary spasm," liable to complete remissions; and the one describes it as marking the onset ('Cyclop. of Pract. Medicine,' vol. iii., p. 22), whilst the other ('Lib. of Medicine,' vol. iii., p. 54) recognises its presence "particularly in the advanced stages" of the disease—a contrariety of opinion the adjustment of which it appears unnecessary to attempt, as the conclusion last stated negatives alike each of the conflicting assertions. It is questionable, indeed, whether the term *spasm* can with strict propriety be applied to this muscular contraction at all. "Spasm," according to Dr. Latham's definition—an exact explanation, I believe, of the meaning usually attached to the term—"is a mode of action in muscular structures different from or beyond the natural and accustomed mode;" but the glottidean contraction accompanying laryngo-tracheal inflammation is a normal act, although called forth by a morbid excitant. If spasm it must be called, it is at any rate a *tonic* spasm: and although *apparent* remissions and exacerbations of dyspnœa are undoubtedly exhibited, with more or less distinctness, in all cases, at irregular intervals throughout the attack, the causation of these admits of a satisfactory explanation, in strict accordance with the views I am endeavouring to establish: for the glottidean contraction, from the moment of its first occurrence, will originate a condition directly antagonistic of itself. By impeding the access of air to the lungs, and, therefore, of a due supply of oxygen to the blood, it will give rise to a gradual poisoning of the latter by carbonic acid. The effect of this toxæmia in the nervous centres, when it has attained a certain degree of intensity, will be to impair the energy of the *vis nervosa;* relaxation of the glottidean constriction must follow, and the stridor of the respiration will abate, or altogether cease for a time. The aeration of the blood will then be in some measure renewed, and the nervous power so far restored as to permit the glottidean contraction to resume its sway, and maintain it until again overborne by the influence of the blood-poisoning which it will in turn re-induce. Unless arrested by treatment, this alternation of opposed pathological states will go on, the toxæmia gradually gaining the ascendancy, until the function of the true spinal system is abolished, and a fatal result succeeds. Some diversity of opinion exists in regard to the mode in which this is brought about. According to most authorities it occurs by *asphyxia*—a term for which Dr. Watson ('Lectures,' &c.) proposes to substitute "*apnœa*"; and Dr. William Budd ('Medical Times and Gazette' for June 19th, 1852) the expression, "*death by privation of oxygen.*" The views I entertain of the pathology of these affections suggest the phrase "*death by toxæmia coma,*" as a more accurate description of the fatal termination in the circumstances in question.

In a direct ratio with the severity of the inflammatory action, but also modified, perhaps, in some measure, by peculiarity of constitution, will be the urgency of the symptoms depending on the glottidean contraction and the toxæmia respectively. It, therefore, varies much in the different forms of laryngo-tracheal inflammation, and even in different cases of the same variety of it; but the difference, in all circumstances, is only that of degree. The frightful

"Gasp and tug for life—the nostril stretched with struggling"—

of sthenic croup, and the calm *habitual* stridor of chronic laryngitis, equally betoken the narrowed glottis. The livid countenance and dull eye of the first, and the pallid

aspect and cold extremities of the last named affection, signify alike the injurious influence of maloxygenated blood.

But, it may be objected, why does this glottidean contraction here stop short of effecting the complete closure of the orifice, and inducing the sudden asphyxia which is observed to ensue from it in other affections, as in *laryngismus stridulus* and the severer forms of *hooping-cough*; as well as from contact with the rima glottidis of noxious gas, or solid or liquid particles? The explanation appears to be that, in the circumstances just enumerated, this action is excited more intensely than by inflammation of the mucous membrane, and abruptly, when the condition of the circulating fluid affords no counteracting influence to muscular contractility; whereas, in laryngo-tracheal inflammation, the contraction supervenes, *pari passu*, with an exciting cause growing in intensity as the disease proceeds, and affording time for the development of a controlling and opposing condition of the nervous centres.

IV. *The glottidean contraction will further impede the ingress of air to the lungs by preventing expectoration.*

Much of the inflammatory products, solid and fluid, which accumulate in the trachea and bronchi in protracted cases, would doubtless be ejected, if a more patent state of the orifice existed throughout the attack. Dr. Watson ('Lectures,' &c., *loc. cit.*), in commenting on a case of croup reported by M. Chevalier in the sixth volum eof the 'Medico-Chirurgical Transactions,' and in which tracheotomy was performed with success, observes: "The effect produced by the operation was very instructive. Air was fully inspired through the opening; then a strong cough took place, by which a large quantity of viscid reddish mucus was forced out by the natural channel through the glottis. It was evident that the child could not expectorate before, simply because it could not sufficiently fill its lungs with air to drive the collected mucus out." I submit with deference, however, that, for "*simply*," in the last sentence, we ought to substitute *partly*, and add *but chiefly on account of the constriction of the glottis*. By surgical interference, no doubt, air in sufficient volume for the act of expectoration was admitted; but if the operation had not, at the same time, relieved the glottis from the contact of the current of inspired air, and thus rendered the constriction more easy to be overcome, it seems obvious that little if any of the accumulated mucus could have been got rid of in the manner described. No mention is made in this case of the more characteristic product of the disease. If it existed here, and was not expectorated in membranous shreds, it may still have been ejected in the form of pus—a fluid into which, as pointed out by Dr. Budd ('Medical Times and Gazette,' *loc. cit*), it has a tendency to degenerate. "The histological and other characters of the exudation," he observes, are very important. If you examine a portion of the false membrane under the microscope, you will see that it is essentially made up of cells or corpuscles lying in a granular blastema. These cells bear, as you will remark, a very close resemblance to the common pus-corpuscle. Pus-corpuscles are, in fact, none other than these cells, dead. . . This close affinity of the exudation to pus is, in croup, a character of great moment: since, in virtue of it, a product, which was at first solid and adherent, may, by a slight change of conditions, give place to a fluid secretion. In case of recovery, some such secretion always supersedes the croupal"—where this is present —"and is, in fact, the chief instrument of that separation from the surface beneath which prepares the way for its ejection."

V. *The other obstacles to the access of air to the lungs, which arise in protracted cases, are traceable, indirectly, to the glottidean contraction. The state of toxæmia it occasions will, when this has been of some duration, produce sanguineous engorgement of the lungs, and consequent serous effusion into the air-cells and lesser bronchial tubes, and thus another mechanical cause of obstruction to the function of respiration will be established.*

Applying the test of clinical observation to these *à priori* deductions, we shall find that it yields them, in many respects, unequivocal support.

The auscultatory signs in every case and stage of the diseases under consideration, when stridor of the cough and respiration has been set up, will be found to point to the aperture of the respiratory tube as its principal seat. In those instances in which the embarrassment of the respiration is greatest, and which advances most rapidly to a fatal termination, it is a fact, attested, as we have seen, by various observers, that no *post mortem* appearance is found to which the severity of the

symptoms or the early issue in death can be attributed. The phenomena and the result must, therefore, have mainly depended on a pathological state which existed only during life, and which, for the reasons already assigned, *could have been* no other than the glottidean contraction.

What is the condition of the patient during the so-called "remission" of croup, for example? A soporose, or, as Dr. Williams expresses it, "a half stupid state," clearly signifying cerebral oppression from malaërated blood, and gradually deepening, as the case advances, into the perfect coma, with or without convulsions, which closes the scene.

Detached portions of false membrane, sometimes of a tubular form, and of considerable size, are, it is well known, occasionally ejected during an attack of croup; but it is matter of common observation that, in general, no favourable augury is derivable from the circumstance, for that it occurs oftenest towards the close of a hopeless case. Does not this tell us of relaxation of the glottidean constriction, delayed until irretrievable injury has been done to the brain by the prolonged circulation through it of non-arterialised blood?

The presence of false membrane in the lower part of the trachea and bronchi, with copious mucous or muco-purulent secretion in these situations, are appearances more especially appertaining to the asthenic variety of the same disease, to cases comparatively slow in their course, and little marked by cessations and aggravations of dyspnœa; because in these the inflammation has been more moderate in degree than in the more acute form, and has excited a less energetic contraction of the glottidean muscles, allowing time for the extension of the morbid action and its results farther down the tube.

A remarkable regularity, as regards the time of invasion of the developed stage of croup, has been noted by all writers on the subject, and is indeed a well known characteristic of the disease. In one case, the premonitory catarrhal symptoms may be present for a day or two preceding; for about the same length of time, in another, slight uneasiness in the throat may be complained of, and, if this have attracted notice, inspection may reveal the diphtheritic exudation, with surrounding redness on the tonsils and uvula; in a third, the little patient may have retired to rest in apparently perfect health; but, in all, the probability is, that the first announcement of the accession of the stage in question of this malady—the first hard ringing cough—will occur early in the night, and during sleep; and the same proclivity to nocturnal invasion or aggravation of stridor, although it has excited more general attention in this the most common disease of the class, is observable, and has also been recognised, in the other forms of laryngo-tracheal inflammation.

The explanation of this peculiarity will flow from the doctrine above advanced regarding the *rationale* of stridulous cough and respiration. "Volition," says Dr. Marshall Hall ('On the Diseases and Derangements of the Nervous System,' p. 254), "has a constant influence over some of the muscular actions, of which we are almost unconscious, and which we only discover by carefully observing the effects of its subtraction. The acts of respiration, originating, as they do, in the reflex function of the spinal marrow, are nevertheless regulated and rendered equable by this silent but constant influence and agency of volition." And thus a degree of inflammatory action in the laryngeal mucous membrane which is insufficient to excite the glottidean contraction, when the muscular apparatus concerned in it is under the full control of the cerebral system, will be adequate to the production of the act in question, during sleep, when this controlling power is greatly in abeyance.

The bearing of these pathological speculations on the *therapeutics* of laryngo-tracheal inflammation may next be considered. Their general tendency undoubtedly is to inculcate promptitude and diligence in the use of remedial measures, as the sole conditions on which success is attainable. We possess resources by which, if employed energetically at the onset of the disease, the *fons et origo mali* may be directly combated, and the speedy arrestment of the malady often procured; but if this be permitted to reach an advanced stage unchecked by treatment, the morbid conditions existing from the first will be so much increased in severity, and so many new sources of danger will have been added as to present an array of obstacles to recovery against which it is utterly hopeless to contend.

A general and the leading curative indication will of course be *to overcome the glottidean contraction*. In acute cases, this must be attempted not alone by means

calculated to subdue the inflammatory condition on which the contraction depends, for, in many instances, life would be extinguished before our object could be attained in this way; but we must also endeavour to procure the immediate and prolonged suspension of the perverted reflex action, in order to gain sufficient time for the removal of its exciting cause. If we cannot first *palliate*, we shall often fail in our efforts to *cure*. It is found, accordingly, of greatest utility in these circumstances are those which can subserve both ends; bloodletting (general and local), tartarised antimony, and the warm bath, being relaxants as well as antiphlogistics; and I believe that, but for the co-operation of these agents, in virtue of their first named property, the remedial action of calomel, although a medicine of undoubted efficacy in the radical treatment of the diseases under consideration, would often come too late.

A complete examination of the *methodus medendi* in these diseases does not fall within the scope of the present communication, but a few cursory remarks on the subject may be permitted me.

In common with many practitioners of experience, I have been led to form a very high opinion of the value of tartarised antimony as a therapeutic agent in every form of laryngo-tracheal inflammation. In one variety (croup), it is regarded by some as the chief remedy—an estimate to which both theory and observation induce me to assent. I have found that its remedial influence is here best obtained by exhibiting it at first in a dose sufficient to induce free vomiting, and afterwards, whilst nothing occurs in the state of the patient to contraindicate its use, in smaller doses, at intervals of an hour or two hours, so as to keep up its nauseating action more or less decidedly, in proportion to the severity of the symptoms, until a palpable impression has been made on the state of the respiration. The proposal made within the last few years, to employ in the treatment of croup, as a substitute for this invaluable appliance, the sulphate of copper, a non-nauseating emetic, and therefore devoid of the very quality which confers on medicines of this class their chief remedial value in the disease, is one not likely to find favour in the eyes of the practical physician. Its *primâ facie* absurdity will justify him in dismissing it without a trial.

Admitting the efficacy of early and free venesection, which, apart from its own remedial tendency, will be of service in promoting the action of the tartar-emetic in acute laryngo-tracheal inflammation affecting the adult, I am yet of opinion that, in the topical abstraction of blood, we possess a measure more generally applicable, and of still greater utility. In croup, I have long restricted my practice to this last mode of depletion. Along with the diminished doses of the antimonial, I exhibit calomel, combining with this, when it is judged expedient to discontinue the other, the compound powder of ipecacuan. Thus guarded and aided, the mercurial is pushed to the extent of inducing its constitutional action, should any degree of stridulous breathing or cough remain: and true exacerbations, if such arise, are met by a recurrence to the use of the nauseant whilst it can be borne.

In all severe cases which have not yielded in some measure to other treatment within the first few hours from the commencement of the attack, I have recourse to counter-irritation along the sides of the larynx and trachea; and I believe no better method of effecting this has yet been devised than the application of the emplastrum cantharidis, succeeded, before complete vesication has taken place, by a warm poultice.

I have made some trials of the inunction over the larynx of belladonna, as an auxiliary to the more powerful relaxants; but, with these at the same time in operation, I have been unable to determine whether it had any share in bringing about a cure, when that was the result. It is, at all events, an expedient which can in no way interfere with the other treatment, and may perhaps have its use.

It has occurred to me that benefit might be derived from the cautious administration of the enema tabaci (other means failing), in cases of extreme urgency in the adult, but, since the idea was formed, no suitable opportunity of carrying it out has presented itself.

There is yet another method of directly attacking the glottidean contraction—a recent, and, in my estimation, a most precious addition to the *armamentarium medicum*, applicable to all forms and stages of laryngo-tracheal inflammation. I refer to the mode of "swabbing" the larynx with a strong solution of the nitrate of silver, lately introduced, or at least brought prominently under the notice of the profession,

by Dr. Horace Green of New York. The immediate power which it often very strikingly manifests over affections of this class can scarcely be derived from its astringent or caustic action on the mucous membrane alone; but, whatever its *modus operandi*, the value of this agent will be acknowledged by every one who has fairly tested it. The alacrity with which even very young subjects submit to the application, after their original dread of it has been overcome, is of itself a proof of the efficacy of this remedy. I am by no means satisfied that the *swab* can be so easily introduced into the larynx as Dr. Green and others assert. At least, I suspect I have myself but seldom if ever succeeded in passing it completely within the rima glottidis; but I believe that, if it be firmly pressed against this orifice just when the patient begins to inspire, the tongue being at the same time depressed to the utmost, we shall be enabled to apply the solution sufficiently over the mucous surface to secure its remedial action. From a want of attention to these precautions, and of readiness to seize the favourable moment, especially with children, the operator may, as I think I have more than once witnessed, carry the sponge over the entrance to the respiratory organs, and introduce it into the œsophagus instead.

Moist air, at a temperature of above 70° Fahr., I have not found to answer the expectations I was led to form from it. Since its introduction, by Dr. Golding Bird, as a remedy in diseases of the respiratory organs, I have tried it on many occasions, but have never been able to trace any part of my success in treatment to its agency. Its employment in private practice is at all times difficult and inconvenient, often utterly impossible.

When the advance of the disease towards a fatal issue threatens to outrun our endeavours to unlock the glottidean constriction, time for the fulfilment of this indication may be gained by *admitting air to the lungs through an artificial channel*. Considerable diversity of opinion prevails respecting the most eligible situation for the operation of bronchotomy, and the comparative merits of laryngotomy and tracheotomy—a question into which it is not my intention to enter, further than to state that, as an expedient in all cases of laryngo-tracheal inflammation, I give a decided preference to the last named proceeding, it being more remote than the other from the seat of disease, and because its performance does not, like this, involve the necessity of dividing, to a greater or less extent, the cartilages of the larynx. I begin the incision into the tube immediately below the cricoid cartilage, and extend it as far down the trachea as is necessary to permit the insertion of a canula of adequate size. The alleged greater difficulty of this operation than that of cutting into the crico-thyroid space ought not, I think, to weigh against its obvious advantages; and if the cricoid cartilage be fixed and drawn forwards by means of a blunt hook applied to its inferior border, whilst an assistant steadily holds asunder the edges of the preliminary incision through the integuments and between the muscles, the last step of the operation will be rendered sufficiently easy of accomplishment in the hands of a surgeon of ordinary dexterity.

The pathological views above adduced will further tend to impress upon the practitioner's mind the expediency of adopting this measure early in the disease, giving additional force to the observation of Louis, that, "as long as bronchotomy is considered an extreme measure (*un dernier ressort*), it will be always performed too late." The period of the attack at which the indication for its employment will arise must vary in the different forms, as well as in the individual instances of the same form, of the malady. I have met with cases in which an attack of croup, preceding and retarding the eruptive stage of some of the exanthemata, continued for four, five, or even six days, without perceptible abatement of the laryngeal symptoms, but in which, notwithstanding, there was no necessity for surgical interference; and I have also seen examples of the same subacute and unyielding condition, similar or even greater in duration in the diphtheritic variety of this affection, where operative procedure was equally uncalled for. On the other hand, circumstances will present themselves under which an hour's delay would be fatal. I therefore believe that the practitioner who follows any general precept in this matter, whether it inculcate a delay to operate of six or of thirty hours, or who accepts any other guidance than that of "the unwritten tact" which is the priceless offspring of experience and attentive observation, will oftener be led astray than directed aright.

In a large majority of instances, I venture to affirm, the failure of medical

treatment, without its surgical auxiliary, may be foreseen before the disease has advanced too far to admit of the operation being had recourse to with a prospect of success; and if an unexpected recovery does now and then occur, under the use of what may be called the ordinary curative measures alone, such exceptional cases can obviously have no weight in determining the rule of practice.

Over and above the *indirect* remedial tendency of bronchotomy, I regard the first stage of this proceeding as a remedy proper in laryngo-tracheal inflammation, exerting its beneficial influence in two ways. The dissection of the integuments and muscles from the tube, to the necessary extent, cannot be accomplished without dividing some of the branches of the thyroid arteries in their course to the interior, and thereby diminishing the supply of blood to the seat of constriction. A condition unfavourable to the persistence of that constriction is thus established, and a very direct local depletion at the same time practised, calculated to lessen inflammatory action. These objects will probably be still more effectually served, if the incision be made to reach from the lower edge of the thyroid cartilage to the isthmus of the thyroid gland, and if the organ be freely denuded of the soft parts covering it to the same extent, especially at the situation of the crico-thyroid membrane. In one case of diphtherite, a marked abatement of stridulous respiration, which the foregoing considerations may explain, almost immediately followed the completion of the preliminary steps of the operation. After reflection, in fact, has inclined me to doubt whether opening the tube itself might not have here been dispensed with. In similar circumstances, I should not again complete this measure until admonished of the necessity of doing so by a recurrence of the dyspnœa.

The value of this operation in laryngo-tracheal inflammations attacking the adult is acknowledged on all hands, but where infantile affections of the same class are concerned, the case, it need scarcely be said, is widely different. Tracheotomy, as an expedient in every form of croup, is almost universally tabooed in this country, all our leading authorities taking part in the process, with more or less energy. Whether this unconditional rejection of it be merited or justifiable, will form the subject of inquiry in a succeeding communication.—*Association Medical Journal, July 7 and* 14, 1854, *pp.* 588, 616.

156.—*Dr. Marshall Hall's Tracheotomy Scissors.*—[At the soirée of the Medico-Chirurgical Society of Nottingham, Dr. Hall thus described the scissors which he uses for opening the trachea, both in epilepsy and in other cases.]

You observe these scissors, (see A.) They meet in a *point;* each blade is filed so as to leave a *notch.* With these the integuments are first to be divided, and the trachea pierced; the blades are then to be gently but firmly opened; into the orifice thus made, this cage (see B) is to be inserted, and the operation is complete

A

B

—complete with scarcely the loss of a drop of blood or of a minute of time. The cage, as you see, admits of compression for introduction, and for removal to be cleaned. It is not tracheo*tomy*, but tracheo*tony*, from τείνω, I stretch, instead of τέμνω, I cut.

With this tracheal opening "epilepsia *laryngea*" is impossible; such epilepsy may be prevented altogether if the case *consists* in *laryngeal* attack exclusively. It will be *abortive* only and issue in other forms of epilepsy if such forms are apt to occur in the absence of laryngismus. There is a case in a New York Hospital of a patient who has *thrice* undergone the operation of tracheotomy; thrice the seizures have been superseded by the operation; thrice they have returned on closing the orifice. At the present time the orifice is permanently patent, the edges of the wound being cicatrized, and the patient is no longer the epileptic "wretch" he was —*Lancet, June* 17, 1854, *p.* 648.

157.—ANEURISM OF THE COMMON FEMORAL ARTERY CURED BY COMPRESSION.

By Dr. Maurice H. Collis, Surgeon to the Meath Hospital.

[The cure of aneurism by compression is now an established practice, so much so, that we have not thought it necessary to make abstracts of the numerous cases which from time to time are published. But the reader will find that in the following case some interesting facts are established.]

In the present instance compression was applied close to the sac, and appears to have acted in a different manner and on a different principle from what has hitherto been claimed for it. According to the highest authorities upon this subject, the artery has been always found pervious at the seat of pressure, when opportunities of post-mortem examination have occurred; and the sac has been found filled with concentric laminæ, which seem to be successive deposits from a current of blood circulating through it; consolidation of its contents *en masse*, as a result of stagnation, is regarded as improbable, and would be thought dangerous, and liable to cause relapse or suppuration. Now, in the case reported below, it will be found that, in consequence of pressure being applied close to the sac, no branches being given off between it and the point of compression, the sac was consolidated with rapidity; and, of necessity, not in concentric laminæ; and the artery was filled up as high as the next collateral branch: these results were obtained by the rapid formation of a coagulum without the occurrence of inflammatory action at the compressed point; nor were they followed by suppuration of the sac or secondary aneurism. This coagulum occupied, as far as we could judge, the entire space in which the current was interrupted, that is, the space between the branches given off above and below the point of pressure. If further observation should prove that this is not an isolated and exceptional case, it guides us to a new principle, which may be applied not only where the aneurism is situated as this was, but with certain limitations to pressure upon the distal side.

The details of the case are as follows:—

Christopher Toole, aged 32, by trade a nailer, a tall, athletic man, of dark complexion, presented himself among the extern patients at the Meath Hospital, on Monday, July 24, 1854, with a pulsating tumour on the upper part of the right thigh. This tumour was 4½ inches long and 5½ wide; the circumference of the limb over the centre of the tumour was 21½ inches, that of the corresponding part of the sound limb being 18½. It occupied the upper part of Scarpa's space; its upper margin was in contact with the depression which marks the position of Poupart's ligament, and it extended downwards along the course of the common and superficial femoral vessel. The pulsation was very strong and diastolic, and the action of the artery above it was so forcible, as to lead some persons to conclude that it was considerably dilated; by pressing upon the abdomen firmly, we could trace the pulsation in the external iliac for a distance of nearly four inches. The superficial veins of the thigh were remarkably large, knotted, and tortuous; the glands in the groin were somewhat swollen; and upon the outer aspect of the tumour two glands could be felt elongated and flattened, and partially sunk into it; the entire limb was enlarged, slightly œdematous, but of a natural temperature and

sensibility. There appeared to be some solidification of the tumour on its outer margin: subsequent observation showed this to be at least partially deceptive, and resulting from the varicose condition of the veins on the internal side. Pressure upon the artery in the groin readily checked all pulsation, but without any sensible effect upon the volume of the tumour. No bruit de soufflet was audible in any position, nor was there any fremissement; the whole arterial system was excited, and the man complained greatly of the throbbing; even the momentary pressure of the thumb upon the artery gave him great relief. He stated that his general health was good, and that for the last two years he had been temperate; his occupation rendered stooping necessary, and also constant rotation of the body at the hips, the feet being kept unmoved. The floor of his workshop was uneven, and his right foot had for some weeks rested habitually on a lower level than the left. The tumour made its appearance, as far as he was aware, spontaneously, about six or seven weeks before his application at the hospital: it was then about the size of a nut, and had ever since grown steadily larger.

Tuesday, 25th. After administering purgatives, &c., compression was attempted; various instruments were tried, by none of which could the artery be commanded, even partially, for more than a minute or two. The only point available for pressure was just above the sac, at the spot where the epigastric and circumflex vessels are given off. As the brim of the pelvis is rounded just at this place, all the instruments were displaced by the ordinary respiratory movements, and slipped off the artery; a relay of pupils kept up pressure sometimes by the finger and sometimes with the clamp or leaden weight; but it was partial and irregular, in spite of the greatest vigilance and anxiety on the part of all, including the patient himself. Low diet was strictly enforced, and purgatives of croton oil were occasionally exhibited. Quiet was procured by opiates. Chloroform was administered once, but produced excessive headache. An attempt was made to draw some blood upon the second day, but it was thick and carbonaceous, and would not flow. On Saturday, after upwards of four days' ineffectual attempts at compression, the skin in the groin began to chafe and suppurate round the root of every hair, and it became evident that pressure could not be borne much longer; the aneurism had increased half an inch outwards and downwards, and the pulsation was as violent as before, compared with the general force of the circulation; no attempt at consolidation had taken place, and all parties were inclined to be disheartened.

On Sunday, the 30th, I determined to make another effort to command the vessel completely. Having procured a double clamp tourniquet, which closed by a screw at the hinge, I applied it, placing a small roll of bandage an inch thick under the part. I screwed it home, and found that it controlled the pulsation for a moment, and, like all the rest, slipped off towards the abdomen. By screwing it a little tighter, and drawing it downwards by a tape from the knee, I was enabled at last to get the artery completely controlled. Before finally adjusting it in this position, I drew up the skin, so as to get an unchafed part under the pad. The tourniquet was not disturbed for thirty hours, and at the end of this time, upon slackening it, all pulsation was found to have ceased in the tumour and in the artery as high as it had been traced before, namely, to the spot where the common iliac bifurcates.

About twelve hours after the tourniquet was applied the man felt a stinging pain on the inside of the knee and leg, and a kind of tingling sensation along the outside of the hip and thigh. While under pressure he took a couple of sedative draughts, containing half a drachm of chloroform.

The spot of skin where pressure had been first attempted was beginning to slough in points, but in a few days it became healthy. The tumour became a little smaller in the first few days, although the subsidence of venous congestion and œdema gave it a more prominent appearance. He never had the smallest pain nor single bad symptom after the removal of the clamp, and upon the 12th of August he got up and walked about the ward.

On the 16th he left the hospital, and returned almost immediately to his work, having been in hospital twenty-three days; nor would it have been necessary to keep him so long but for the slough of the integument caused by the first abortive attempt at compression. I saw him on the 6th of October, and found the tumour reduced very considerably in volume; the difference between the circumference of the two limbs was less than one inch.

In the mere details of this case there is little worthy of remark beyond the rapidity and painlessness of the cure, and the fact that it is the first instance on record of the cure of aneurism of the common femoral artery by compression. There are, however, some points of general interest to which I wish to draw attention. These are:—

First. The position of the point of pressure as regards the sac and as regards the collateral branches. The artery was compressed close to the sac; the point of pressure covered the usual origin of the superficial circumflex ilii and epigastric vessels, the latter of which could be felt pulsating before treatment was commenced; hence the nearest collaterals were practically the internal iliac on the one side, and the profunda on the other, and pressure was thus made between the sac and one of the next collateral branches.

Secondly. The fact that under these circumstances the aneurism increased in size, as long as pressure was incomplete, and upon the artery being completely controlled coagulation was rapidly obtained.

Thirdly. The simultaneous blocking up of the artery above the point of pressure, as high as the next collateral branch.

Fourthly. The non-occurrence of secondary aneurism, or of suppuration of the sac.

Fifthly. The bearing of this case upon the question of pressure at the distal side.

The immediate proximity of the point of pressure to the sac appeared to be a dangerous though unavoidable complication; it was, however, not only unavoidable, but essential to the cure. As but one point existed where pressure could be made, it was necessary that the cure should be rapid, or the skin would have failed us. Ignorant of the principle which appears to guide the cure in pressure close to the sac, we at first were satisfied in endeavouring to cut off the impulse of the heart without completely shutting off the current. As long as we did so the sac continued to increase in size, and to pulsate with as great a vehemence as ever. No sooner was the current completely intercepted from the sac by pressure between it and the nearest collateral, than coagulation of its contents took place. No current through the sac was now practicable, and the consolidation was, as we may suppose, not only rapid but complete. In compression at a distance from the sac, as in ligature similarly situated, blood is deposited layer by layer, until not only the sac but the artery at the injured part is filled up. No such process could have occurred in this case, for no branch existed between the sac and the point compressed, and therefore no current could have made its way through it. The effect of thus converting it into a *cul de sac* was the coagulation *en masse* of its contents. The same result took place in the *cul de sac* above the tourniquet, the artery becoming obliterated in a similar rapid manner. To this latter circumstance is probably to be attributed the non-occurrence of relapse or of suppuration in the sac.

This case seems to suggest the possibility of curing aneurism by distal pressure, under favourable circumstances. If we can effect the consolidation of an aneurism by converting it into a *cul de sac*, there is reason to believe that with due regard to preparatory treatment, this result might in some cases be obtained by choosing a spot between the sac and the distal collateral branch as the seat of pressure. We see in this case that not only the sac was filled up, but the artery also, which was exposed to the pulsatile wave of blood. When pressure is made on the distal side of an aneurism, we observe an increase of impulse for a few moments. If the compression is *complete, and firmly kept up*, this momentary irritation subsides, and the aneurism pulsates with perhaps less force than before. This principle of converting the aneurism into a *cul de sac* appears to act occasionally in compression at a distance. Dr. Carte, who has had great opportunities of observing the various phenomena which arise during the cure of aneurism, has stated it to me as his conviction that many, if not all, of the rapid cures are effected by a loose clot blocking up the distal portion of the artery. Certainly we can scarcely conceive it possible that a cure which occupies seven, ten, or even thirty or forty hours, could be effected by the slow process of a deposit, layer by layer, until the sac is filled. We see also, that a similar result has been obtained purposely by manipulation of the sac, a process which is too dangerous to be other than exceptional in application. From all these circumstances, therefore, I think we may infer the probability of distal pressure being occasionally useful; it is at least worth a trial, where the position of

the sac prevents our using the ordinary method; and if we can get a point for compression between the sac and the nearest collateral branch, I think the result of the case which I have now put upon record will give us encouragement, although indirect, to hope for success.—*Dublin Quarterly Journal of Medical Science, Nov.* 1854, *p.* 315.

158.—ON THE USE OF TEREBINTHINATED VAPOUR BATHS IN RHEUMATISM, GOUT, STIFF JOINTS, &c.

By Dr. Antonin Chevandier, of Die (Drôme.)

Turpentine, which is probably the most active agent in the aromatic vapour baths, which form the subject of this memoir, has been employed both internally and externally,—in frictions and local fumigations. The other resinous principles which enter with it into the dry vapour in which we immerse our patients have been acknowledged to be worthy of the reputation our predecessors had attached to them.

It is therefore not with a new remedy I am anxious to enrich the materia medica; it is a new mode of administration I wish to bring before the profession, namely, the exhibition of resinous vapour in the form of bath, or rather of general fumigation.

The following is, in a few words, the crude fact which attracted my attention :—

As I have stated in a former publication, the peasants of our mountains have long been accustomed to meet around the furnaces in which black pitch is distilled. It was some moments after the chips of pine had been arranged at the bottom of the furnace, previously heated for forty-eight hours, when the temperature is sufficient to melt the resin solidified in the air, that they descended into this *infernal* medium. They took the precaution of enveloping themselves in thick woollen blankets, as a protection against the excessive heat. At the end of ten, twelve, fifteen, or even twenty minutes, they came out of this bath deluged in perspiration, lay down subsequently in badly closed huts, and returned to their homes delivered in a few days from pains of very long standing, or from very serious articular rigidities.

These immersions, of which I wished to be a witness, took place each year, during about two months, at a height of upwards of 6,500 feet above the level of the sea, in a climate where the snow is absent only for a few days.

Patients courageous enough to employ this method have assured me that they always found it agree with them. When positive and numerous facts caused me to reflect on the happy application which might be made of this general fumigation to a large number of diseases, I perceived that it was necessary to remove the danger of such unfavourable external circumstances, and to render the administration of the baths more easy. I lost no time in recommending the provisional construction in the village itself of a furnace similarly circumstanced to that in the mountain. It is a little more than six and a half feet in depth, three and a quarter at the opening, and six and a half in its mean diameter; it is oviform, and has at the bottom a very narrow tube through which the melted resin flows into a reservoir placed externally. Chips of the same wood were used in feeding it in my experiments: thus preserving all the elements which appeared to me indispensable, removing those which could be only dangerous. I devoted two months to the accurate observation of the physiological and therapeutical effects of baths of terebinthinated vapour, at a high temperature.

The patients who had taken several baths on the mountain descended the first, encouraged those who had not done so, and by their easy resistance of heat justified me in establishing a mean temperature of 70° R. (189°·6 F.), much lower than that which some had been subjected to on the mountain.

It will at first view be thought surprising that a man can remain without danger in the midst of so elevated a temperature, saturated with exciting aromatic vapours disengaged from resin in a state of fusion. It should be observed that the patients seated round the furnace, protected by their blankets from the heat radiating from the walls to which they turn their backs, have their heads in the centre, where a current of cold air rushes in. The thermometer, placed at the foot of this descend-

ing column, marks even there 55° R. (155°·75 F.). Thus situated some patients have remained for more than half an hour without experiencing any serious inconvenience. Violent excitement of the circulation, some difficulty of breathing, stinging heat of the skin, or commencing determination to the head, induced them to withdraw.

In order to test in my own person the physiological effects of the bath of terebinthinated vapour, but above all with the view of ascertaining the dangers to which patients might be exposed during their immersion, I was anxious to descend into the furnace, having provided myself with a spirit thermometer to examine the temperature of the furnace in its several points, and sheltered under an immense blanket, which descended from the head over the shoulders and down to the feet, under which I almost entirely disappeared, I squatted down on the chips from which the bubbling resin was escaping: I wore no other clothes than a large pair of pantaloons and cloth waistcoat: the thermometer placed behind my back, and consequently exposed to the heat radiating from the walls of the furnace, indicated at the end of ten minutes 82° R. (216°·5 F.), and remained at that point as long as the bath lasted; when held between my knees it fell to 68° R. (185° F.), and even to 60° R. (167° F.), at the foot of the descending column of cold air to which my head was exposed. On entering the furnace I felt almost suffocated so long as I remained standing, but once seated on a wooden bench arranged for that purpose, I was no longer uncomfortable. It was exactly 7 o'clock. My pulse, at the period of immersion, beat 65 in the minute. For ten minutes I conversed with my neighbour, asking him how he felt, and telling him that I was not at all incommoded. However, I took the precaution of holding a handkerchief steeped in cold water on my lips and forehead to cool the air I breathed.

The resinous vapours, which were disengaged in the form of a light transparent smoke, far from causing suffocation or disagreeable burning in the throat, gave rise, on the contrary, to a pleasant and soothing sensation; an almost imperceptible tingling was produced in the skin, which became the principal seat of congestion; it was very hot, very red, and was covered with perspiration. The arterial circulation was accelerated, the pulse was developed, hard and full, and beat 88 in the minute; the pulsations of the heart were not troublesome; the head was free, burning; the face was very animated; there was no dulness nor nausea; even the conjunctiva was not irritated. I was streaming with perspiration, and yet had not experienced the weariness which precedes or accompanies that produced by ordinary means. At the end of twenty-five minutes, the respiration becoming more rapid, the determination to the head more decided, and the internal heat insupportable, I came out of the furnace; I ran to my room, which was only a few steps distant; my step was firm; I threw myself into a well-warmed bed, keeping on my clothes and my burning blanket, which was a little injured. The pulse soon rose to 95; the determination to the head became more decided; the beating of the heart caused the chest to heave violently. After some minutes all these symptoms abated; the pulse fell to 80; the palpitations ceased; perspiration set in so profusely as to penetrate the mattress. I had taken an hour's repose, when, after having had myself wiped with flannel, I got up, put on warm clothes, and returned to my patients.

I had an extraordinary suppleness in my limbs, and experienced surprising agility; I did not feel weakened; I ate with more appetite than usual; at night my sleep was disturbed by dreams, in which hyper-excitement of the genital organs was not idle. The urine was highly coloured, and had a decided violet odour, but presented no sediment. I have since several times used the bath, which has never fatigued me.

160 patients descended into the furnace in the space of two months: every age, from 16 to 75; every species of temperament, from the rachitic to the apoplectic, have been subjected to the action of the resinous vapours, at the average temperature of 65° R. (178°·25 F.); two persons only were obliged to leave the furnace; one an old man of 64, overcome by the heat at the margin of the lower part, was seized with a fainting fit, which soon went off in the open air; the other, after an immersion of sixteen minutes, having returned by himself to bed, laboured for some minutes under a general delirium with tendency to sleep. A few cold applications quickly dispersed these symptoms. These two patients have subsequently had

recourse to the baths without inconvenience. In others I have observed, in a more or less intense degree, all the physiological effects which were produced in myself, and which I have above described. A great number suffered from ardor urinæ; sleep is in general interrupted by starting, and disturbed by erotic dreams. In several the excessive perspirations were followed by transient miliary eruptions, which have in no instance produced an amount of weakness proportioned to the loss the patients suffered by the skin.

Twelve baths, taken in two series, with an interval of eight days' interruption, constituted the treatment of the season.

I say nothing as yet of the therapeutic effects which numerous observations, collected daily and carefully noted, will soon bring before the reader.

Without waiting for the voice of experience, which concedes a great value to this therapeutic means, what are we to think, *d priori*, of the possible advantages of its application to medicine? Its physiological effects, the general hyper-excitement produced by baths of terebinthinated vapour, warrant us in affirming that their employment will be almost co-extensive with excitant medicine itself; and that, moreover, the special action of resinous bodies on the mucous membranes causes this method to be doubly indicated in chronic diseases of the skin, in chronic catarrhal affections of the chest or bladder, and, above all, in chronic rheumatism, gout, gonorrhœa of long standing, amenorrhœa, &c.

My anticipations are justified by the ordinary success of rational medicine in the great class of diseases which Hufeland calls *rheumatosis*,—in all cases of disease requiring the employment of resinous bodies, the terebinthaceæ, tar, benzoin, &c. In fact, the vapours which I have called terebinthinated would deserve a name of a more extensive application, for it is not only turpentine, but also all the volatile principles of the resins, which are resolved into vapour.

The tree from which black pitch is extracted occupies the elevated table lands of some of our mountains. It pretty closely resembles the *Pinus pinea*, from which it differs in the elevated situations it inhabits; it is also distinguished from the latter species by the comparative smallness of its fruit. This pine is very rich in resin. In the month of May a long piece of bark is removed by the stroke of a hatchet, and the wood is thus left exposed. The resin which exudes hardens in the air, and is found at the end of a fortnight in a layer of greater or less thickness, which is removed by a second stroke with the portion of wood it covers, and this constitutes the chip. The solidified resin is met with in the form of yellowish-white opaque masses, of the consistence of wax, the taste of which is at first sweet and agreeable, but afterwards acid and bitter. It is the turpentine derived from the pine from which the essential oil has not been previously extracted.—*Dublin Journal of Medical Science, Nov.* 1854, *p.* 476.

159.—ON CANCEROUS DISEASE OF THE CŒCUM.

By Dr. R. B. Todd, F.R.S., Physician of King's College Hospital.

[In this case Dr. Todd describes the pallor of the patient as being very remarkable, attracting the notice of most people who entered the ward.]

Concomitant with this anæmic state, there was a very manifest fulness in the right iliac region, accompanied by pain and tenderness there. A resistance was experienced on pressure being made, and a sensation was communicated to the hand, as if a firm elastic tumour were compressed. On comparing the two regions on opposite sides, one presented the natural yielding feel, while the other felt firm and resisting, with an indistinct feeling of punctuation. On this latter side (the right) there was decided dulness upon percussion, and deep percussion gave no indication of the presence of intestines beneath; occasionally, however, a slight sonoriety could be elicited.

When the problem before us is, to diagnose the nature and connexions of an abdominal tumour, we must notice the sounds elicited upon superficial and deep percussion. By the former term I mean percussion made with only very slight pressure on the abdomen; by the latter term I would signify percussion made simultaneously with deep firm pressure on the surface. We have a good illustration

of the importance of the use of both kinds of percussion in physical examination of the abdomen in cases of enlarged liver. In such cases, when the liver is thin and soft, superficial percussion alone would often lead you to overlook the enlarged gland, by eliciting a clear sound from subjacent intestines. In other instances, the very reverse takes place; a slight superficial tap will give you the dull hepatic sound, but a deeper percussion would elicit the intestinal sound. In the present case the dulness was equally decided, both upon superficial and deep percussion.

Pain, referred to the right iliac region, and extending to the loin, was a prominent symptom of the case. The patient complained of sharp shooting pains in the region of the tumour; and there was also considerable tenderness on pressure, which varied considerably, probably according as the bowels were full or empty.

The ordinary functions of the body were properly carried on, the circulation appeared healthy, there were no symptoms of dyspepsia, the bowels acted freely, the kidneys secreted as in health, and the usual quantity of urine was secreted. The urine was frequently subjected both to microscopical and chemical examination, but we were unable to discover anything abnormal; the specific gravity varied from 1010 to 1015, and in every respect the secretion appeared healthy.

In endeavouring to arrive at a diagnosis in this case, our attention was directed to determine first the cause of the anæmia, and, secondly, the cause of the pain. Now, as anæmia in women is so often associated with something astray in the catamenial function, we made particular inquiries respecting it.

An anæmic state may (as you know) be produced either by a too profuse flow, or even by the opposite state—a stoppage of the catamenia. The condition which is termed chlorosis, is almost always dependent upon a deficient secretion or total suspension or non-appearance of the catamenial flow. The catamenial function may not have been developed, or having commenced it may have stopped, in either of which cases an anæmic condition might result. Where there is menorrhagia, the constant or repeated drain exhausts the colouring matter of the blood. When no catamenial discharge takes place, or when it is very defective, the nutrient changes of the blood are impaired, and the hæmatine is not formed of sufficient quantity, or of proper quality. The result of our inquiries was that there was no fault connected with the catamenial function adequate to explain the anæmic condition.

It appears also that there had been no loss of blood from any other source, none from the rectum or the bladder, or the kidneys, or the nose, nor from any internal organ.

Neither was there any evidence of exposure to the influence of the marsh poison, which often gives a pallid sallow hue, similar to, though seldom to such a degree as was exhibited by this woman.

These causes failing to explain the peculiar anæmic condition, I was led to suspect the existence of malignant or cancerous disease; nor was this idea contradicted by the patient's age, which was about 30; for at that period malignant disease is by no means uncommon, especially in women.

The presence of a tumour in the right iliac region was of course confirmatory of this suspicion, and seemed to indicate that the cancer was seated there; but to determine its precise seat, and the exact relations of the tumour, demanded further investigation, which was necessary for the solution of the next question, namely, as to the cause of the pain.

What, then, gave rise to the pain which this woman experienced in the right iliac region? It was a severe lancinating pain, and occurred at all times, and not only when the part was pressed upon. Her sleep was very much interrupted by it. It was increased, but not very materially, by pressure. It did not seem to be aggravated by the action of purgatives. Soon after she took castor-oil daily for a few days, but with rather an alleviation than an increase of the pain.

A very decided swelling existed in the right iliac region. It was not a prominent tumour; but, as the patient lay upon her back, you saw a decided convex swelling of the right iliac region, the corresponding position on the left side being concave and rather collapsed. Pressing upon this convex swelling, you experienced a decided resistance. This resistance began about a finger's breadth internal to the anterior superior spinous process of the ilium, and extended upwards for about an inch and a half or two inches. Over all this resisting surface it was that we found the dulness on percussion to which I have already referred. Above and to the inside of this surface the percussion sound was clear.

Is it possible that this tumour could have been caused by enlargement of any of the great internal organs? The spleen and left kidney were excluded, by the tumour existing on the right side. We had no difficulty, also, in excluding the liver, as that organ for the first few weeks after the patient's admission was clearly not increased in size.

Might it not have been an enlargement downwards and forwards of the left kidney? An enlarged kidney might certainly extend to the iliac region, but it would also show itself elsewhere—you would find it bulging in the loin, causing a fulness and a dulness in that region; but we found no such fulness here, and, on making pressure, no resistance was encountered, such as you experience from enlarged kidney. Nor could we trace the tumour upwards into any mass resembling an enlarged kidney; on the contrary, the swelling ceased abruptly above the iliac region, and the dull sound gave place to an intestinal sound. Add to all this the fact that repeated examination showed the urine to be quite healthy, and, I think, you will admit we were justified in excluding enlarged kidney as forming the tumour.

There were, however, other and more likely causes of this swelling. Might it not have been a fæcal accumulation in the cœcum? The tumour was scarcely large enough for this; and it was too hard for it and too resisting, and, also, much too painful. Still, I thought it desirable to test it, by giving repeated doses of castor oil. This was done soon after her admission, but although feculent matter passed in large quantity, it was not so abundant as might have been expected from fæcal accumulation; and, what is more important, neither the size, nor the resistance, nor the painfulness of the tumour ever diminished by the free action of the purgative.

There is a thickening of the walls of the cœcum of a benign kind, resulting from chronic inflammation, of which I have seen cases in both men and women. Could the tumour be caused by this? The extreme pallor and the constitutional sympathy were greatly against this, for in the cases to which I allude there is little or no constitutional disturbance, and then only when fæces accumulate. Moreover, in all these cases the walls of the cœcum can be felt, and the gut can be made to roll beneath your fingers.

Sometimes the cœcum is the seat of an inflammation which involves the areolar tissue in which it is lodged, and the peritoneum around it; and in some cases it begins at the vermiform process (typhlo-enteritis). Could we refer our case to this? I decided in the negative, because we had no acute symptoms, no rigors, no fever, none of that exquisite tenderness which accompanies that inflammation; because, likewise, purgatives did no harm, which they inevitably must have done had there been inflammation of the cœcum. Moreover, the continuance of the symptoms for three years, and the extreme pallor, could not have been explained by this view of the case.

Thus, then, I fell back upon the idea, that the swelling of the iliac region was due to the presence of malignant disease there. This view was most consonant with the extremely chronic nature of the complaint, and offered the only explanation of the peculiar anæmic condition of the patient. But now we had to inquire—What was this cancer? Was it a tumour external to the bowel, and compressing it, or pushing it inwards? Or was it a deposition of cancer in the walls of the bowel, causing great thickening, and encroaching more or less on its cavity? I felt it impossible to decide between these two questions, although, in some degree, I leaned to the opinion that the tumour was external to the bowel. The marked dulness on percussion, with rare exception, the fixedness and hardness of the tumour, and the absence of all signs of intestinal irritation, and the tolerance of purgatives, seemed to me to favour this view.

Our patient remained for a long time in the hospital. From the 12th of December till the 10th of April, no very striking change took place; she became weaker and thinner—paler she could not be; however, at the latter date, a new and most interesting symptom showed itself, tending to confirm the diagnosis of malignant disease. This symptom was an œdematous condition of the whole of the left upper extremity. The veins of the upper part of the arm were much distended, and all the tributaries of the subclavian vein appeared swollen, thickened, and hard. The extreme pallor and transparency of the skin served to display them in great perfection. The obstruction was evidently confined to the subclavian vein, for the circulation in the veins of the neck seemed quite free. This also showed that the inno-

minata was untouched, and that the destruction did not extend beyond the subclavian. The appearance of œdema of the upper extremity under these circumstances affords a beautiful illustration of the mechanism of dropsy, and shows how it may result simply from the percolation of the serum of the blood through the coats of the vessels, in consequence of the existence of an obstruction to the return of blood by the plugging up of one of the large veins. That there was obstruction was quite certain; there could be no doubt from the enlargement of the tributary veins; but we had to ascertain what was likely to be the precise nature of the obstruction.

The obstruction depended either upon the passage of cancerous matter into the veins, and the coagulation of the fibrin of the blood around it forming a plug, or it resulted from the introduction of some other morbid matter into the circulation, and the occurrence of local phlebitis; which of these conditions was present it was impossible to decide. The introduction of cancer germs or cells into the circulation is now a well-established fact, and, no doubt, explains the transfer of cancer from one part of the body to another, or even far distant from its primary seat.

On the 15th, the right leg became œdematous, and the veins enlarged, and the œdema of the arm subsided considerably, in consequence, no doubt, of a collateral circulation having been established. On the 29th, chest symptoms manifested themselves in the occurrence of dyspnœa and dulness over both sides of the chest behind; and, at the same time, there was an absence of vocal vibration, indicating that effusion had occurred into the pleural cavity. This effusion had taken place, most probably, in consequence of the very watery condition to which the blood was reduced, a state which, as you know, favours the escape of serum from the vessels, and the effusion of fluid into any of the great serous cavities.

At this time another remarkable symptom showed itself; this was the occurrence of a distinct pericardial rubbing sound, heard all over the heart, but most distinct at its base. This is a point of great interest, occurring, as it did, in such a case as the present, where the blood had become so attenuated; for it shows that fibrinous deposits, sufficient to create friction sound, may take place in very low states of the system, and where there exists a very watery condition of blood—a warning to those persons who believe that the mere deposition of fibrin is evidence of an inflammatory state, and that this deposition is to be averted by depletion, *i. e.*, by impoverishing the blood. The patient did not survive the appearance of these symptoms above twenty-four hours.

I should have noticed before, that, in the progress of the case, the liver gradually increased in size, and extended downwards towards the swelling in the iliac region, so that at last the latter seemed to be continuous with the liver.

At the *post-mortem* examination, it was found that, with the exception of the liver, which was much larger than natural, and extremely fatty, none of the solid viscera were increased in size. The spleen was certainly not enlarged, but was paler than usual, and its trabecular tissue appeared well developed. Upon microscopical examination, the spleen was found to contain a great number of white corpuscles.

The left subclavian vein was thickened throughout its whole course, and for about three-quarters of an inch from the junction with the left internal jugular it was plugged up. On following the vein down the arm, it was observed, that the axillary and one of the venæ comites of the brachial artery for half their length was also impervious, the other of these veins being pervious, and probably affording one, at least, of the channels for the collateral articulation, which was established soon after the obstruction had occurred. The chief point in the examination was to determine the nature of the tumour in the right iliac region. On laying open the abdomen, the intestines appeared bleached, but otherwise natural, excepting on the right iliac fossa, where there lay upon the iliac muscle a mass connected with the intestines. This, on being opened, looked like an abscess communicating with the intestines. The wall was thick and fibrous, and a distinct communication was traced with the ilium below and the ascending colon above, which became continuous with each other through the cavity of the abscess. As we were unable to discover the cœcum, and the tumour occupied its position, and preserved its relations, the conclusion was forced upon us that it was, in fact, the cœcum itself in a diseased state.

The interior of the cavity was very rough, its surface jagged, with large pieces of flocculent sloughy matter, slightly adherent. The whole mass was about the size of a large fist, and lay upon the iliacus muscle, which appeared soft and soddened,

but otherwise had undergone no change. The liver adhered to this mass above, and the portion in contact, when removed, exhibited an incipient slough. Beneath the tumour there were many enlarged glands, which, as well as the thickened walls of the tumour, contained numerous cells, similar to those met with in hard cancer. They varied much in size and form, some being oval or circular, others elongated or fusiform; the cells were granular, and a distinct nucleus could be found in almost all of them. No doubt this disease was cancerous in its nature. The microscopical examination affords good evidence of this, which is confirmed by the duration of the disease, the severe lancinating pains, and the cachectic condition of the patient. When the woman came in, the walls were probably in the condition called close cancer; but, after a time, a sloughing process became established, which produced great prostration, and ultimately caused the patient's death.

Some of the cancerous matter had probably been taken up by the veins, and deposited in the adjacent femoral vein, which had become, in consequence, plugged up, just as had previously happened in the case of the subclavian vein.

It is a curious point to inquire how the absence of symptoms of intestinal disturbance is to be explained. It seems to me to have been due to the perfect freedom of transit for the intestinal contents through the cavity of the cœcum. The cœcum was considerably reduced in size, and the caput coli was almost obliterated, so that the channel between the ilio-cœcal valve, of which one lip still remained, and the ascending colon, was exceedingly short; thus the action of the ilium was sufficient to propel the contents of that bowel into the colon, without any contraction on the cœcum itself being necessary; and since the thickened state of the walls of the cavity opposed undue distension, the contents of the bowel were prevented from accumulating in this situation. This is an exceedingly rare case of disease; indeed, I only recollect having seen one instance of it before, which occurred many years ago, in an old man who was a patient in the hospital in Sutherland ward, under my care. Unlike the present case, this man died of obstruction. I regret that I have been unable to lay my hands on the notes of the case, as the comparison between the two would be interesting.

Upon looking into various authorities, I scarcely find any reference to cancer affecting the cœcum, although many instances are recorded where the colon was the seat of malignant disease. Cancer of the colon occurs next in frequency to cancer of the rectum; it was for the relief of the former condition that the new operation of making an artificial anus in the loins was suggested by Amussat. The cœcum, then, so far as I know, seems seldom to be the seat of malignant disease, which appears the more remarkable when we keep in view the analogy in function between this portion of the intestine and the stomach.

The treatment to which our patient was subjected was entirely palliative and supporting, with the exception of a little purging with castor oil at first. Narcotics and tonics were chiefly employed. The former were used both internally and locally. Of the local remedies, the patient attributed the greatest relief to the belladonna.—*Medical Times and Gazette, Oct.* 28, 1854, *p.* 433.

160.—ON INFLAMMATION.

By Dr. Addison, F.R.S., Maidstone.

In epidemical diseases, inflammation is usually present in some part of the body. In eruptive fevers, it is visible on the skin; and in continued fever, dysentery, influenza, &c., there is generally evidence of some degree of inflammation.

Inflammation arises in joints from local injury, a sprain, or contusion. It may arise in the same places from gout. Inflammation follows from injuries inflicted on the skin, hot water, a blister, or mustard plaster. It appears in the same part in measles, scarlet fever, and erysipelas. Pustules arise in the skin from local irritants; also in the same part, from the poison of small-pox. What are the relations of these varieties of inflammations? and what is the natural history of the process?

In the following sketch of inflammation, conclusions derived from original investigations are stated affirmatively. The facts and experiments upon which the conclusions are grounded have been published at different periods; a short account of

them will here be given, and full references stated, for those readers who may require their details at greater length.

I. *Inflammation is a preliminary of repair.* Man placed in the world, is liable to wounds, contusions, and fractures; and properties inherent in the animal economy have been provided for their reparation. Broken bones, torn tendons, and other parts separated by wounds and lacerations, reunite and heal.

In these and all other instances of injury or irritation from injurious agents, a degree of inflammation arises; the blood-vessels of the part are more than usually injected with blood, and there is some effusion or swelling, with heat and pain. The simplest example of inflammation follows the short application of a mustard plaster. The part becomes red, hot, and painful; but effusion is scarcely to be noticed. Upon the application of a common blister, appearances are somewhat different: there is a pain and effusion, causing the cuticle to rise, but the redness is less. When an ankle or wrist has been sprained, inflammation arises; there is pain, heat, and swelling; but, as the inflammation is seated internally, in parts around the joint, redness cannot be seen. In fractured bones, ruptured tendons, and lacerations of all kinds, phenomena of inflammation occur. Lastly, in all accidents, the pain immediately following denotes the injury inflicted upon the part: but that which continues, or arises afterwards with heat and swelling, is a sign of inflammation. It is admitted by all surgical writers, that some degree of inflammation is present in all examples of repair of serious injury: and the proposition, that inflammation is a preliminary of repair of injury to the solid textures of the body, may be considered as established.

II. *Inflammation alters the structure of blood-vessels, permits effusion of lymph, and allows the blood itself an outlet into new vessels sustaining new forms of growth.* In natural or original growth, that, for example, of a bone, tendon, blood-vessel, or any other fibrous texture, preliminary stages are observed. The part at first is extremely soft, then firmer, and then fibrous, tendinous, or osseous. If any animal tissue be observed at a very early stage of growth, through a microscope, it is found composed of cells loosely cohering. The blood-vessels have the same character; they are extremely tender, and cannot be handled without rupture. As growth advances, the cells are found cohering more firmly, forming a species of cellular texture. At still more advanced periods, this cellular tissue is replaced by one fibrous, tendinous, and elastic. Analogously, it has been found, by examination at various periods after the accident, that fractured bones, ruptured tendons, and lacerated wounds, do not unite end to end at once; but that, in all such cases, union is brought about by a preliminary soft growth, which follows the course of original growth: and much evidence has been accumulated by microscopical observers, to the effect that injured textures are disposed or prepared for new growth by inflammation. The summary of the evidence upon this point may be expressed as follows.

Injuries and accidents after birth must happen to parts which are advanced in, or have completed, natural growth. A new growth is demanded for repair which requires new blood-vessels. And new blood-vessels cannot arise in parts complete in growth, without a previous change in their forms and qualities. It is a necessary preliminary of repair, that established blood-vessels assume the disposition and properties favourable to the offset of new vessels; for, without these, new growths cannot be maintained. And it is unquestionably the tendency and effect of inflammation to bring parts around an injury to the condition of an anterior form of growth. Blood-vessels and connective tissue recede, until their properties and structure are coincident with those of early growth; whereupon opportunity is afforded for new vessels and new cell-structure, replete with the energies of growth, to arise.

Case 1.—*Purulent Ophthalmia.*—The eyelids were swollen and closed. Upon separating them, a thick white matter (pus) escaped. The cornea was clear and bright, but surrounded by and almost buried in a red, highly vascular texture, composed of granulations, having a fleshy appearance. A small portion of this was snipped off; and, on submitting it to microscopical examination, the walls or coats of the blood-vessels were found quite altered in structure, greatly thickened, and composed chiefly of colourless cells.

Case 2.—A man died of consumption and pleurisy. On opening the chest, the pleura was found covered with a clear gelatinous matter, traversed by wavy bands of fibres and new blood-vessels. The size, number, and arrangement of these

vessels, bore no resemblance to those of the normal texture. In one place, fifteen large vessels were noted, running, within a very short distance of each other, a parallel course for an inch—a thing never seen in the healthy structure. With the microscope, multitudes of cells were seen in the walls of the vessels, and the smaller branches appeared no more than simple channels in the gelatinous material.

Inflammation, then, loosens the coherence of existing blood-vessels, cells appearing in the walls; upon which, the colourless elements or lymph of blood first, and afterwards the blood itself, finds outlets into new channels, organizing and sustaining new textures. These ends accomplished, if the process be connected with repair, inflammatory action ceases naturally, just as original growth ceases. And from this point, from the newly formed texture (the granulation texture), as a basis, a series of transformations in the natural order arise for cure,—fibrous, tendinous, or osseous, respectively, as may be required for the restoration of the injured structure.

That these changes of form, structure, and properties of blood-vessels—this yielding of established texture to new organisation and retrograde order of growth—does violence to the parts themselves, and to the system generally, seems proved by the pain, heat, feverishness, and excitement, which are attendants of the process. But, when for repair no further yielding is demanded, inflammation ceases, and symptoms decline: and subsequent transformations taking the course of natural growth, all remaining signs of the adverse or reverse changes incidental to the preliminary or inflammatory period disappear, superfluous elements being absorbed or removed.

III. *There are analogies between the elements of some of the fixed organs of the body and those of blood: and inflammation is preliminary of repair of injury inflicted on the blood.* The liver is composed of organised cells, and the fluid peculiar to them is the bile. Blood is composed of organised cells, and the fluid peculiar to them, that in which they swim, is the lymph or liquor sanguinis. The cells of the liver are fixed, and constitute a great organ of the body. The cells of blood are in circulation. But this physical or mechanical difference does not vary or diminish the vital properties of the elements, either of the liver or the blood. And the one is not more liable to injury than the other. The blood itself, therefore, may be injured. In accidents and wounds, injury is inflicted on the solid textures of the body; but the properties by which inflammation is established and carried on are in the blood. Blood-vessels extend throughout the whole body, therefore inflammation and its consequences may arise in any part of it. The common expressions—impure, deteriorated, contaminated, vitiated, impoverished, and poisoned—as applied to blood, include the idea of injury sustained by it, either wholly or in some of its parts. Blood is injured by the inhalation of chloroform, by improper substances taken into the stomach, and by noxious effluvia inhaled by the lungs, impure air, and epidemic miasms. It is injured by poisonous matter, absorbed by the abraded surface of the wound, and by suppression of the natural excretions. What are the phenomena of cure in such cases? That they are various and often complicated there can be no doubt. Blood is so intimately connected with all the secreting organs and outlets of the body, that it is often freed from injurious materials by their means. There is abundant evidence establishing this mode of relief of cure by increased exhalation and secretion. But leaving for the present this part of the subject, it remains to show that inflammation is a preliminary of repair of injury to the blood.

A paroxysm of *gout* is preceded by signs of disturbed health, which arise from the circulation of blood vitiated or injured by incongruous material mingled with it. The paroxysm consists of inflammation, usually in some joint, making its onset with great vehemence. By this, the blood-vessels are disposed to allow the fluid elements of the circulation a passage outwardly, and the outward matter leaves the blood and is deposited in the inflamed part. In *small-pox* a poison is introduced into the blood, which multiplies itself, causing injury. What is the consequence? Inflammation arises at various points upon the skin. The inflamed vessels lose their coherence, permitting a discharge of the poisonous matter. And when this has been thrown out from the circulation, inflammation and illness subside together. Poisonous material, from various sources, sometimes accidentally gains access to the blood, through an abrasion in the skin. Whereupon fever sets in, and there is inflamma-

tion not only in the wound, but in one or more distant parts of the body. The inflamed vessels furnish the required outlet to noxious matter; for, with the formation of abscess, and discharge of its contents, the symptoms of illness disappear. "Let us ask," says Sydenham, "what is the nature of plague? Is it aught else than a complication of symptoms, which nature puts in play, in order that through natural eliminations either in the way of abscesses or by help of some other form of eruption, she may expel from the body those infectious particles that we have taken in along with the air we breathe? And what is gout? It is a provision of nature to purify the blood of old men, and to purge the deep parts of the body. Such, at least, is the language of Hippocrates."

Blood, then, is injured by infectious and gouty particles; and inflammation, eruption, pustules, and abscess, are preliminaries or conditions of its repair or cure.

IV. *Inflammation is destructive to existing forms. It alters the function and uses of the parts in which it is seated. And yet it is beneficial, as establishing new growth necessary for the repair of solid textures; also as forming outlets, from the circulation, for injurious matter mingled with the blood.* Let us say a few words upon the two-fold aspect of inflammation as preliminary to the repair of injury. It is, on the one hand, clearly prejudicial to the vessels and textures in which it is seated, inasmuch as it renders them exceedingly soft and tender; it takes away their natural properties and uses; and gives outlet to the blood, which flows in new channels. On the other hand, the previous injury has been sustained, and these yielding, changes in the blood-vessels are necessary and beneficial. Because, 1. Repair of injury to the solid textures requires new growth, new growth requires new blood-vessels; new blood-vessels cannot appear except by a change of forms and properties in established vessels; and 2nd. Unhealthy or incongruous matter circulating in the blood, if not thrown out by a secreting outlet, requires a yielding of established vessels for its elimination.

V. *There are two stages of inflammation, each of which is sufficient for repair.* In a sprained joint the parts have been severely stretched or twisted. Inflammation and swelling arise; that is to say, there is hyperæmia and effusion of lymph. The effusion forms the proof that the tone of the inflamed vessels has so far yielded, as to afford to the fluid elements of blood an outlet through their walls. But as, in this example, there is no rupture of large tendons, ligaments, or bones, so there is no display of new growth demanded for repair. The stages and influence of inflammation are therefore limited. The coherence of the inflamed vessels yields only to the extent of effusion of lymph. And this done, what is necessary seems accomplished, for the vessels without further change recover their normal tone or constitution; whereupon elements of blood no longer pass outwards; on the contrary, the tendency is now inwards, and what has been effused, is absorbed as the swelling subsides. On the other hand, in more serious injuries, fractures, burns, and lacerations, new growth is demanded for repair, and inflammation follows ulterior courses. The coherence of the inflamed vessels yields not only to the extent of effusion of lymph, but further, so as to permit the blood itself, cells and all, to issue forth in new channels, organising new growth. Thus, by extent of injury, and requirement of repair, are established two varieties of inflammation. The one, which may be called the first variety, is limited to hyperæmia, and so much change of structure as allows of effusion of lymph; in the other, or second variety, the changes extend to the formation of new vessels sustaining new growth.

So likewise, from the phenomena, it would appear that lighter injuries to the blood are relieved by the first stage or variety of inflammation—such are *gout*, *rheumatism*, *measles*, and *erysipelas* and *scarlet fever* in their mild forms. But that graver injury demands the ulterior changes, and pus, which is a form of new growth, appears in *plague*, *traumatic fever*, *small-pox*, and the severer forms of *scarlet fever* and *erysipelas*. In all the circumstances relating to life there is a middle term, and it is that only which is co-ordinated with benefits to health. Too much and too little food is hurtful; too much warmth is heat occasioning burns and scalds; too little is cold producing chilblains and mortification. Health is in the middle,—a yielding of structure at either extreme; so it is with respect to inflammation itself. When injury has been sustained, a certain amount of yielding in vascular tissue subserves the purposes of cure and repair; but from this required amount, a deviation on either side is disease. It has been shown, that every part of the human body passes

through progressive phases of growth, and that every phase of growth has its forms of disease. And if in natural or original growth, we find monstrosities and defects —arrests of development and excesses—exuberance and deficiency; so *à fortiori*, analogous deviations may be expected in preternatural inflammatory growth. "If injured textures are repaired and wounds heal, *inflammation is healthy;* if granulations persist and will not heal, *inflammation is unhealthy;* and if normal textures continue to yield, giving way to inflammatory growths, *scrofulous disease is established.*

The general conclusion which arises from the establishment of the foregoing propositions, may be expressed as follows.

Vascular tissue, upon injury or irritation, recedes from its established or completed form to some anterior type of growth. Blood-vessels yield in structure; upon which follow effusion of lymph, and the formation of new vessels sustaining new growth. To this phenomenon the term inflammation is applied. The products or results are extremely various: repair of injury, elimination of morbid matter from the blood, pustules, abscess, ulceration, and scrofulous diseases.

"As to what may be the *essence* of small-pox, I am, for my own part, free to confess that I am wholly ignorant. Nevertheless, when I carefully weigh the evidence of the symptoms, it suggests to me the idea of inflammation. In clearing herself of this, nature is at work during the first two or three days, striving at the digestion and concoction of the inflamed particles, with the intention of afterwards discharging them upon the surface of the body, for the sake of maturation, and finally of expelling them from her boundaries under the form of little abscesses.

"We must, then, if we wish to make our *methodus medendi* the superstructure to a foundation in principles, recognise two periods in this disease: first, the period of separation; secondly, the period of expulsion.

"The first of these two periods is generally passed in a febrile ebullition, which usually is completed within the first three or four days. *During this stage nature is employed upon picking out and gathering together those inflamed elements which fret the blood, in making them over to the fleshy parts of the body, and in depositing them therein.* This being accomplished, she returns to her former repose, having allayed the tumult which was excited during her operations in the blood.

"When the ebullition has thus brought about the separation, the process of expulsion begins, and this continues during the remainder of the disease, by means of the little abscesses in the solid parts. These, inasmuch as they agree with a true abscess in character, pass through all the stages, viz., those of crudity, maturation, and excrescence. If all this is done properly matters are safe; upon its being done properly, however, all chance of cure depends; everything goes wrong when this is faulty."—(*Sydenham.*)

In local injury, the person at the time is usually in health. The blood is healthy; inflammation sets in with good materials. Phenomena take the normal course; and the cause of irritation ceasing, cure follows. But when the blood itself is suffering from injury or disorder, inflammation commences with degraded or imperfect elements; and, therefore, the action is more likely to exhibit irregularity in its course. When a bone has been broken, the case comprises: first, the fractured bone, with loss of use of the limb; and, secondly, the history of inflammation,—its course and transformations for cure. Analogously in epidemical disease, the illness comprises: first, the injured or contaminated blood; and, secondly, the history of some form of inflammation, eruption, pustules, abscess, or crisis, established for the discharge of the tainted particles. When a fracture occurs, we know what occasioned the injury, and we can see or determine the amount of mischief,—*simple, contused, comminuted, compound,* &c.; and thus we are prepared for an easy or protracted, difficult or dangerous process of cure. On the other hand, in epidemical diseases, we know but little, perhaps nothing, of the miasm, or of the amount of injury it has inflicted on the blood; and, therefore, there is here a very insecure ground upon which to base an anticipation of the coming lightness or gravity of the process of elimination or cure.

We have said that pus is a species of growth, and it remains in conclusion to show how this appears. Small colourless particles or cells are universally the primary elements of growth; they constitute the early forms of all animal textures and blood-vessels. Similar small colourless particles circulate at all times in the blood, and the more abundantly the earlier the period, or the greater the energy of the

growth. Such particles or cells accumulate in parts experiencing inflammation. They appear in the coats of the inflamed vessels, and loosen their texture; they exist in lymph, and are the prevailing elements of new growth; they also form the walls of the new vessels; and, lastly, they are extremely abundant in pus. These considerations, together with the determined manner in which pus arises and accumulates in spite of many obstacles of a physical kind, and the remarkable way in which established blood-vessels recede before it, prove that *growth* is the appropriate idea to be attached to the formation of pus. A retrogression of vascular tissue or inflammation precedes alike repair, new growth, abscess, pustules, and ulceration. It is in products or results that differences arise. In growth and repair, the new matter which follows upon the retrogression enters into new forms, new blood-vessels appear, and granulation structure arises. In abscess, pustules, or ulceration, it does not enter upon new forms, remaining fluid: so that when the wall of the abscess gives way it flows out as pus. In abscess or pustules, the purulent accumulation forms the prominent character; it occasions the swelling, whilst the receding changes in the surrounding vascular tissue are not so readily perceived; but in an open ulcer these constitute the prominent character, because the pus is continually disharged.

Finally, arsenic and mercury are virulent and destructive poisons; yet no one doubts that both may be so adjusted, in quantity and appropriateness of application, as to become most valuable medicines. So, analogously, of inflammation, no one doubts its destructiveness in respect of the vessels and textures in which it appears; yet still, adjusted by the hand of nature, in quantity and appropriateness, it opens the way to repair of injury to the textures of the body, and to the elimination of morbid matter circulating with the blood.—*Association Med. Journal, March* 31, 1854, *p.* 281.

161.—ON METASTASIS, IN ITS PRACTICAL BEARINGS.

By Thomas Hunt, Esq.

The object of this paper is not to demonstrate the existence, nor yet to investigate the nature, of that process by which disease is sometimes translated from one organ to another, but to inquire in what manner and to what extent the knowledge of such a principle or contingency should modify or restrain the practice of the physician and the surgeon.

That diseases do occasionally attack two or more organs or regions in succession, which have no direct continuity of parts, or special physiological relation to each other; and that, in this translation of morbid action from place to place, a mutation or conversion sometimes occurs, that is, a change in the character of the disease as well as in its locality,—are well established, and, as far as I know, universally admitted facts. The manner in which these changes have been explained by different writers has been sufficiently contradictory and confused, to show that the subject is imperfectly understood. Nevertheless, authors of repute have spoken with confidence, and even with dogmatism, of the "salutary" nature of certain diseases of local or superficial seat, describing them as protective and prophylactic, and as manifesting a condition of the system which forbids all interference, whether surgical or medical; inasmuch as they cannot be cured without risk of the morbid action being transferred to some important or vital organ.

In inquiring into the origin of this somewhat popular conceit, I was naturally led to place all the diseases described by different writers as salutary disorders in a class by themselves, in order to see if any common property could be found in each of them which would both identify them as belonging to so distinguished a group, and explain the nature of their protective power; but they do not appear to have any one pathological property in common. Some consist of inflammation, others of passive discharges; some affect the skin, some the bones, some the ligaments and muscles; others, as convulsive asthma, appear to reside in the nerves. At length, on reflecting upon their history, one feature, common to them all, disclosed itself, which seems to a great extent to explain the grounds upon which they have been pronounced salutary. It is simply this; *they are all difficult of cure.* Every disease, without exception, which it is thought dangerous to cure, lest some important organ

should become affected by metastasis—every disease considered protective, or salutary, or prophylactic, and thus to be regarded with a kindly feeling of forbearance on the part of the practitioner, and to be considered as a necessary but minor evil on the part of the patient—every one of these diseases happens to be *difficult of cure*. The following catalogue embraces most of them:—Gout, asthma, hemorrhoids, fistula, ulcers of the legs, otorrhœa, cutaneous diseases of the chronic class, especially eczema, porrigo, strophulus, lichen, prurigo, urticaria, herpes, furunculus, lepra, psoriasis. All these diseases are occasionally, and most of them always, difficult of cure: and each one of them has been accordingly assigned, by one author or another, to a class of diseases supposed to be salutary or protective to the general health, and which cannot, as a general rule, be cured without risk. The accomplished Dr. Parry, late of Bath, describes gout, asthma, hemorrhoids, and ulcers, as especially protective of the general health. Fistula and otorrhœa are, even to this day, often allowed to take their course, not unfrequently a fatal one, under the erroneous assumption that "something worse" would follow if these discharges were suppressed; and, as regards cutaneous diseases, the late Mr. Plumbe actually placed in a class by themselves the diseases, no fewer than seven in number, which he believed to exert "a probably salutary influence on the system." Rayer says: "The best advice which can often be given to patients advanced in life, and of infirm constitution, is, not to attempt the radical cure of such an infirmity as chronic eczema, if the disease be at all endurable. The removal of these natural(?) drains of the system is often followed by serious symptoms of a different kind." Mr. Erasmus Wilson coincides in this opinion, and both authors even propose to restore the eruption by counter-irritation after it has been cured, to prevent injury to the health, not only where such injury has actually taken place, but where there is reason to believe that it would result from the drying up of eruptive discharges of long standing; yet it is remarkable that eczema, the disease which both Rayer and Wilson regard as salutary in certain cases, is not included in Mr. Plumbe's list of salutary eruptions; neither is lichen, another disease which Rayer would prefer leaving to itself. If there is any truth in these doctrines, one cannot fail to perceive that this morbid provision for the safety of the patient is in general a sufficient safeguard, inasmuch as there are few diseases considered more difficult of cure, or less likely to be cured by ordinary treatment, than those which are thus said to be appointed to the guardianship of human health.

It cannot be denied, however, that there prevails in the profession a *suspicion*, to use no stronger term, that some of these diseases cannot in all cases be safely cured—a suspicion which has a most unhappy influence on practice, and which often prevents that steady perseverance in the treatment of chronic disease which is always essential to success; yet a suspicion which, if well founded, cannot too soon ripen into certainty, and, if not well founded, cannot too speedily be scattered to the winds.

That this question is worthy of calm inquiry, may be proved by the consideration that, while we are hesitating, the quack is acting. Reckless of consequences, he pushes his specifics, and in some instances cures the disease, with impunity certainly as regards himself, and possibly as regards the patient; while we are shaking our heads over the case, and hesitating lest even our legitimate practice should be brought into discredit by sequences which are too often mistaken for consequences.

The class of facts which are appealed to in support of this dogma are of this kind. We are told that we often succeed in relieving internal congestion by external stimuli, used for the purpose of producing what we call counter-irritation. And further, if a congestion is chronic in its nature, we are sometimes enabled to unload the vessels of the affected organ by establishing a discharge from the integuments, by means of blisters, tartar emetic ointment, issues, or setons. The current of the circulation is thus diverted, and the patient is relieved. Hence we are apt to conclude, *vice versâ*, that, in an opposite case, a drain being already established through an ulcer, eruption, or muco-purulent discharge, if this drain is stopped, the current will be reversed, and some internal congestion must be expected to take place as a consequence. I submit, however, that the premises do not warrant such a conclusion; neither is it borne out by facts. It is a very low and mechanical view which some writers take of the causation of disease, when they attribute it solely or chiefly to increased momentum of the circulation. It is as fallacious as attempting to

explain the theory of digestion by regarding the stomach as an alembic. The fallacy of deducing the theory of metastasis from the results of counter-irritation may be thus exposed. There is in every patient, as a general rule, a tendency towards health, an inherent power which only requires certain conditions and opportunities for action, and the patient recovers. Now, take a case of internal congestion. The circulation of the part is clogged; the vital stream is interrupted in its flow; there is not room for the transmission of the proper quantity of blood in the proper time. Divert the current of blood, congest the veins of some part not in the immediate vicinity, and you reduce the quantity of blood to a stream which can pass freely, and the part is thus enabled to recover; but when it has recovered, it must be supposed to have resumed the tone and vigour of health; consequently, if your issue is allowed to dry up, you do not, *cannot* thereby create disease in a healthy part, provided the quantity of blood in the system, and its quality, are both equivalent to the requirements of health. It is, therefore, in the highest degree unphilosophical to suppose that healing a blister, or an eruption, or an ulcer, seton, or issue, can, under ordinary circumstances, create disease, or even re-establish bygone disease.

But it may be replied that such occurrences have taken place, and that these have been sufficiently frequent, and sufficiently varied in their character, to establish a sort of rule which should put us on our guard. Patients, in whom inflammation of the lower extremities, with œdema of long standing, has been indiscreetly treated with cold lotions and bandages, have been for the first time seized with epilepsy. Others, having suffered for years from ulcerated legs, have been seized, upon the healing of the ulcers, with sickness and vomiting, headache, and giddiness, or fatal disease of the heart. In two cases, related by Dr. Parry, the immersion of a gouty foot in cold water, which produced instant relief from pain, and a proportionate abatement of the inflammation, was, in a few hours, followed by hemiplegia. And it is granted that the cure of cutaneous diseases has been known to issue in different forms of dyspepsia, asthma, and dropsy. In each of these cases, possibly, the treatment was mischievous; and, if no better treatment could have been devised, it were better to have left the patient to his fate. But granting all this, it may still be denied that the treatment was anything more than the *occasional* cause of the disease.

It has already been observed that, as a general rule, there is in patients a strong tendency towards health. But some patients are too weak to be healthy. There is something deficient in their *vis vitæ*. In them, the functions of life cannot all of them be healthily and vigorously performed for any great length of time together. One organ or another flags, as if it required rest. In some, the action of the heart intermits every twenty or thirty beats; in others, the stomach flags, and fails to digest the food; and in many, the bowels become weak and will not act, or the kidneys fail, and gout or dropsy supervenes. After a fit of illness, these patients often recover for a time, and begin to think they are strong and hearty; but soon they fail again. Now, it is this class of patients who are subject to metastasis, revulsion, translation, reaction of morbid irritability, and overy other kind of morbid change. These individuals are few in number, as compared with the mass; but, as they figure largely in the daily list of patients, it becomes a practical question, how we can treat their more ordinary, and especially their local infirmities, without seeming to light up the latent embers of organic disease into a flame.

And this is the practical question which it is proposed to discuss. What is the safest mode of treating local disease, when there is any real or imaginary danger of metastasis?

In the first place, ought such disease to be left to nature? Surely, it becomes those who maintain this, to point out the general limits to which our interference with disease ought to extend. There is, perhaps, no one disease which has not been known to give place to another; and, unless we have some means of ascertaining that the disease which may succeed will prove less serious than the existing one, we ought on this principle to leave all disease to nature. This reasoning would at once, if carried out, put an end to the practice of medicine. It is a refinement of the homœopathic doctrine, in which we are charged with creating more diseases than we cure. In truth, to shrink from the treatment of local disease, under the influence of an undefined apprehension of serious consequences to the general health, shows either an ignorance of the resources of medicine, or a great want of confidence in those resources.

At the same time, it must be admitted that there is a way of treating local and superficial disease, which may possibly so act on the general system, as to develope organic disease, when there exists a predisposition to it. I refer to *local and discutient treatment, pursued regardless of the general health.* And to this I should feel disposed to attribute, not only the results of the cases alluded to, but *every unfortunate case of metastasis occurring in consequence of medical treatment.* And this remark applies not only to local diseases of constitutional origin, but also, in some degree, to local diseases of local origin; inasmuch as some of these, from their severity or duration, may at length implicate the general organism, and then local treatment may embarrass the circulation: whereas the cure may be very safely accomplished merely by assuming the existence of some general lesion, and combining with the topical treatment such a course of alteratives as shall effectually excite into activity such of the secreting organs as can readily be stimulated by medicine.

Questions of the following nature are often put by patients. One inquires, Is it safe to allow this issue, or that seton, to heal? Another says, I have had an ulcer on my leg for many years, and I am told if it heals I shall be struck with apoplexy: is it safe to heal it? A third has an extensive eruption, which has rendered life miserable for many years, and he is told that it is a great preservative of health, although he is, perhaps, all the while, conscious of very much impaired health; he asks if it is right or prudent to use any means for the restoration of his skin, or whether he is doomed either to carry his affliction to the grave, or to seek relief at the peril of his life? Now, the answer to all these questions will, I apprehend, depend upon the antecedents in each case. An issue, or a seton, which has manifestly been useful in relieving the patient of some local congestion or lesion of magnitude, may be allowed to heal at any period; provided only, either that some satisfactory evidence can be produced that the tendency to disease has subsided, or else that the discharge may be compensated for by a scale of diet or regimen, or a course of alterative remedies, which shall supersede it in effect. And what is true of these artificial drains, is also true of morbid discharges of spontaneous origin. This ulcerated leg, or that eruption, may be very safely healed, not only without danger to the general health, but probably to the manifest improvement of it, provided it is healed, not as a navigator would dam up a leak in a reservoir, but as a skilful physician would heal every curable disease, both within and without, by adapting his treatment to the infirmities of the constitution, particularly with reference to the glandular system, the digestive organs, and any other part of the economy which is liable or likely to become defective or redundant in its action, or lame and irregular in its functions.

To enter into detail on this part of the subject, would be the same thing as writing a treatise on the modern practice of medicine. For nearly all the cases which are liable to metastasis are to be found on the broad and hazy line which unites rather than separates the practice of physic and of surgery.

In requesting to be favoured with the views and experience of my brethren, on this important practical question, which concerns the treatment of disease generally, in all its departments, medical and surgical, I may be permitted, in conclusion, to say that my own experience has fully established the conviction that the safest as well as the most satisfactory method of treating local disease is to treat it constitutionally—to attack it from within, not from without: and, in cases where there is no necessity for surgical or mechanical treatment, to abstain as much as possible from local medication, or at least to make it secondary to the general treatment, and directed rather to the relief of pain or irritation than to the ultimate cure of the disease.

Three classes of facts which have come before me, have confirmed these views:—

1. Although it has occasionally happened to me to be told by a patient suffering under skin disease, ulceration of the lower extremities, or other local ailment, that his health is *now* very good, but that before the local disease broke out his health was unsound; yet, for one case of this kind, I have met with at least a hundred in which the reverse has occurred—the health has been very good until the local disease appeared, the general disorder then accompanied the local, and on recovery from the latter, the patient has also recovered his general health.

2. In the exceptional cases—that is, where improved health has appeared to

result from the outbreak of eruptive or ulcerative disease, and where, if anywhere, local disease might be presumed to be salutary or protective, and therefore not to be cured without risk—I have invariably treated the disease (which, if eruptive, is generally some form of eczema), not by external medication, but by purgatives, diuretics, sudorifics, and every kind of treatment calculated to augment the excretory discharges: at the same time enjoining active exercise and abstemious diet. And the same line of treatment has been pursued during the healing of ulcers on the legs, when they have appeared to be the means or condition of improved health. And whenever this plan has been adopted, I have never, in any single case, known the health to become deteriorated in consequence, nor have I ever seen any reason to regret the healing of these so-called salutary eruptions and protective ulcerations.

3. I have observed, in common no doubt with many of the Fellows of this Society, that in those rare cases in which a strong disposition to metastasis has been manifested in the system, it has been connected with a peculiar idiosyncrasy, to which no general rules either of theory or practice can possibly apply; and in these cases, *medicine seems to have no power to control disease.* The viscera and the tegumentary tissues are affected alternately, and no human means can either drive the disease from one part of the system to another, or effectually check its violence in any organ or tissue which it may happen to attack. Thus, I have known a case in which every year a child was attacked with an eruption of boils on the scalp, and, as these subsided, the brain became affected; both the boils and the cerebral disorder became more severe every year, until the disease ultimately proved fatal. No medical treatment appeared of the slightest use. The disease always took its course. The membranes of the brain were found inflamed after death.

These cases of metastasis are extremely rare as compared with the mass. Yet, because they are rare, they are very generally published; whereas, the every-day cases, those in fact which establish the rule for practice, are not published because they are common. The result is, that cases of metastasis are thought to be much less rare than they really are.

As a general rule, then, I greatly question the existence of any disease which is essentially of a salutary nature, or which is of so much importance as a safeguard to health, as to deserve the slightest consideration, as such, at our hands. We meet with cases, indeed, in which perfect health cannot long be maintained; disease will appear in some form or in some locality, attacking sometimes one organ or structure, sometimes another, sometimes both at once; yet even here medicine is not altogether at fault; judicious and rational treatment will generally improve the health of the most unhealthy.—*Association Medical Journal, May 5, 1854, p. 390.*

162.—ON GALLIC ACID IN THE HEMORRHAGIC DIATHESIS.

By Dr. William Bayes, Physician to the Brighton Dispensary.

[When we wish for a local styptic or anti-hemorrhagic remedy, we may use tannic acid; but this becomes changed into gallic acid in its course through the system to any distant part, and therefore it is better, perhaps, to use gallic acid at once. Gallic acid enters the blood rapidly, and in a case of Dr. Bayes', the bloody sputa became perfectly *inky* in less than two hours from the first taking of gallic acid, the hemorrhage at the same time ceasing.]

The effects of tannic and gallic acid on dead animal tissues are familiar to us in the process of tanning. It contracts and hardens the dead animal tissue. Experiments have also shown that a saturated solution of gallic acid, mixed with fresh drawn blood, both quickens coagulation and hardens the clot. Solutions of vegetable astringents thrown into the veins of animals have produced death through coagulation in the central organs of circulation; and since the astringent power of all vegetables depends upon the amount of tannic and gallic acid contained in them, and since we have seen that gallic acid readily enters the blood, I think we may fairly assume that, when introduced into the human system, gallic acid acts through its tendency to contract and condense the fibrous tissues, and to coagulate the vital fluids, wherever they have this natural disposition.

Affecting, therefore, as it does both the circulating fluid and its containing vessels,

giving an increased proclivity towards contraction in the one and towards coagulation in the other, this twofold effect of gallic acid, and the perfect safety with which the largest dose may be administered, should render it our sheet-anchor in hemorrhage, both active and passive.

The ease and rapidity with which it enters the blood render it equally powerful to arrest hemorrhage throughout the whole body. If the vital stream be oozing in passive flux from relaxed and atonic vessels, it will contract them and steady the circulation; or if a bounding pulse forces an active hemorrhage through the torn walls of some weakened vessel, it will contract this, and form a firm clot around it, reducing the pulse and the heart's action at the same time.

Where an overdose of gallic acid has been taken, the pulse becomes small and wiry, the face grows pallid, and a whizzing sound is heard in the ears, the head feels dizzy, and faintness supervenes. These symptoms arise from the contraction of the capillaries and larger vessels produced by the astringency of the remedy, and the consequent interference with the intra-cranial circulation. But I conceive it would not be possible to poison a patient by the largest dose of the remedy when introduced by the mouth, because the capillary and distal vessels would become too contracted to receive any further supply, long ere a sufficient quantity could be carried into the blood to produce dangerous effects upon the central circulation.

The ease with which gallic acid enters the circulation renders it a most valuable remedy, not merely in hemorrhages, but also in those diseased habits of body in which excessive secretion becomes a prominent and distressing symptom, and also in some other diatheses, which I will proceed to enumerate.

Dr. Scott Alison, in a paper on Tannic Acid, which appeared in the 'London Journal of Medicine,' for Jan. 1850, has recommended its use as a tonic in dyspepsia—as a nervine in languor and debility—as tending to arrest tubercle and malignant disease—and as valuable in rickets, in phthisis, in bronchitis, when accompanied by profuse expectoration, and in arresting colliquative diarrhœa and exhaustive sweats.

Dr. Burns, of Glasgow, praises the powers of gallic acid in chlorosis, in doses of one hundred grains daily. From a consideration of its effects upon contractile tissues and upon the various solid and liquid constituents of the animal body, we should anticipate benefit from the use of gallic acid wherever relaxation of the one or too great fluidity or dilution of the other were apparent, and experience fully testifies its virtues in the very classes of disease where advantages from its exhibition were predicated.

In the following classes of disease the remedy is worthy of every confidence.

I. In active hemorrhage, from whatever organ or part of the body they proceed.

II. In passive hemorrhages, hemorrhagic diathesis, purpura, &c.

III. In excessive secretions, as in pyrosis, serous diarrhœa, chronic bronchitis, bronchial flux, profuse night sweats, some forms of dysentery, and perhaps in diabetes.

IV. In atonic states of the alimentary canal and of the body generally, especially in rachitis, and in those cases where a weak and congested state of the capillaries is apparent.

V. As an adjunct to other means in piles, wounds, &c.

[Although gallic acid is thus extolled by Dr. Bayes, we have found it inferior to turpentine when the bleeding is from the alimentary canal, and to lead when the hemorrhage is pulmonary.]—*Association Med. Journal, June* 9, 1854, *p.* 506.

163.—*Hemorrhage from the Jejunum.*—By Dr. R. B. Todd.—Though the theory of hemorrhage by exhalation is somewhat surrounded by obscurity, we now and then meet with cases which afford striking proofs that the blood cannot have escaped by any other way than exudation; and among these instances a perfectly unbroken intestinal mucous membrane after death from melœna, is one of the most unanswerable. When this form of passive hemorrhage takes place in a patient debilitated by want and mental distress, we are naturally led to establish a comparison between the intestinal hemorrhage and purpura. For it is plain that both pathological phenomena are originally the same, the difference being simply that the epithelium offers

less resistance to the passage of blood than the epidermis. No doubt but the fluid of perspiration finds its way through the cuticle, but we may suppose that in diaphoresis there is more force acting from within and more relaxation of the vascular network.

To return to hemorrhage from the bowels, it may be stated that cases of this kind are of a very distressing nature, both as regards the patient and the physician, since it is extremely painful to the latter to see the life-blood oozing from the system, and to observe the inadequacy of the means adopted for the control of the hemorrhage. And the task becomes doubly difficult in cases where bad nourishment and depression of mind have been principally conducive to the hemorrhage, for it may be supposed that in such cases two powerful causes are acting at the same time—viz., increased fluidity of the blood, and want of tone in the vessels. It is evident that astringents, topically used, can only act upon the vessels of the affected mucous membrane; and the aqueous condition of the blood remaining the same, it cannot excite surprise to see the best directed means for arresting hemorrhage proving useless. It is here that prevention should be thought of, for by proper medicines and diet the conditions of the circulating fluid may be greatly improved, and this favourable result might in many instances altogether prevent, or render but very trifling, an attack of intestinal hemorrhage.—*Lancet, June* 17, 1854, *p.* 634.

164.—ON UTERINE POLYPUS.

By Dr. Robert Barnes.

[The following remarks ought to have accompanied Dr. Barnes' other papers at p. 210. Polypus uteri is a term used perhaps too carelessly to describe affections of very different kinds; sometimes cancerous and cauliflower excrescences are erroneously called polypi.]

The classification of polypi admits of more precision than I think has hitherto been observed. On an analysis of cases occurring in practice it will be found that polypi take their rise from three sources: from the blood-vessels lying under the mucous membrane, or from the mucous membrane or sub-mucous fibro-cellular tissue. I believe that a division thus founded upon the origin is at once the most scientific and the most practical.

1. *On Polypi springing from the Muscular Wall of the Uterus.*—The polypi which take their rise in the muscular wall of the uterus have been commonly called "fibrous." Recent investigations have, however, proved that they really consist in an abnormal development of muscular fibre, their structure essentially resembling that of the muscular wall of the uterus itself. This muscular character of the so-called "fibrous tumours" and "fibrous polypi" of the uterus, both in the unimpregnated and in the gravid womb, was first, I believe, distinctly proved by Vogel, and figured in his admirable work. Cruveilhier had, however, previously observed that there were "hard polypi," which consisted in hypertrophy of the tissue of the uterus—such is the one figured pl. vi., liv. xiv—and others consisting of fibrous bodies developed under the uterine mucous membrane. The celebrated French pathologist thus describes the structure of the polypus referred to: The figure represents an antero-posterior section of the polypus and of the fundus of the uterus. The tissue of the polypus is seen to be continuous, without any line of demarcation, with the proper tissue of the uterus; it is a prolongation of this proper tissue, and not a fibrous body developed in the thickness of the uterus, capable of being separated by enucleation. The identity between the tissue of the uterus and the tissue of the polypus is such that the closest examination does not reveal the slightest difference."

Cruveilhier does not appear to have suspected that the ordinary fibroid tumour, distinctly defined from the proper uterine tissue, and capable of enucleation, might also consist of muscular fibre in every respect resembling the muscular fibre of the uterus.

As this discovery is of especial interest in relation to the pathology of uterine tumours and polypi, I think it of importance to translate Professor Vogel's account at some length. In describing fig. 5 of pl. ix., he says, "It represents the pri-

migenous cells of a fibrous tumour arising in the uterus. These cells are doubtless the rudiments of organic muscular fibres, which but rarely come under observation. The history of the disease is as follows:—A single woman, aged forty-four, was admitted into the hospital, after having suffered for several years from a fluctuating swelling in the right side of the abdomen, which hitherto had caused no pain, a dragging sensation, as if the patient were about to bring forth, only excepted. Suddenly violent pains came on in the abdomen, increased by pressure. These continued, notwithstanding energetic treatment by bleeding and emollients. The patient died after three days.

"The autopsy gave the following results:—The omentum was considerably thickened on the right side, and adherent both to the abdominal walls and to a hard tumour, which reached below into the pelvis, and was of the size of two fists. The tumour was knobby on the surface, of a white colour, and hard to the feel; it was intimately connected with the fundus of the uterus, from which it seemed to spring. The inner surface of the uterus appeared natural: the mucous membrane unaltered. In its cavity, however, was a round tumour, the size of a billiard-ball, hard, of a bluish-white colour, and covered with a yellowish purulent matter. It lay free in the cavity of the womb, without any attachment to the walls. The substance of the uterus was much thickened; the thickness was not uniform, but in many places it was three inches. In the substance were many round tumours, of the size of a pea, of a bean, of a walnut, up to that of a billiard-ball. These tumours were for the most part free, or at least could be easily detached from the surrounding substance of the uterus: they were of a white colour, and very firm texture. They were in general bullet-shaped, but most of them were knobby. On section, they showed interiorly the same hard, glistening tissue as the outside: but a fibrillation or distinct texture could not be distinguished by the naked eye. The large tumour first described, arising from the fundus uteri, was softened in parts; it exhibited irregular, eroded excavations, traversed by bands, which were soft at the surface, but hard in the interior. These cavities were, some empty, some filled with a soft blood-coagulum, or a greasy, purulent, gray matter. The softened tumour was broken above, and had emptied a part of its contents into the cavity of the peritoneum, whence secondary peritonitis and death.

"Another tumour, of the size of a pigeon's egg, was found in the vaginal portion of the uterus; this was softer, and could not be separated from the uterine substance. It was white, and consisted of a fibrous web with large meshes filled with a thick albuminous fluid.

"A more minute examination of these parts by the microscope gave the following results:—The substance of the uterus consisted of the ordinary organic muscular fibres. In some places brownish cells in great numbers were found between these fibres.

"The mass contained in the cavity of the uterus showed quite the same structure, the same fibres, and similar brown cells. The tumours contained in the substance of the uterus, both great and small, were exactly similar. The upper softened part of the large tumour contained, together with blood-clots and isolated blood-corpuscles, many pus globules, which underwent the ordinary change with acetic acid. The softening of the tumour was plainly occasioned by inflammation. The traversing bands and threads in the cavities of the softened portion were recognised under the microscope as the *débris* of organic muscular fibres and connecting tissue, which had withstood the destruction of the surrounding tissue. The softer tumour in the vaginal portion appeared to be a fibrous tumour; the fibrous portion consisted of organic muscular fibres and connecting tissue; the albuminous-looking fluid in the meshes showed numerous round or elongated cells, sometimes solitary, sometimes in groups, probably primary cells, which, later, would have been developed into muscular fibres.

"Some of the tumours contained in the substance of the uterus were carefully enucleated, cut into small pieces, repeatedly washed in water, and submitted to chemical investigation. They were gradually dissolved in boiling concentrated muriatic acid; the solution was colourless. In acetic acid they swelled up, became translucent and gelatinous; but a perfect solution was not obtained at the end of a week's treatment.

"Similar pieces, carefully washed with water, then dried between pieces of blot-

ting-paper, were weighed, and completely dried at a temperature of 100° in a water bath, and again weighed; 1000 parts of the fresh substance gave 220 parts of dried residuum."

The next figure described by Vogel is that of a similar muscular tumour taken from the hypertrophied muscular coat of a man.

Another figure is taken from a fibrous tumour of the uterus of a woman who had died from heart-disease. The tumour was of the size of a walnut. "Its texture had altogether the ordinary character of fibrous tumours. Portions taken from the interior showed very beautifully the development of fibres out of cells: many elongated nucleated cells were bound to a fibre which in every respect resembled the ordinary fibres of organic muscle."

[Dr. Oldham was one who very early recognised the muscular structure of fibrous tumours of the womb. This will be found in Guy's Hospital Reports for 1844. It is the well known case of Dr. Crisp, who found a tumour in the womb after delivery which could not be extracted. The woman died collapsed.]

Dr. Oldham says, "the prevailing tissue (of this tumour) was a clear unstriped fibre, which when examined with a portion of the muscular fibre of the uterus, differed only in the latter being more full of cells and blood-corpuscles, which rendered its definition as fibre less distinct than the former." In 'Guy's Hospital Reports,' for 1852, Dr. Oldham again refers to the conclusions expressed above as to the muscular character of fibrous polypi.

In February, 1851, an opportunity occurred to myself of verifying this point in the pathology of fibrous tumours.

Case 3.—On the 6th of February of that year, Mr. Chance sent me the uterus of a woman who had been brought into the dissecting-room. It had been cut open by a student. A tumour the size of a filbert had been attached to the orifice of each Fallopian tube. When sent to me, that from the right side had been removed. The other had been cut through, but its attachments were preserved. It appeared to be connected with the uterine walls by fibro-cellular adhesions, such as might easily have been lengthened out into a pedicle, permitting the tumour to escape from the uterus in the form of a polypus. The orifices of the Fallopian tubes were completely obstructed. The tumour remaining appeared to be of a fibrous structure. It was submitted to microscopical examination by Dr. Hassall and myself. It was made up of fibres in every respect resembling the organic muscular fibres of the uterus from which it was taken. I exhibited the specimen to the London Medical Society on February 17th following, stating the opinion Dr. Hassall and myself had formed as to the muscular character of the tumour. The report of the proceedings was published in 'The Lancet' at the time.—*Lancet, June* 17, 1854, *p.* 635.

165.—*On Pneumonia.* By —— Martyn, Esq.—A man twenty-one years of age, said to be of delicate constitution, was seen by Mr. Martyn on the 8th March, 1852, having then been ill nine days, not wholly without treatment, but the true state of the patient apparently not recognised. He now presented all the signs of pneumonia, general and physical, and in an extreme degree, the right side being wholly without natural respiration. The constitutional distress was very great, the patient being unable to lie down, or to turn in bed. "His countenance was flushed, puffed, and anxious; skin hot and dry; respiration sixty in a minute; pulse 120; with great pain in the right side, and incessant short cough, with scanty, red expectoration. He expressed great exhaustion, was restless, and dull, and indifferent to his situation. His life seemed absorbed in the labour of breathing and coughing." Mr. Martyn was deeply impressed with the marks of want of power presented by this case, which forbad the employment of antiphlogistic measures, lest even a slight shock might precipitate the patient altogether. The following plan was prescribed: —one ounce and a half of sherry wine every two hours, and good broths and milk. Sesquicarbonate of ammonia, five grains; potassio-tartrate of antimony, one-sixth of a grain; nitrate of potash, ten grains; spirit of nitric ether, twenty minims; water: make a mixture, to be taken every two hours; also a pill, containing three grains of calomel, and one of opium, in the morning and at bed-time.—A large blister was applied to the chest. The following day the respirations had fallen to forty-six in the minute, pulse 110; the exhaustion was still fearful. To continue the plan, substituting chloric ether in the mixture for the nitric ether, and one dose of calomel

and opium at bed-time. He had a second blister on the following day, and went on slightly improving, except that he had scarcely slept at all. But little other change took place till the 12th, when some degree of pneumonia was detected in the left side, the exhaustion being still very great, and the pulse more than 100. A third blister was applied, and the wine and other nourishment continued with little interruption. His improvement was progressive, although many weeks elapsed before his recovery was perfect. He has since been as well as ever. This case appeared to the author to be suggestive of some such reflections as the following: From how very extensive and grave an inflammation of the lungs a patient may be rescued, whilst in another a much less degree will destroy life. Here was a case, in which the whole of the right lung was affected throughout, even to dulness on percussion; and the left to some extent, posteriorly and below, was dull, and characteristic small râle existed in it, and life was carried on at the labour of sixty respirations in the minute. To keep the patient going, some active plan was demanded, and stimulants appeared to present the only hope. This was the second and chief point to which Mr. Martyn wished to draw attention. He thinks the case goes far to establish that stimulants do not necessarily disagree with, and aggravate all cases of pneumonia, and that it is confirmatory of the observations of some of the best authorities, on the high value of stimulants in some grave forms of inflammation. Upon the whole, perhaps, especially in inflammation of the chest, most men would be shy of stimulants. But in the case now related, not only were they borne well, but seemed directly to refresh and relieve. At the outset of inflammation, especially of the chest, stimulants may be uncalled for, but it does not follow that the opposite plan is to be unduly persisted in. The time comes, in all cases of some duration, to regard the wants of the system. The general nutrition of the body must be attended to; we dare not exclude any element of food essential to the economy for many days together; the vital forces soon suffer. In conclusion, Mr. Martyn inquires, have not the present generation of medical men been taught too much the *material treatment* of disease—too much that *drugs cure*—that Nature must wait upon them; and that the cure of inflammation in British practice, had come to mean bleeding, mercury, and such treatment? But, at the same time, who had not seen that in many cases the remedies had proved worse than the disease?—*Lancet, April* 29, 1854, *p.* 472.

166.—*Sickness in Friendly Societies.*—At the last ordinary meeting of the Institute of Actuaries, held at the rooms of the Institute in St. James's Square, John Finlaison, Esq., President, in the chair, a paper was read entitled "Observations upon the Sickness and Mortality experienced in Friendly Societies," by Henry Tompkins, Esq. The author commenced by urging the importance of correct data as to the law of sickness. It was more difficult to determine than the law of mortality. Mr. A. G. Finlaison, in his recent report made by order of the House of Commons, upon the sickness in Friendly Societies, in which associations nearly three millions of persons were enrolled, had determined the ratio of sickness with a greater approach to accuracy than had ever been accomplished before. Previous investigations had included claims made on account of old age with those of sickness. This had led to erroneous results. Thus at age 60 every man had been stated to be ill on an average one month in the year; at 70, two and a half months; at 75, four months; at 80, five months; and at 85, six months. In consequence of the promulgation of this erroneous data, it was supposed by many that Friendly Societies in general were in an insolvent state. The author proceeded to analyse Mr. Finlaison's report, from which it appeared that between the ages of 20 and 30 each person had on an average nearly 7 days' illness a year: at 40 it increased to 8 days; at 45 to 9; at 50 to 11½; at 55 to 14; at 60 to 18¾; at 65, to 27¼; at 70, to 43½; at 75, to 66; and at 80, to 97½. It was stated that the total amount of the sickness experienced by each person in the 32 years elapsing between 18 and 50, was 260 days; in the ten years between 50 and 60, 147 days; between 60 and 70, 299, and between 70 and 80, 712. Locality had very little to do with the ratio of sickness. It had also been found that the duration of sickness was in the inverse ratio to susceptibility of attack, as shown in the proportion of persons attacked with sickness. Thus in the northern parts of England nearly 20 per cent. were yearly attacked with illness, while in the midland districts 30 per cent. were so attacked;

but, on the other hand, the proportion of sickness to each sick man in the north amounted on the average to 50 days, while in the midland districts it reached only to 36 days; the liability to attack being balanced by the duration, thus making very little difference in the amount of sickness experienced in one part of England when compared with another. With regard to the mortality experienced in Friendly Societies, it was very light, there being at age 20, only 1 death in 135; at 30, 1 in 130; at 40, 1 in 97; at 50, 1 in 67; at 60, 1 in 38; at 70, 1 in 18; and at 80, 1 in 8. This part of the inquiry coincided with previous investigations almost to repetition. The report divided labour into two classes,—viz., light and heavy labour; and each of these again into the two sections—labour in the open air, and labour under shelter; and the results showed a great difference in their ratios of sickness and mortality. The most healthy condition of existence appeared to be "light labour in the open air." The author concluded by enunciating the theory, that the duration of sickness was in direct proportion to the vital power. Thus, let Sx the sickness, be multiplied into the numbers out of which one will die, or intensity Lx, it should produce a constant number C. It was stated that tables of payments required in Friendly Societies, calculated from the report by Mr. Finlaison, were in the press.—*Med. Times and Gazette, July* 15, 1854, *p.* 56.

167.—ON THE MEDICAL AND POISONOUS ACTION OF ALCOHOL.

By Dr. H. Bence Jones, F.R.S., Physician to the St. George's Hospital.

Alcohol, like most, if not all stimulants, does not act on muscles and nerves with more energy the greater the quantity of the stimulant taken. When a great excess is administered, it acts like cold or heat externally, or like ether or chloroform internally when inhaled. In great excess these substances cease to act on the nerves and muscles as stimulants, but, on the contrary, they produce insensibility, depression, coma, and death. During the course of these lectures, I shall have occasion to dwell on the same double action as exhibited by quinine, which acts in small doses as a stimulant, and in large doses produces the most remarkable depression of the nervous system. In digitalis, and even in opium, the same double action may also be observed. In addition to the stimulating action of alcohol on the muscular structures and nerves, it acts through the circulation on every tissue. It produces a chemical effect. This is best seen when it is used for the preservation of morbid specimens. Even in the living body, this action may be observed, but it cannot be separated from the effect produced by the stimulating action. Thus alcohol hardens the skin, when applied locally. It hardens and thickens the stomach. When long continued it thickens the cellular tissues of the liver. It passes off by the lungs and kidneys, and affects both organs. Ultimately it lessens the amount of carbonic acid expired, and hinders the removal of the products of decomposition by the urine. It changes the composition of the blood, and affects the nutrition of every organ and structure of the body. It has its own peculiar poisonous action on the muscles and nerves. As a poison it has been placed by its effect on the nerves and muscles between ergot of rye and arsenic. My time will not allow me to give you many illustrations of the general paralysis of the nerves and muscles, which results from the excessive use of alcohol. I must refer you to the work of Dr. Magnus Huss, from pages 50 to 75. I may shortly, however, mention the following case, which was admitted into St. George's Hospital, December, 1853. A bricklayer, aged 32, had habitually taken large quantities of stimulants, as much as five or six quarts of beer, and half a pint, and even more, of gin daily. About six weeks previous to his admission he was attacked with numbness of the right leg from the ankle to the knee; a few days after the left leg became numb; after a week, the numbness, with formication, commenced in both hands; after about five weeks the nose and both cheeks became numb, and increasing loss of power in the extremities obliged him to give up work and come to the hospital. He had a similar attack six years previously, and recovered after thirty-six weeks' treatment. On admission there was scarcely any feeling in his legs nearly to the thighs, nor in the arms up to the elbows. Both sides of the face and nose were also numb, but the forehead was free. He had difficulty in walking, and had lost much of his manual power; for the first six weeks he continued to become more and more paralysed; becoming

unable to move his feet, and having very little power over his hands. After this his hands began to regain their strength. In three months after full doses of tincture of cantharides, and extract of nux vomica, he was enabled to walk, and he left the house to return to his work four months after his admission. The resemblance of this case to one given by Dr. Huss, in which the paralysis was caused by ergot of rye, is worthy of your notice.

A woman, aged 48, having been perfectly well ten weeks before her admission, was taken suddenly ill at night with violent sickness, which lasted two days. She had pain in the head, and a feeling of weakness in the limbs. She remained weak for eight days; she suffered from formication in the extremities, with pain sometimes amounting to cramp. Unsteadiness of the arms and legs succeeded, so that she could scarcely walk, or pick up things. This increased to perfect paralysis of motion and sensation, without any pain in the head or back, the bowels acting regularly. Other persons in the same village were affected in the same way. With baths and strychnine in two months she recovered the use of her limbs.

I need not here dwell on the paralysis which is caused by arsenic, still less on that produced by lead. Phosphorus also is said to produce the same symptoms of paralysis. For the difference between the paralysis from these different causes, and for the distinction between the general paralysis of the insane, and the general paralysis from alcohol, I must refer you to Dr. Huss's work.

Alcohol is the chief poisonous ingredient of unadulterated fermented liquids. Like all poisons, when not taken in too large a quantity, or too long continued, it has a medicinal action, on which its dietetic value depends. This action may be summed up in one word, stimulant.

But fermented liquids do not consist of water and alcohol alone. Other substances are present also,—of these as most nearly related to the alcohol in volatility and action, I shall next mention volatile oils and ethers. These volatile oils and ethers chiefly determine the excellences of different wines; the quantity present in any wine is usually so small, that very little can, under any circumstances, be taken into the body. Still these substances are of great interest to the chemist, because many of these principles have lately been formed artificially, to the medical man because of the energetic action which some of these substances exhibit.

Here are some of the most remarkable of these flavouring principles which have lately been artificially prepared. Here is the essence of pear, here of pine-apple, here of grapes, here of black currants, and of many other fruits. These substances now made in large quantities are in daily use. Some of them are peculiar ethers, others are fusel-oil compounds.

There can be little doubt that the odoriferous principles of different wines will, like those of the different fruits, be ultimately formed artificially. Essence of cognac is now manufactured.

Here is a specimen of œnanthic ether, which is present in all wines and which is not the cause of the peculiar flavour of wine. It was discovered by Liebig and Pelouze. It has been formed artificially by oxidising oleic acid, and by the action of nitric acid on the etherial oil of rue. Moreover, the acid of this ether (œnanthic acid) has been found to exist in the pelargonium roseum. It is said that, in 400,000 parts of wine, one part of this ether exists. In Bourdeaux wine other ethers, as acetic ether, butyric ether, and valerianic ether, are said to exist, and these ethers contribute to that aroma which so quickly disappears on opening these wines. The Rhine wines contain even a higher aroma, but I know of no experiments on the ethers of these wines. The minute quantity present alone prevents a perfect investigation of the subject.

Generally, it may be said, that beer contains much saline matter, wines contain very little, and spirits contain none at all. Each year our knowledge of the chemical composition of these fluids will increase, and, with advancing knowledge, we shall obtain more clearness regarding the dietetic and medicinal properties of all fermented liquids. At present, the answer to the question, Which is the best wine? cannot be given, because of the imperfect knowledge which we possess of the chemical composition of different wines. But, even when full knowledge is obtained, no universal answer can be given; for that which is best in one state, or for one person, may be the worst in a different state, or for another person. Generally, however, it may be said, that that wine is best which contains least alcohol, least acid,

least sugar, and highest flavour. This is the best for a person in health; while, for a person in a state of low fever, that wine which contains least salts, most sugar, most astringent acid, and most alcohol, will cause least irritation of the bowels, will furnish most nutriment, be most febrifuge, and most supporting. Where there is much irritation and much depression, brandy will be better than wine.

In some states in which diuretic action is desirable, Moselle wine, by the excess of salts it contains, may prove almost as energetic, and far less heating, than the essential oil in gin or whiskey,—the difference in action being, that between cream of tartar and oil of turpentine.

In diabetes, ale and porter, which contain much sugar, cannot, on rational grounds, be advised; while claret, which is free from sugar, and contains perhaps much tannic acid, is highly beneficial; while cider, though free from sugar, contains much malic acid, and is diuretic.

In dyspepsia and gout, the wine which is freest from ultimate acidity, and which is least stimulating, is best. The least acid claret wines first, and then some sherry, and even perfectly dry champagne, when very free from excess of acid, are not unsuitable. When a taste is acquired for good Mansanilla, it answers all the requirements, and is obtained at a far cheaper rate than first-rate Amontillado sherry, which may be procured free from all sugar, and with little acid, and scarcely stronger than Mansanilla; and so far surpasses it in flavour, that the difference in expense is almost forgotten. But by adding water to brandy or other spirit, a fluid is obtained which is far less acid than any wine, and which may be made of any strength, and be free from all sugar. Theory then as well as practice leads to the belief, that if any stimulant is taken in dyspepsia, the best is brandy and water.

With regard to porter and stout, these containing little acid have much sugar, and hence give rise to much more acidity than some pale ale. Some pale ale may be found less sweet and acid than ordinary sherry, but generally pale ale is ultimately more acid than sherry, and less stimulating; yet for this compensation is made in the greater quanity of ale drunk. The larger quantity of salts in malt liquids must always be borne in mind.

Such is the meagre information which at present is the best I can bring before you to enable you to answer the question, What may I drink?—*Medical Times and Gazette, Sept.* 2, 1854, *p.* 231.

168.—*Action of Alcohol.*—From an elaborate series of experiments, the following conclusions are arrived at by Dr. Duchek: 1. Alcohol in the organism is subservient to an increased combustion, the intermediate products of which are found in the blood. 2. Intoxication is dependent upon the existence of aldehyde in the blood at the time. 3. The effect of aldehyde upon the blood is that of rapid consumption of oxygen; and, finally—4. Hereby the combustion of other substances is interrupted, or rather diminished.—*Viertel Jahr.*

[In these short remarks, the reader will at once perceive the most powerful inducements to abstain from alcohol. It runs away with that oxygen which he is always inspiring for the oxidation which is required in almost every process of the animal economy. We live, as it were, to oxidize. Almost all the changes in the body are the results of oxidation, and yet the spirit drinker continually checks this process of nature.]—*Lancet, April* 8, 1854, *p.* 394.

169.—IODO-TANNIC SYRUP.

This syrup is directed to be prepared according to the following formula:—Iodine two grammes; extract of rhatany root, eight grammes; water and sugar, of each a sufficient quantity to make a syrup, one kilogramme. Care must be taken that the extract of rhatany root employed for this purpose should be completely soluble; and in order to secure this complete solubility, the extract should be prepared *in vacuo.* The iodine is then to be dissolved in a small quantity of alcohol, and subsequently mixed with the extract of rhatany root, previously dissolved in water. The mixture is then to be poured into a glass mattrass, and carefully set aside for some hours, so that the reaction of the iodine and the extract of rhatany upon each other may be complete, which will be known by the deposition of a brown pulverulent mass.

This is to be entirely separated by filtration, and then repeatedly washed with water, so as to remove any portion of iodine that it may contain; the various strained or filtered liquids are to be mixed together, and submitted to slow evaporation by means of a vapour bath, and when sufficiently concentrated, the sugar is to be added, so as to produce a syrup, which, when well prepared, should have a fine red colour, and a very agreeable taste. It will contain six centigrammes of iodine in thirty of the vehicle, and it may be preserved without undergoing alteration for an almost indefinite time. In preparing this syrup, care must be taken that glass vessels be employed.

Normal Iodo-Tannic Solution.—This solution of iodo-tannin is directed to be prepared by triturating together five grammes of iodine, forty-five grammes of tannin, and one thousand grammes of water. The solution of the iodine and tannin in the water is soon effected, when it is to be filtered, and concentrated, by careful evaporation, until it is so far reduced as to amount to one hundred grammes, after having sedulously examined it by means of starched paper.

Iodureted Iodo-Tannic Solution.—This solution of iodo-tannin is directed to be prepared in the same manner as the former, by triturating together five grammes of iodine, ten grammes of tannin, and ninety grammes of water, and subsequently promoting the solution in a glass mattrass, by means of gentle heat obtained from a sand-bath. This solution possesses the decided advantage of retaining the iodine it contains, in such perfect solution, that it is not deposited whatever amount of water may be added with the intention of diluting it, for it thus becomes soluble in every proportion of this vehicle; and it may therefore be substituted for the various solutions of iodine made by the intervention of either alcohol or potassa. The two latter solutions of iodo-tannin are recommended for external use, and the first—the syrup of iodo-tannin—for internal use, by Messrs. Socquet and Guillermond, who, in a paper communicated to the 'Bulletin Général de Thérapeutique,' state that, in the preparation of the syrup of iodo-tannin, they invariably avail themselves of tannin contained in rhatany root, whereas, in the preparation of the solutions for external use, they make use of tannin obtained from oak-bark.—*Annals of Pharmacy.* —*Dublin Hospital Gazette, Sept.* 15, 1854, *p.* 255.

170.—*Syrup of the Carbonate of Iron.*—This new preparation is described by Dr. Richardson in the 'Association Journal' of July 28. He finds that it preserves the iron in the state of carbonate for an indefinite period, and as a medicine is agreeable in taste and convenient for dispensing. It may be prepared according to the following formula:—Take of carbonate of potash half an ounce, sulphate of iron three drachms and one scruple, simple syrup eighteen ounces, water ten ounces,—dissolve the sulphate of iron and carbonate of potash each in five ounces of the water,—mix the solutions. The precipitate is to be collected on a cloth filter, and after being washed with distilled water, is squeezed into a pulp as dry as it is possible,—this pulp is then rubbed up quickly with the syrup. The dose of the syrup thus formed is from two drachms to half an ounce.—*Dublin Hospital Gazette, Aug.* 15, 1854, *p.* 220.

171.—*Iron Alum.*—Two preparations have recently been employed in medical practice by Dr. Tyler Smith, of London, under the name of *iron alum.* They are stated to possess more powerful astringent properties than common alum, and at the same time are not liable to produce the usual stimulating effects of other salts of iron. It is well known that the term *alum* is applied by chemists as a generic designation to an extensive series of salts, which coincide with common alum in constitution and crystalline form, and in the composition of which the alumina may be replaced by any other sesquioxide. Thus we have:—

Common alum	$Al_2 O_3\ 3\ SO_3 + KO, SO_3 + 24$ aqua.
Iron alum	$Fe_2 O_3\ 3\ SO_3 + KO, SO_3 + 24$ aqua.
Chrome alum	$Cr_2 O_3\ 3\ SO_3 + KO, SO_3 + 24$ aqua.
Manganese alum	$Mn_2 O_3\ 3\ SO_3 + KO, SO_3 + 24$ aqua.

In like manner, the potash of these salts may be replaced by other alkaline bases, such as soda or ammonia; it is therefore evident that many of the salts to which the appellation alum, in its most comprehensive sense, is applied, will contain no alumina whatever.

The preparation now used at St. Mary's Hospital is obtained by peroxidizing protosulphate of iron with nitric and sulphuric acids (as in the first part of the process for preparing hydrated peroxide of iron in the 'Dublin Pharmacopœia'), and adding an equivalent of sulphate of potash. The solution is then to be evaporated with excess of sulphuric acid, until crystals form on cooling; it affords a beautiful salt of pale violet colour, which is more soluble than common alum, the solution having a reddish colour. The second salt known as iron alum, is obtained by adding an equivalent of sulphate of potash to the sulphate of iron, and evaporating in the manner already described, so as to obtain crystals. As it is important that substances used in medicine should be strictly defined, the former salt ought to be designated *potassio-sulphate of peroxide of iron*, and when the ammonia salt is prescribed, it should be termed *ammonio-sulphate of peroxide of iron*.—*Pharm. Journal, Jan.*—*Dublin Hospital Gazette, March* 1, 1854, *p.* 47.

172.—*Saccharized Alcoholic Extract of Ipecacuanha.*—Mr. A. G. Dunn, in the 'New York Journal of Medical Science,' recommends the employment of a saccharated extract of ipecacuanha in preference to the usual preparations of the drug, such as the wine and syrup, which he considers liable to vary in strength; and the powder is, he thinks, objectionable, from its insolubility. The following is the formula for preparing the extract:—

> Rad. ipecac. ℥ iv.; bruise to a coarse powder, and macerate for thirty days in f ℥ xvi. of diluted alcohol, shaking it occasionally, then filter and express. Evaporate the tincture thus formed to ℥ ii. with which mix powdered white sugar ℥ viij., and triturate them in a mortar until they become dry.

The extract thus prepared has the peculiar odour and taste of ipecacuan; it is of a brownish yellow colour, and soluble in water, ether, alcohol, mucilage of acacia, and in fact, in all the solutions with which this remedy is usually combined. The dose required to be exhibited is the same (twice as much) as the genuine powdered root. From its agreeable taste and perfect solubility in fluids, it is much preferred to the other preparations of the drug, by those who have employed it; more especially in prescribing for children, for whom its sweet taste renders it an excellent form for combining with other remedies.—*Dublin Hospital Gazette, June* 1, 1854, *p.* 142.

173.—*Iodide of Potassium with Ammonia.*—An opinion prevails at Guy's Hospital that the efficiency of the iodide of potassium is very much increased by combining with it the carbonate of ammonia. The proportions we usually observe prescribed are, two to three grains of the iodide with four to five of the ammonia. Dr. Gull, who strongly advocates the combination, explains to his class that he believes the ammonia to act in three ways:—1st. As a gentle stimulus to the stomach, preventing the iodide from disagreeing. 2nd. As a diaphoretic, diffusing the blood, and with it the remedy, well through the whole system. 3rd. By chemical decomposition, itself being changed into nitric acid, and then by combination with the base of the salt, liberating the iodine in its free form.—*Med. Times and Gazette, May* 13, 1854, *p.* 488.

174.—*Inga: a new Astringent.*—This substance enjoys a reputation as an astringent and tonic in some European countries, and also in America. The bark is compact and heavy, and its fracture presents alternate layers of white and red. When chewed it is found to be astringent to the taste, and quickly imparts a red hue to the saliva. It is rich in extractive principles. The alcoholic extract resembles that of rhatany, both in its colour and general properties. In America inga is extolled as an astringent tonic in diarrhœa, in gonorrhœa, in hæmoptysis, in incontinence of urine, and in relaxation of the tissues. As an antiseptic its powder has also been used in the same instances as cinchona. Some trials already made in Paris

appear to justify its reputation and its claim to take rank in our Materia Medica.—*Dublin Hospital Gazette, June* 15, 1854, *p.* 159.

175.—*Mode of Death from Chloroform.* Mr. R. E. BICKERSTETH, of Liverpool, after much careful investigation, comes to the following important conclusions on this important subject:—

1st.—That in death from the inhalation of chloroform, the respiratory movements cease before the cardiac.

2nd.—That the heart continues its action, uninfluenced by the chloroform, for a period longer or shorter after the cessation of respiration, and that its then failing may be considered as a natural consequence of the respiration having ceased, and as independent of the influence of chloroform.

3rd—That if after the respiration has ceased, and while the heart is still in action, chloroform continues to be absorbed into the system, its movements may become impaired or cease—the chloroform in such case acting directly upon the heart.

4th.—That if artificial respiration be resorted to before the cardiac contractions are seriously affected, and be properly maintained for a sufficient period, the respiratory functions may be re-established.

He adds the following highly practical observations:—

I would here direct attention to the expediency of drawing forward the tongue, in all cases when it is found necessary to resort to artificial respiration. When the patient is lying on the back, so soon as the breathing ceases and the jaw drops, the tongue is particularly liable to fall backwards and close the orifice of the glottis. Artificial respiration, under such circumstances, is worse than useless. It is better at once to pull the tongue well out of the mouth, and passing a hook through the tip, confide it to the care of an assistant. I am convinced that in some cases in which artificial respiration has failed it has been from the neglect or too tardy adoption of this very simple means. Time of the utmost value has been lost in the absurd attempt to restore animation, by applying stimulants to the nostrils, or pouring cordials into the mouth, without even a thought that the first can have little or no effect after the respiration has ceased, or that the second would as likely pass into the trachea and bronchi as into the stomach.—*Edin. Monthly Journal.—Dub. Hosp. Gazette, Feb.* 15, 1854, *p.* 32.

176.—*Belladonna in Salivation.*—A woman treated by mercury, internally and externally, for serous diarrhœa, was affected with profuse salivation. Dr. ERPENBECK treated this latter complaint with belladonna in divided doses, of two grains and a half, taken in emulsion every twenty-four hours. Next day the salivation had subsided, and the mouth was quite dry. On stopping the belladonna, the salivation returned, and again ceased when it was resumed. The author believes that after this fact and some similar ones, belladonna is the best treatment for salivation.—*Hanover Corresp. Blatt.—Dublin Hospital Gazette, Sept.* 15, 1854, *p.* 251.

177.—*On the Use of Belladonna in Poisoning by Opium.* By Dr. THOS ANDERSON.—Dr. Graves first suggested, that in continued fever, with contracted pupils and coma, if an agent administered internally would occasion dilatation of the pupils, it might also relieve the other symptoms of cerebral derangement. Dr. Anderson, acting on this theory, administered large doses of belladonna in two cases of poisoning by opium, which he related as follows.

A patient, of whom I had charge, and labouring under delirium tremens, having received an over-dose of a solution of the muriate of morphia, became comatose. He had taken, in thirty-six hours, two ounces of the solution of the muriate of morphia, and it had been continued by the attendant after sleep was procured. When I saw him he was in profound coma, his breathing was stertorous, amounting to no more than four or five per minute, and his pupils were contracted to mere

points. His pulse was excessively weak, and rather slow; it was quite impossible to rouse him. I ordered him immediately the following mixture:—Tincture of belladonna six drachms, in five and a half ounces of water, of which an ounce was to be given every half hour. Three ounces of the mixture were administered with great caution, after which his pupils began to dilate. The six drachms of the tincture of belladonna were taken, and in four and a half hours after the first dose of it was given, the patient was in the following condition:—The coma was entirely gone, respirations were between twenty-two and twenty-five per minute, the pupils were much dilated, the pulse had risen to nearly one hundred and twenty in the minute, and was also increased in strength. His countenance, also, from being cold and pallid, had become much flushed, and the whole body was much warmer. He replied readily and coherently to all my questions. He continued to improve for three days after, when rising suddenly to stool, he fainted, and before the assistance of the nurse could be procured he was dead.

A fortnight afterwards, a woman, about fifty years of age, took, at 4 o'clock p.m., two drachms of laudanum, and at half-past 5 p.m., three drachms more. She was brought to the Infirmary at 8 o'clock p.m. After making vain attempts to rouse her from the coma, by walking her about, &c., the stomach-pump was used at a quarter-past 8 o'clock. By this means her stomach was thoroughly evacuated, but no trace of opium was detected by smell or sight. It had probably been all absorbed. A current of electricity was then applied to her hands for nearly ten minutes, but without rousing her. I saw her at a quarter to nine p.m. for the first time, and on being told that she had been poisoned by laudanum, I determined to try the effects of belladonna.

At that time her pupils were contracted to mere points, her respiration was stertorous, ten per minute, the pulse was feeble, and the extremities rather cold. Between 9 and half-past 9, I gave her one ounce of tincture of belladonna in three ounces of water, which was all swallowed, but with difficulty. In the course of the next half hour two drachms more were administered. At 11 p.m. the first alteration in the size of the pupil was observed; the respirations had also then increased to twelve or thirteen in the minute, and the pulse was much stronger. The symptoms continued to improve till 2 a.m., when all indications of opium poisoning had disappeared. The woman was then sitting up in bed talking to the nurses, with the pupils dilated to a little more than their natural size, and still slightly sensible to light. The extremities were quite warm, the pulse was about 100, and of good strength.

She gave me a coherent account of her motives for taking the poison, of the amount of money she had spent in purchasing the laudanum, and the names of the druggists where it had been procured. She also replied sensibly to questions about her family, and the age and occupations of her children. She continued awake till nearly 4 o'clock a.m., after which she slept till 9 a.m. In the morning I found her pretty well, her pupils being no more dilated than they were four hours after the first administration of the belladonna. She complained, however, of nausea, but unaccompanied with vomiting. This symptom, along with the dilated pupils, had entirely disappeared in the course of two days. She was kept in the hospital, under observation, for ten days after the accident, at the end of which time she was dismissed, perfectly well. The tincture of belladonna, used in both these cases, was of the strength of four ounces of the leaves to two pints of rectified spirit, and prepared by percolation. Half a drachm is considered a dull dose. I have seen dilatation of the pupil produced by a drachm given at once.—*Monthly Journal of Medical Science, April*, 1854, *p.* 377.

178.—*Saturnine Poisoning treated by the Iodide of Potassium.* Dr. H. Swift, states ('New York Medical Times') that the treatment of saturnine poisoning by iodide of potassium has been tried in the New York Hospital in twenty-three cases, and with highly satisfactory results. "In thirteen instances the urine was submitted to chemical analysis, and the investigation has established the fact that the lead may be eliminated from the system by the iodide of potassium, and found in the urine. In no case was the lead detected before the administration of the remedy.

The chemical analyses were made by Professor Outram, and the results of his experiments are perfectly reliable.

"All the patients began to improve rapidly after this treatment was adopted, though they had previously resisted the ordinary means. No bad effects resulted from the long-continued use of the remedy. In two cases, as M. Melsens suggests may occur, the symptoms were at first slightly aggravated,—one of them was profusely salivated while under treatment, and the other slightly so. One patient also suffered from coryza and gastric disturbance for a few days; but the treatment was only suspended for a short time. One patient was under the influence of iodide of potassium for six months, one for five and a half, and another for four months.

"In case 6, the urine was examined shortly after the treatment was commenced, and merely a trace of lead was detected. The quantity sensibly increased, until it was clearly shown both in the urine and saliva; and, as the patient convalesced, it disappeared entirely, and the iodide of potassium was found abundantly in the saliva. In case 5, we did not suspect the existence of lead poisoning until after the patient had been put upon treatment for constitutional syphilis. While under this treatment, a well-defined 'blue line' appeared upon the gums. The urine was then examined, and found to contain lead.

"Of the twenty-three cases treated by the iodide of potassium, sixteen have been discharged cured, and three so far relieved as to be able to resume their ordinary duties; four are still under treatment, and are gradually improving. Thirteen of the patients suffered from lead colic, complicated with neuralgia, arthralgia, &c.; four had paralysis of the wrists, and in six the paralysis was general."—*Med. Times and Gazette, Aug.* 26, 1854, *p.* 223.

179.—*On slow Copper Poisoning.* By Dr. CORRIGAN, Dublin.—[Dr. Corrigan relates several cases of this description, and then comes to the following conclusions:]

1st.—They show us that copper, or its carbonate, will act as a slow poison by absorption, undermining the constitution, producing emaciation, catarrh, and loss of strength, and leaving the system in a state little capable of resisting the ordinary exciting causes of many diseases.

2nd.—The symptoms, although not acute, are well marked—emaciation, a cachectic appearance, loss of muscular strength, colicky pains, cough without physical signs to account for it, *and the peculiar characteristic sign of retraction of the gums, with a purple, not a blue edge.*

3rd.—In none of the cases, although there was muscular debility, was there either acute colic with constipation, or a local paralysis that so often results from the poison of lead, and the colour of the gums was quite distinct from that produced by lead.

4th.—Copper, in slow poisoning, seems to exert its deleterious influence mainly on the nutritious functions, or assimilation, including absorption and secretion; while lead acts energetically on the nervous system of both organic and animal life, exhibited in its action on the former by the obstinate constipation, and on the latter by the violent pains of lead colic, and by the production of its peculiar paralysis.

5th.—The knowledge that copper, or its carbonate, is capable of acting by absorption as a slow poison, will be useful, as it may lead us to discover the nature of some apparently, at first sight, anomalous diseases that might otherwise elude our diagnosis. The tint of colour produced on the gums, whether by copper or lead, remains a very long time. It had not disappeared from any of the cases above narrated, as long as they were under my observation. In the case of a policeman who has been very lately in the Whitworth Hospital, the blue colour produced by lead was very distinct. He had been in the police force two years. Previously to his becoming a policeman he had been a plumber.—*Dublin Hosp. Gazette, Sept.* 1, 1854, *p.* 232.

180.—*The Venom of Serpents.*—Dr. J. GILMAN arrives at the following conclusions:—1. That the venom of all serpents acts as a poison in a similar manner.

2. That the venom of some varieties is far more active than that of others. 3. That a variety of the *coluber*, known as the "cotton-mouth," is the most venomous serpent in Arkansas. 4. That the venom of serpents destroys all forms of organized life, vegetable as well as animal. 5. That alcohol, if brought into contact with the venom, is, to a certain extent, an antidote. 6. That serpents do possess the power of fascinating small animals. 7. That the blood of small animals destroyed by the venom of serpents, bears a close resemblance to that of animals destroyed by lightning or hydrocyanic acid: it loses its power of coagulation, and cannot be kept long from putrefaction.—*St. Louis Med. Journal.—Lancet, July* 8, 1854, *p.* 10.

181.—*Charcoal as a Disinfectant.* By DR. STENHOUSE.—Mr. Turnbull, nine months ago, placed the bodies of two dogs in a wooden box on a layer of charcoal-powder of a few inches in depth, and covered them over with a quantity of the same material. Though the box was quite open and kept in his laboratory, no effluvia was ever perceptible; and on examining the bodies of the animals at the end of six months, scarcely anything remained of them, except their bones. Mr. Turnbull sent me a portion of the charcoal powder which had been most closely in contact with the bodies of the dogs. I submitted it for examination to one of my pupils, Mr. Turner, who found it contained comparatively little ammonia, not a trace of sulphuretted hydrogen, but very appreciable quantities of nitric and sulphuric acids, and acid phosphate of lime. Mr. Turner subsequently, about three months ago, buried two rats in about two inches of charcoal-powder, and a few days afterwards the body of a full-grown cat was similarly treated. Though the bodies of these animals are now in a highly putrid state, not the slightest odour is perceptible in the laboratory. From this short statement of facts, the utility of charcoal-powder as a means of preventing noxious effluvia from churchyards, and from dead bodies in other situations, such as on board ship, is sufficiently evident. Covering a churchyard to the depth of two or three inches with coarsely powdered charcoal would effectually prevent any putrid exhalations ever finding their way into the atmosphere. Charcoal-powder also greatly favours the rapid decomposition of the dead bodies with which it is in contact, so that in the course of six or eight months little is left except the bones.—*Pharm Journal.—Lancet, July* 15, 1854, *p.* 34.

182.—*Plan for Deodorizing the Sewage matter which passes through Water Closets.* By Mr. J. BEARD.—It is proposed to suspend within the cylinders at present in use, a perforated zinc or earthenware receiver, of a sufficient size to contain a month's supply of chloride of lime, the top of the cylinders being made to open and shut, and to be air-tight. The water that is admitted into each cylinder by the action of a person using the closet, will entirely surround and come in contact with the lime, so that the whole of the water that is used will, by this means, become impregnated, and thus deodorize, not only the animal matter, but also the atmosphere of the closet.

The advantage of this plan over the one suggested by a correspondent of the 'Medical Times and Gazette' is, that it is perfectly self-acting, and, therefore, cannot become a nuisance by any neglect of children or invalids using the closet; and also, that the receiver may be made to contain even two months' supply instead of a week's.

The opening and closing of the cylinders for the purpose of giving the necessary supply is so simple, as not to require any tradesman to attend to it, but may be performed by any man that is capable of using a screw-wrench. I think the plan is so very simple, that it will be understood at once without the aid of a diagram, or I would have enclosed one.—*Med. Times and Gazette, Sept.* 23, 1854, *p.* 334.

183.—ON PURULENT OPHTHALMIA.

By GEORGE CRITCHETT, Esq., Surgeon to the Royal London Ophthalmic Hospital, &c.

[The progress of this disease is so extremely rapid, that we are glad to receive any suggestions of treatment which are likely to be an improvement. Mr. Critchett's plan is as follows. He says]

The plan I adopt when there is extensive chemosis, and swelling of the lids, and the cornea is threatened, is, in the first place, freely to divide the membrane by the method recommended by Mr. Tyrrell, and in addition to this, to endeavour to relieve the distended vessels by the application of leeches to the swollen lids. I also keep a piece of lint, dipped in cold water, constantly applied to the surface of the lids, and I direct a strong solution of the nitrate of silver, varying from five to ten grains to the ounce, to be dropped between the lids every hour, or every two hours, as the patient can bear it. I think it very important that the discharge should be carefully removed previous to the insertion of the drops, so as to ensure their getting into contact with the surface of the membrane; this can best be accomplished by means of a syringe and a little lukewarm water; if the nitrate of silver occasions great pain some other astringent may be advantageously employed; the sulphate of copper may be brushed over the surface, or a strong solution of alum may be substituted; but I place more reliance upon the nitrate of silver than on any other local means. If there is an ulcer threatening to penetrate, a finely-pointed piece of the nitrate of silver may be often advantageously applied to the ulcerating surface.

As regards constitutional treatment, I am of opinion that internal remedies exert little or no influence over this form of disease, and certainly have no specific powers of controlling it. Mercury I have seen freely exhibited so as to produce its full constitutional effect without any benefit; and in as far as it enfeebled the system with manifest detriment, all the most severe and rapid cases that have come under my observation have exhibited symptoms of feeble power; and although the character of the poison is no doubt an important circumstance in estimating the virulence of the disease, the condition of system exerts no modifying influence. I should therefore advise that while the vessels or the part are relieved by local depletion, the system be sustained in every possible way. Tonics and diffusible stimuli must be freely administered. Decoction of bark with carbonate of ammonia, or a combination of steel with quinine, in full doses, taking care at the same time to act freely on the bowels. The diet must at the same time be liberal and even stimulating—a full allowance of good fresh meat and at least a pint of porter daily. As this disease may be the result simply of local contagion, of course it is liable to occur in strong and plethoric persons, to whom the above remarks would not apply; but even in such cases, I should not advise general depletion, a healthy condition being that most favourable for the action of those local means upon which successful treatment must mainly depend. A case came under my care at the Ophthalmic Hospital some months ago, which so strongly illustrates and confirms many of the above remarks, that though I am, as a general rule, unwilling to insert cases into these lectures, desiring them to be regarded as the impressions left on my mind by a large aggregate rather than as influenced by any solitary case, yet I am tempted to give it, because the disease I have been dwelling upon is fortunately so rare in this country that we are compelled to form an opinion upon very limited data.

The young woman whose case I propose briefly to detail, had lost her left eye during infancy. When I first saw her she had been suffering for five days from a very acute attack of purulent ophthalmia in the right eye. The symptoms were in all respects well marked; the lids were much swollen and of a bright red colour, and closed; the discharge was thick, yellow, and abundant; the conjunctiva was in a very chemotic state, and highly injected, and on the lower part of the cornea was a large irregular ulcer, deep, and with well defined edges, and near the centre was a small transparent vesicle, indicating that at this point the true corneal tissue was ulcerated through, and that the aqueous humour was merely retained by the inner elastic layer. Collateral circumstances strongly indicated that gonorrhœal inoculation was the cause of the disease. The pulse was rapid, the skin moist and cool, the tongue foul but rather flabby; great pain and restlessness were complained of, and there was, as might be expected, great mental depression and anxiety. She

stated that she was a needle-woman, that her diet was usually rather scanty, and that since her eye had been bad she had lived lower than usual.

My impression, on first seeing the case, was most unfavourable, and I entertained very slender hopes of saving any sight. In my treatment of the symptoms, I adopted, in all respects, the plans I have recommended above. My first and most urgent anxiety and attention were directed to the ulcer, which threatened every hour to penetrate. In the hope of preventing this, I applied a finely-pointed piece of nitrate of silver to the surface of the ulcer. I then divided freely the chemosis round the cornea in three directions. I ordered the eye to be cleansed carefully with a syringe and luke-warm water, and a drop of a strong solution of the nitrate of silver, ten grains to the ounce, to be put into the eye as often as it could be borne, and wet rags to be kept constantly on the lids; in combination with this local treatment I prescribed full doses of quinine, and a liberal meat diet, together with a pint of porter daily. The result was in all respects most satisfactory; the ulcer healed with a dense large white cicatrix, which obscured about half the pupil. The conjunctiva remained inflamed and villous for a considerable time, but by means of repeated applications of the sulphate of copper, this was at length removed, and a considerable amount of useful vision was the result. The nitrate of silver appeared to act most favourably in this case; it caused but slight pain, and rapidly diminished the discharge and the swelling of the conjunctiva, and the feeling of heat and grittiness in the eye. The strength of the solution that I employed was ten grains to the ounce of distilled water.

[Mr. Critchett next proceeds to the cases of infants in whom the disease appears about the third day, but sometimes sooner. Indeed we remember a case related by the late Mr. Walker, of Manchester, in which the disease evidently had run its course *in utero*.]

There are two symptoms that may be selected as a gauge of the intensity of this disease as it occurs in infants: the one is the condition of the lids; the other the colour and consistence of the discharge. If the lids are of a bright red colour, and much swollen, and if the discharge is yellow and thick, and very abundant, it is a severe case, but only in the first stage, and the eye is as yet probably safe; but if the lids appear of a bluish red colour, and rather flaccid, the disease has most probably seriously damaged the eye. There is often considerable difficulty in examining the condition of the sight in these little patients, on account of the swelling of the lids, the smallness of the palpebral aperture, and the amount and thickness of the discharge. On separating the lids the space is immediately filled with water; this must be carefully wiped away, and the lids reopened; this must be repeated until no more discharge escapes, when the state of the cornea may be ascertained. The rapidity and destructive character of the disease are regulated by the nature of the morbid secretion that has been applied to the eyes, and also by the constitutional powers of the little sufferer. Some of the worst cases I have seen have been in seven-months children. The investigation of the causes of this disease is always attended with difficulty, the mother being naturally unwilling to admit that she has been the cause of her child's disease; nevertheless, I feel satisfied, from extensive inquiry, that leucorrhœa in the mother is a common cause of the disease; and that, in the very severe and rapidly destructive cases that sometimes present themselves, gonorrhœa is the cause. I do not deny that the disease might arise from cold, but I believe this is rare, and that the symptoms are mild.

From these remarks, it will be inferred that purulent ophthalmia in infants varies much in degree; the modifying causes being, as I have already said, the nature of the discharge and the constitutional powers of the child. It follows that if the matter applied be gonorrhœal, and if the child be prematurely born or very feeble, the disease produces its disastrous results very speedily. I have, on several occasions, known it to destroy both eyes within a fortnight from its first development, and I have known it to exist many weeks without seriously damaging the eyes. It has been such cases as these latter that have no doubt lulled many medical men into a false security with regard to the result, and into neglect of the case, or the adoption of most inefficient treatment. If the disease in its severer forms be allowed to pursue its course unchecked, the cornea gives way either by sloughing or by ulceration, but most frequently by the latter, the iris prolapses, and either the globe becomes atrophied or staphylomatous. In milder cases ulcers form that leave dense

central opacities that are often very detrimental to sight. A curious result of these cases, even where the cornea remains bright, is a small central white spot upon the capsule, which has probably been precipitated from the aqueous chamber during the time the pupil was in a state of extreme contraction. It remains during life, but is not detrimental to sight.

The treatment of these cases is merely local; it is very simple, and the result highly satisfactory. All that is required is the frequent application of some mild astringent or caustic lotion to the surface of the conjunctiva. At the Ophthalmic Hospital we use a solution of alum, from five to ten grains to the ounce; but a weak solution of nitrate of silver answers equally well, and I think acts more rapidly. The essential point is, that whatever is used be well applied to the surface of the membrane. A frequent source of failure is to be traced to the neglect of this measure. The remedy has been judiciously chosen, but has failed for want of being properly applied. If a lotion is used, it should be frequently injected with a syringe between the lids, so as to wash away the discharge, and get well over the surface of the membrane; if drops are used, the eye should be first carefully cleansed. This plan of treatment, when properly carried out, is almost uniformly successful. Out of many hundred cases that I have seen, I can scarcely recal a single instance where sight has been lost, if the treatment has been commenced sufficiently early in the disease.—*Lancet, May* 27, 1854, *p.* 562.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOREGOING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

FEVER.—Be careful of your prognosis, when the nervous symptoms in fever preponderate at an early period of the attack, especially in the wealthy, who cannot bear wine so well as the poor. The more the secondary affections of fever are *anatomical*, the greater will be the use of stimulants; and conversely, the more they are neurotic, the less will be the use of stimulants. (Dr. W. Stokes, p. 23.)

When the tongue is dry and brown, head not very hot, respiration short, frequent, and irregular; and when added to these there is delirium, more or less violent, give twenty-five minims of chloroform in a draught, to be repeated in an hour. This may be repeated in a few hours if necessary, and if the pulse be watched, may be continued every second hour for some time. The *inhalation* of chloroform is useless in these cases, and may be followed by convulsive movements. (Dr. Gordon, p. 28.)

Yellow Fever.—Give quinine in the early stage of depression, promptly, boldly, and in large doses. The first stage is one of diminished vital energy which quinine raises. Give 10 to 20 grains every second or third hour. Four or six hours should never elapse between one dose and another during the stage of depression; and, when necessary, give it up gradually. (Dr. W. J. Cummins, p. 27.)

CANCER.—Apply a frigorific mixture of ice and salt, for four minutes, so as to destroy the vitality of the cancer cells. Repeat this every few weeks or month. This may not remove the hardness, nor the tumour of scirrhus, but may destroy its life and its malignancy. (Dr. J. Arnott, p. 251.)

GOUT.—Give the following: ℞. Extract. acet. colch. gr. j.; pulv. Doveri, gr. ij. Ft. pil. ter die sumend. Pulv. Doveri, gr. v., omni nocte sumend., et pulv. rhæi cum magnesiâ ℈ij. omni mane. (Dr. Hughes, p. 29.)

Iodide of potassium dissolves lithate of soda very readily, but not lithic acid. Therefore give this medicine in cases of gout, except during the acute attack. Give 5 or 8 grains two or three times a day for a short time; or one grain every day in one or two doses for some weeks or even months. (Mr. T. S. Wells, p. 31.)

RHEUMATISM.—When delirium occurs in acute rheumatism it is of grave import. Examine the heart carefully,—there is very likely to be pericarditis upon which the delirium depends. Bleed generally or locally according to circumstances, give calomel and opium, with salines and colchicum. Blister the cardiac region well, both in front and back. At the same time support the patient with broth, and even wine if necessary; always remembering that many of these apparently inflammatory diseases are asthenic and not sthenic. (Dr. Watson, Dr. C. M. Durrant, p. 222.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—Begin with sulphate of zinc in small doses (gr. i. to gr. ij.), and gradually increase the dose to five or eight grains. Sometimes liquor arsenicalis is very efficacious: give three minims to a child, and five or six minims to an adult in a bitter infusion three times a day. (Dr. Barker, p. 40.)

DELIRIUM TREMENS.—In delirium tremens arising from the *leaving off* of stimulants, which have been habitually taken in excess for a long time, give opium and moderate doses of alcohol, with good broth and nutritious diet. But in delirium tremens of a sthenic character, arising from the direct action of alcohol on the brain, taken lately but not habitually in excess, cases which are inflammatory and congestive, avoid both opium and alcohol, and try chloroform; but be careful of the above distinction in the two kinds of cases. Give half a drachm of chloroform in a draught every two hours. This may be increased to a drachm; but watch the respiration, and if slow and laboured, give a little compound spirit of ammonia, to rectify the sedative effects of the chloroform. We have seen chloroform given in this way in hysteria, have rather a serious effect on the respiration, the same as from inhalation. All bad effects, however, are rapidly obviated by a drachm of compound spirit of ammonia in water, and repeated in a quarter or half-hour if necessary. (Dr. M'Dowell, p. 40, and Edit. *Retrospect.*)

EPILEPSY.—As long as an animal is fully under the influence of chloroform, no convulsions can be produced. Chloroform controls and modifies the convulsions of epilepsy, and also the delirium of epilepsy, as well as those convulsive jerks of the muscles of both upper and lower extremities which are so distressing. Chloroform also has a marked power over puerperal convulsions; also over the convulsions of infants, and laryngismus stridulus. (Dr. R. B. Todd, p. 37.)

Give an infusion of the inner bark of the black elder (sambucus nigra). It acts as a hydragogue, and probably eliminates the poison from the blood which causes the epileptic fit. For directions how to prepare and give this medicine, see page 38. (M. Bogetti.)

NERVES, *Excision of Tumours of (Neuroma).*—When a tumour pressing upon or in any way implicating a nerve, has to be excised or dissected out, make a clean incision of the nerve either above or below the tumour before the operation, as this will not only considerably lessen the pain, but also the subsequent inflammation. The consequent paralysis lasts only a short time, as the divided nervous extremities soon re-unite. (Mr. Bickersteth, *Monthly Journal*, Aug., 1854, p. 121.)

TETANUS.—Apply a solution of morphia to the wound instead of giving it internally. The following is a good formula: Hydrochlorate of morphia, one scruple; tincture of hyosciamus, two drachms; water, three ounces. Moisten a piece of lint with this solution and keep it on the wound. (Dr. E. Hodges, p. 39.)

Chloroform completely relieves the spasms for the time. The difficulty is to know how far to push the remedy: it may be continued for several hours. (Dr. Hassal, p. 39.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

HOOPING COUGH.—Nitric acid arrests the paroxysms and removes the hoop. It is a tonic, and supplies the blood with nitrogen, which removes or neutralises the excess of fibrine existing in that fluid at the time, and which is a great source of danger. Give dilute nitric acid. Twelve drachms of the dilute acid in a four ounce bottle with syrup—a dessert-spoonful to be taken every few hours by a child under two years old. For an infant give about a teaspoonful. (Dr. G. D. Gibb, p. 51, and *Association Med. Journal, July* 28, 1854, *p.* 677.)

LARYNGOTOMY.—Place the patient on his back, and completely expose the crico-thyroid membrane by dissection before opening the larynx. This is necessary, as an artery (the inferior laryngeal) is often passing this space, and requires a ligature. Then make a vertical incision into the larynx, through the crico-thyroid space, and insert a full sized trachea tube. It is sufficient, in dyspnœa arising from causes in the larynx *of the adult*, to make the opening in the crico-thyroid space, but not so *in the child.* In the adult, any inflammatory effusion, or thickening, arising from acute affections of the larynx, takes place in the submucous tissue, and is more local. In the child, it takes place more on the free surface of the mucous membrane, and spreads downwards, thus becoming tracheal as well as laryngeal. Laryngotomy, therefore, is not so applicable to the child as to the adult. (Mr. J. Tufnell, p. 132.)

PNEUMONIA.—In sthenic pneumonia the sputa are of a rusty red colour, viscid,

and adherent. In asthenic, or typhoid pneumonia, the sputa are more like *prune-juice, i.e.* darker coloured, and less viscid than in the former. A very interesting point is, that, in many cases of pneumonia, the chlorides either disappear entirely or nearly from the urine, while there is a corresponding excess in the fluid poured out into the lung. The chlorides disappear from the urine up to hepatization of the lung, and as soon as resolution commences the chlorides begin again to appear in the urine. Therefore, in cases of pleurisy and pneumonia always *test the urine for chlorides*, as follows:—Add a few drops of nitric acid to a portion of urine in a test-tube, and then a few drops of a solution of nitrate of silver. If chloride be present, a dense white precipitate of chloride of silver, which is insoluble in acids, will fall. If chloride of sodium be altogether absent, no precipitate will occur. (Dr. R. B. Todd, p. 50.)

Phthisis, *Night Sweats of*:—Give the following draught at bedtime:—℞. Acid. gallic. gr. vij.; morphiæ acet. gr. ½; alcohol q. s. (a few drops); syr. tolutan. ʒss.; aquæ ʒj. The night-pill of the Brompton hospital is as follows: ℞. Acid. gallic. gr. v.; morphiæ hydrochlor. gr. ½: mist. acaciæ. q. s. Ft. pil. ij. (Mr. Hutchinson, p. 49.)

Give gallic or acetic acid. Dip the night dress in sea-water, or salt and water, and dry it before using. But the best remedy is four grains of oxide of zinc at bedtime, combined with a little henbane or hemlock.

Diarrhœa of.—Give the trisnitrate of bismuth, combined with gum and magnesia, or with Dover's powder.

Cough of.—Mix one part of chloroform with three parts of spirits of wine, and let the patient inhale when necessary, but with caution, and under medical direction. The inhalation of camphorated spirit is often sufficient, or even the vapour of hot water, or infusion of hops. Sometimes frequent deglutition, as the swallowing a little oil, will relieve the cough. Sometimes four minims of tincture of aconite is a good palliative.

Profuse Expectoration of.—To check this give creasote, pyro-acetic spirit, infusion of pitch, or balsam of tolu; but by far the best remedy is petroleum or Barbadoes tar, which often moderates the cough and expectoration remarkably. (Dr. Theophilus Thompson, p. 46.)

Tracheotomy.—To facilitate this operation both in epilepsy and other cases, make use of Dr. Marshall Hall's scissors, figured at page 271. These scissors are made so as first to divide the skin over the trachea, and are then pushed into the tube and the blade *opened* so as to stretch and cut as much as may be required. An instrument called a cage is then introduced so as to keep the orifice open. (Dr. Hall, p. 271.)

When the trachea has to be opened to *extract a foreign body*, don't be satisfied with making a mere slit, but make a sufficiently large valve-like aperture, larger than the size of the body which has passed. (Mr. J. Hilton, p. 133.)

Impromptu Tracheotomy.—If you are called suddenly to a case requiring tracheotomy, without proper instruments, take a pair of pointed scissors, and, the integuments having been divided with a lancet or sharp penknife, pierce the trachea with the scissors, and separate the blades. You may then insert the handle of a teaspoon and turn it one quarter or half way round. (p. 132.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Cholera.—For a valuable paper on the pathology and treatment of cholera by Dr. Cormack, see page 254.

Do not arrest the purging, it is eliminatory. Give turpentine and yeast as follows: yeast a tablespoonful, oil of turpentine a teaspoonful, cold water or barley water two teaspoonfuls. Give this dose every second, third, or fourth hour according to circumstances. (Mr. L. F. Crummey, p. 262.)

Don't give opium. Give calomel freely, combined with camphor. Exhibit oxygen gas. This gas is easily given by shaking powdered chlorate of potash on a heated surface, and making the patient inhale the fumes. Or mix chlorate of potash with one-sixth its weight of black oxide of manganese, and throw the mixture on an iron shovel, heated to dull redness. (Dr. T. O'Brien, p. 262.)

Make a strong decoction of the bitter bush, eupatorium, as follows: boil about two drachms in a pint of water, and give a small tea-cupful, cold, every half-hour. It may be given alternately every half-hour with some alkaline or saline powder. (Dr. W. E. Stulo, p. 263.)

Diarrhœa.—When the ordinary remedies for diarrhœa fail, try the following: ℞. Ether. chlorici 3ij.; speciei pro conf. arom. 3ss.; mist. cretæ comp. ad ℥vi. Ft. mistura. Take a fourth part every half-hour till relieved. (Mr. G. B. Mead, p. 66.)

Dyspepsia.—In cases of irritative dyspepsia of some standing, when the digestion is painful and imperfect, give from fifteen to twenty minims of lactic acid in half an ounce of water at meal times. Lactic acid may also be used to improve the tone and power of the stomach generally. [Dr. Handfield Jones, p. 59.)

Epigastric Neuralgia.—Give the following: Infusi valerianæ, infusi cinchonæ, āa ℥iij. Sumat ℥i ter in die. Also apply a small blister over the part and dress it with acetate of morphia ointment, gr. j. to ℥j. (Dr. Lees, p. 57.)

Fissured Anus.—Introduce a speculum into the anus so as to bring the fissure into view. Then make an incision through the ulcer, beginning a little above its upper edge and carrying it through its base to a little below its lower edge. These fissures are in reality narrow oval ulcers at the margin of the anus, which look like fissures in consequence of the plicated arrangement of the end of the rectum. In slight cases touch the fissure with a solution of the nitrate of silver, a drachm to the ounce, or touch it slightly with the solid nitrate. This should be done every second or third day soon after the morning motion; and a bit of dry lint applied well to the fissure after each application. The bowels should be kept open by rhubarb or some mild laxative. (Mr. J. Hamilton, p. 156.)

In those very painful ulcerations and fissures of the anus, which used formerly to be relieved by division of the sphincter muscle, Mr. Copeland merely divides or cuts through the mucous membrane by a slight incision; a few of the muscular fibres of the sphincter are necessarily also divided. (Mr. T. B. Curling, p. 157.)

Fistula-in-Ano.—The opening into the rectum from the fistula will generally be just above the internal sphincter, although the probe may easily pass upwards to a considerable distance outside the gut; about five or six lines high up will generally be the situation of the orifice through the bowel,—we might, therefore, almost detect it with the eye by separating the sides of the anus sufficiently. In operating, pass a director through the fistula into the rectum, and where the internal aperture is near the anus, the director, by being a little curved, can be brought out again at the anus. Then pass a probe-pointed bistoury along the director and divide all the parts between the two openings, including the sphincter. Then stop up the wound for forty-eight hours with dry lint. If the wound heal indolently touch it with a solution of nitrate of silver (3j. to ℥j.) In blind internal fistula it is best to make it a complete fistula by making an opening at the side of the anus in the hard, tender, red part, and then operate as for complete fistula. Sometimes the external point where the fistula exists is not well marked; in this case a probe or curved director can sometimes be passed through the opening *from the rectum* till its point is felt through the skin, so as to guide the knife accurately. In blind external fistula a probe bistoury with the front of the end a little sharp and projecting is passed up the fistula till its point presses against the wall of the rectum, the nail of the fore-finger in the rectum scratches against the end of the bistoury till it pierces the bowel, and then is drawn downwards with the finger on the end, dividing the sphincter and all the parts below it. (Mr. J. Hamilton, p. 141.)

To ascertain whether the fistulous opening communicates with the rectum, inject a little tincture of iodine up the orifice, and at the same time introduce a finger up the rectum. If there be a communication the finger will probably be stained with the tincture. (M. Limauge, p. 144.)

Hematemesis.—When acetate of lead and opium, turpentine, gallic acid, tannin, &c., fail, we may try ergot of rye; give a drachm and a half of the powder divided into eight doses—each dose every four hours. (Mr. C. Neate, p. 63.)

Hemorrhoids.—Cauterize the base of the tumour by means of caustic potash and lime placed in the grooves of a particular kind of forceps described in M. Amussat's paper (Dr. Amussat, jun., p. 144).

In many cases the Vienna caustic may be used. Before using this, or nitric acid,

or any other caustic, it may be an advantage to ligature or encircle the tumour with M. Jobert's hemorrhoidal instrument, figured at page 152. When this is applied it will effectually prevent the caustic acting on the contiguous parts, and by its strangulation, may prevent much of the pain which would otherwise be caused. (M. Jobert, p. 151.)

Excision of internal hemorrhoids would be dangerous if we could not control the bleeding vessels. This, however, can now be done, and, therefore, the danger is proportionably less. Excision of internal piles may be performed with safety, and is more expeditious than nitric acid, and, if properly performed, as safe. (Mr. H. Lee, p. 155.)

Two methods may be used for internal piles: one, that of absolute removal by the heated platinum wire; the other, that of destroying the substance of the tumour *from the surface* by the application of a *disc-shaped* coil of incandescent wire. Small isolated and prominent internal piles may easily be removed across their base; but when the mass is larger, it is best to cauterize it deeply, from the mucous surface. (Mr. J. Marshall, p. 152.)

In ligaturing piles use *twine* rather than silk. In all operations on or about the rectum and anus, remember Mr. Copeland's golden rule, "*cut skin and tie mucous membrane.*" (Mr. I. B. Brown, p. 218.)

Hernia.—Before operating for hernia, read Mr. Luke's opinion on the "Strangulation test," at page 138.

Strangulated.—If you use opium, do it promptly and decidedly; give the following pill at once: Morphiæ muriatis, antim. potass. tart., of each one grain; and give a quarter of this dose every fifteen minutes for four doses. Then, if necessary, put the patient into a warm bath and attempt the reduction. (Mr. R. Hey, p. 139.)

Apply ice associated with permanent pressure. Begin with cold water and then with ice. Salt may even be mixed with the fragments of ice, and an elastic bandage is to be applied so as to compress the part. The pelvis is to be kept elevated. (M. Baudens, p. 231.)

JAUNDICE.—When arising from an excessive secretion of bile, *turpentine* is often very useful. Give one drachm every two or three hours. It may be mixed with nitrous spirit of ether to avoid strangury. (Dr. Gordon, p. 69.)

ŒSOPHAGUS, *Stricture of.*—If caused by an aneurism pressing on the trachea and nerve-trunks, the voice will be *stridulous*, and paroxysmal attacks of suffocative dyspnœa will occur. If caused by an abscess, the voice will not be lost. If caused by malignant disease, the recurrent laryngeal nerves will soon be involved, and *paralytic aphonia* will result, although the larynx may not be affected. (Dr. Risdon Bennett, p. 135.)

PROLAPSUS ANI.—Dupuytren's method of treating prolapsus ani, was by excising radiating folds of skin round the anus, while Guersant used the actual cautery. Duchaussay thinks these acted only by stimulating the sphincter muscle, and therefore he applies strychnine. Apply one-sixth to one-third of a grain of strychnine to a blistered surface in the neighbourhood of the anus. Repeat this occasionally according to its effects. (M. Duchaussay, p. 155.)

Apply nitric acid as follows: wipe the acid down the protruded part in separate streaks or tracts vertically from the sphincter down to the lowest portion of the gut; then replace the protrusion and allow no motion for two days. You may re-apply the acid at intervals of seven or ten days if necessary. (Dr. M'Dowel, p. 154.) Nearly the same practice is followed by Dr. Fleming. (See p. 153.)

STOMACH, *Dilatation of, with Obstruction at the Pylorus.*—A good diagnostic symptom of disease of the pylorus is the vomiting of sarcinæ with a peculiar yeast-like substance. Give half a drachm of sulphite of soda in an ounce and a half of water every four hours. If necessary increase the dose to a drachm. The sulphite of soda will destroy the sarcinæ, but it will not act upon a diseased pylorus. Where the disease, therefore, is simply from sarcinæ give the sulphite, but otherwise, it is not to be depended upon. (Dr. R. B. Todd, p. 59.)

STOMACHIC BITTERS, TONICS, AND APERIENTS.—*Quinine* strengthens digestion, especially when the stomach has been injured by drinking, or in persons exhausted by overwork. It does harm in organic disease, with a furred tongue, and when the urine throws down lithic acid, or lithate of ammonia. Give quinine half an hour or an hour before meals.

Calumba.—This is a sedative as well as a tonic.

Gentian and Chiretta stimulate the liver, as well as act as tonics on the stomach; whereas, quinine and quassia sometimes check the secretions of the liver, and are therefore not adapted to cases where the secretion of bile is defective.

Iron.—The citrate or ammonio-citrate mixed with a little bicarbonate of soda or potash, have the same effect as Griffith's mixture (the mist. ferri co.). Iron medicines may be given at meal-times.

Castor-Oil acts very mildly on the large intestines, and is therefore useful in convalescence from typhoid fever, in dysentery, and whenever we wish not to act too much on the large intestines.

Senna acts chiefly on the small intestines, and increases the secretion from the mucous membrane, and also that of the liver. It is, therefore, one of the best aperients in bilious affections.

Aloes and Colocynth act chiefly on the large bowels, and are, therefore, the best purgatives in disorders of the stomach. (Dr. G. Budd, p. 58.)

TEETH, *Cement for making them insensible to pain.*—Previous to stopping teeth permanently with any metal, use a cement composed of Canada balsam and slaked lime, which is introduced into the hollow tooth like a pill. The most sensitive tooth may, by repeating this application, be made quite insensible to the most severe operative proceedings. (Mr. P. Clark, p. 137.)

TOOTHACHE.—Sometimes this is owing to the stomach and is not relieved by any remedy, not even by the extraction of the tooth. In this case try an emetic of ipecacuanha. (M. Fredericq, p. 137.)

TÆNIA.—When the usual remedies for the expulsion of tape-worm fail, try *tatzé* and *saoria*. (M. Strohl, p. 65.)

AFFECTIONS OF THE URINARY ORGANS.

BLADDER, *Irritability of.*—Is the irritability direct or sympathetic? Whilst finding out this, you must use palliatives. Camphor and opium are the best. Three grains of camphor, with half a grain of opium, may be given two or three times a day, in the form of a pill. (Dr. Bence Jones, p. 80.)

1. *As a consequence of Gonorrhœa.*—Give iodide of potassium, and active tonics, as cinchona, with doses of Plummer's pill; or colchicum, combined with blue pill; or ten grains each of powdered guaiacum and calcined magnesia once or twice a day.

2. *As a consequence of diseased Prostate.*—Pass a catheter occasionally, *i.e.*, with intervals of two or three days or longer. If there be some little purulent discharge, apply a little nitrate of silver to the prostatic portion of the canal.

3. *When accompanied by more or less mucus in the urine, or muco-purulent deposit.*—Apply stimulating injections to the bladder. Nitrate of silver from half a grain to two grains to the ounce, increased, if need be, to ten and even fifteen grains to the ounce. Give half a grain to a grain of acetate of lead, with two grains of extract of opium. Give nitric acid, one or two minims to the ounce of water, when the urine is loaded with earthy phosphates. Creasote and copaiba are also recommended in some cases. The injection of simple warm water is often useful, and when the urine is very offensive, a solution of chloride of soda, from six to ten grains to the ounce of water, used once a day, is recommended. The vulcanized india-rubber bottle, fitted with pipes and valves, is the best instrument. In non-inflammatory cases, give infusions of diosma, or buchu, or uva ursi, combined with hyosciamus or camphor, and even copaiba or chios turpentine. Pareira brava is also strongly recommended, but often disappoints us. (Mr. H. Thompson, p. 83.)

DIABETES.—It is very probable that this depends on cerebral disease, and not on either the stomach or kidneys. Therefore, look out attentively for morbid cerebral symptoms. This is a new idea, and facts are not yet sufficiently numerous to confirm its truth. (Dr. R. H. Goolden, p. 70.)

Prevent the *arrest of the conversion of starch at the stage of sugar* when undergoing the process of digestion. Give a tablespoonful of German yeast two or three times daily in milk. (Dr. W. B. Herapath, p. 76.)

KIDNEY, *Irritation and Inflammation of, from a Calculus.*—Prevent the increase

of the concretion by correcting the unhealthy state of the urine according as acid, alkali, or oxalate of lime, predominate. Avoid severe exercise. Allay pain by the hottest hip-bath, at a temperature of 110° or 112° Fahr.; by chloroform internally and externally; by opium and the injection of warm water into the rectum. Guard against inflammation, by general or local bleeding; by aperients and sudorifics. (Dr. Bence Jones, p. 85.)

Lithotrity.—In the choice of subjects for this operation, take care, if possible, 1st, to let the age be between 30 and 60, with neither the irritability of youth, nor the debility of old age. 2nd. The urethra should be large enough to admit instruments of considerable size. 3d. The bladder should be healthy, and capable of holding six or eight ounces of fluid. 4th. The kidneys should also be healthy. 5th. The calculus should be loose in the cavity of the bladder. 6th. The diameter of the stone ought not to exceed 1¼ inch, as near as can be calculated. Where the above kind of case occurs, lithotrity and not lithotomy ought to be performed. (Dr. S Fenwick, p. 163.)

Simple enlargement of the prostate need not prevent the operation, but where there is inflammation or suppuration it is inapplicable, and lithotomy should be performed. Lithotrity should never be adopted, unless the bladder be free from inflammation, and one great means of knowing this is to ascertain the quantity of water which the bladder can hold. A bladder violently inflamed, is like a stomach inflamed, neither can contain much without parting with it. If the bladder can hold eight or ten ounces (other circumstances being favourable), lithotrity will generally succeed; but if it will contain only three or four ounces, this operation is not proper. As soon as the surgeon has filled the bladder, he should suddenly change the position of the patient backwards, so as to throw the stone into the back of the bladder. The manipulations can be carried on better in this place than nearer the prostate and the anterior part of the bladder, which is always more irritable and sensitive than the back part. (Dr. S. Fenwick, p. 165.)

Retention of Urine.—Alkaline urine in this case irritates the mucous membrane of the bladder: whenever urine remains too long in the bladder it is disposed to become alkaline or ammoniacal. Therefore, in retention of urine always empty the bladder once or twice a day. The object of this is not only to relieve the patient of suffering but to prevent the urine becoming alkaline, and consequently irritating to the mucous membrane. If, then, the morning or mid-day urine be alkaline, use the catheter night and morning. If the cause of the retention and suppression cannot be reached by medicines, then we must relieve the congestion of the kidney, by acting strongly on the bowels by hydragogue medicines, and on the skin by sudorifics, to relieve the poisoned system, and then to have time to use other remedies. (Dr. Bence Jones, p. 81.)

Stricture of the Urethra.—Pass a catheter, if possible, through the stricture. This catheter or sound is grooved, and in this groove is a cutting blade, which can be gently opened as the instrument is *withdrawn*, so that the stricture is effectually divided. (M. Maisonneuve, p. 226.)

Test for Sugar.—If a freely alkaline solution of chromate of potash be mixed with urine suspected to contain sugar, and boiled, the liquor will assume a deep sap-green colour, arising from the decomposition of the chromic acid, the reduced oxide of chromium being held in suspension by the potass. Such is the sensitiveness of this test, that 5 or 6 drops of saccharine urine diffused through water, is easily detected. Therefore, always have ready a mixture in equal parts of a solution of the neutral chromate of potass, and liquor potassæ. This should be labelled "*Test for Sugar.*" A piece of white paper at the back of the test tube will allow the colour to be better seen. (Mr. J. Horsley, p. 77.)

Vesico-vaginal Fistula.—Dr. Wagner of Dantzic, operates for this defect in a new way. For a minute detail of his proceedings see page 169 of this volume.

VENEREAL AFFECTIONS.

Bubo.—It is seldom necessary to open a bubo. Employ counter-irritation. Take a solution of nitrate of silver (one drachm to two drachms of water), with the addi-

tion of three drops of strong nitric acid. This is to be painted freely into the skin over the inflamed gland. It causes great pain and soreness, which is followed by rapid diminution of the gland. This treatment may be extended to other cases in which inflammatory deposits, whether purulent or fibrinous, require to be absorbed or diminished. (Mr. J. Hilton, p. 178.)

Syphilis.—Treat the *infecting* chancre, the true firm *indurated* sore, with mercury. The *unindurated* sore may be safely treated *without* mercury. In treating the former, in Paris, the proto-iodide of mercury is used, but this does not answer so well in this country as mercurial frictions, blue pill, or the grey powder.

The *Phagedænic Chancre* is treated by Ricord, by first destroying the whole of the unhealthy surface with the red-hot iron, previously placing the patient under the influence of chloroform. After the sloughs have separated, the surface is covered with strips of ammoniacum and mercurial plaster, and doses of tartrate of iron are given with great success, in the less obstinate cases. The most frequent variety of sore in London is the phagedænic, with induration. In this, the actual cautery does no good; mercury must here be given, and the system supported by bark, wine, or steel. (Mr. W. Acton, p. 172.)

AFFECTIONS OF THE SKIN, &c.

Erysipelas.—Early in the case, when the pulse is hard and full, and when there is no intestinal irritation, give an emetic of tartarized antimony and ipecacuanha, followed by minute doses of the antimony, till the pulse softens. Then give wine and good nutriment, especially if the tongue be brown; give from four to six ounces of wine daily with sago, beef-tea, &c., but avoid giving the wine or brandy so long as the fever is high. The above quantity ought sometimes to be doubled. As a topical remedy, mercurial ointment is as good as any, and perhaps better, but use it cautiously, or the patient will be suddenly salivated. (Dr. J. S. Hughes, p. 174.)

Favus.—Apply the following ointment:—B. Sulph. sublim. ℔ss.; hydrarg. ammonio-chlorid. ʒss.; hydrarg. sulphureti cum sulph. ʒss. Leviga simul, dein adde olivæ olei, ʒiv.; adipis recentis, ʒxvi.; creasoti ♏xx.; misce. Sometimes four or five nightly applications will perfectly clean the scalp, but it requires to be used continually, a little every night, to keep off the disease. The same ointment may be used for scabies and porrigo. (Mr. J. Startin, p. 177.)

Porrigo.—Use the ung. sulph. comp. of the Pharmacopœia of the Hospital for Skin Diseases. See "*Favus.*" (Mr. J. Startin, p. 177.)

Scabies.—Use the ung. sulph. comp. of the Pharmacopœia of the Hospital for Skin Diseases. See *Favus.*" (Mr. J. Startin, p. 177.)

Small-Pox, *to prevent Pitting in.*—Make a saturated solution of iodine in spirits of wine. Brush this tincture freely over the face once or twice daily, from the earliest period of the eruption, and continue it during the maturation of the pustules. (Dr. Crawford, p. 175.)

Take of carbonate of zinc three parts, oxide of zinc one part, rub these in a mortar with olive oil to a proper consistence. Apply this all over the face, and renew it from time to time, so as to take the place of the natural scab. (Dr. Bennett, *Dublin Hosp. Gaz.*, *Μφγ* 1, 1854, p. 107.)

Ulcers, *Cancerous.*—For the prevention of smell, use the following powder made into a paste with water, brushing the parts over once a week or fortnight: Hydrarg. chloridi, ʒijss; hydrarg. bisulphureti ʒij; acidi arseniosi 3j.; misce. (Mr. J. Startin, p. 209.)

Of the Leg.—Instead of applying common adhesive plaster and bandage after Mr. Baynton's method, use strips of linen or calico, well soaked in water, just as you use the sticking plaster. Over these wet strips of linen apply the common bandage. (Mr. C. Holthouse, p. 176.)

AFFECTIONS OF THE BONES AND JOINTS.

Ankle-Joint.—In operations at the ankle-joint, arrange your incisions so that you can gain access to the tarsal bones, and, if the disease be more extensive than anticipated, that you can even remove the entire foot at the joint, if you wish. Make

"a transverse incision across the sole of the foot, commencing about three-quarters of an inch in front of one malleolus, and ending at a similar point in front of the other malleolus: a second incision is then made in the median line, beginning over the tendo-Achillis on a level with the ankle-joint, and joining the former at right angles in the sole of the foot. The two lateral flaps thus marked out being next dissected upwards close to the bones, the calcaneum and astragalus are freely exposed. By division of their ligamentous and tendinous connexions, one or both of these bones may be easily removed;" and other parts may be easily reached by extending the median incision a little forward. If from the great extent of disease it is found necessary to remove the entire foot, it may be done by uniting the two extremities of the transverse incision by a curved incision across the dorsum of the foot. (Mr. T. P. Teale, p. 117.)

FRACTURE, *Compound.*—When ought amputation to be resorted to where gangrene supervenes? Military surgeons, as Larrey and Guthrie, say you may amputate when the gangrene is spreading. Most surgeons say, delay till the gangrene has ceased to spread, and the line of demarcation has been fairly established. Perhaps the best practice is a middle course, viz., if within a certain period no disposition to an arrest of the gangrene appear, we ought to operate while the patient is strong enough to bear it. Should, however, the gangrene spread quickly, accompanied by great prostration of strength, delirium, and rapid pulse, the operation is inadmissible. (Dr. S. G. Wilmot, p. 126.)

Ununited.—When fractured bones will not unite, the seton, as first used by Dr. Mott, will cause such a degree of inflammation at the ends of the bones as to cause their union when not too far asunder; but when too movable, or too far asunder, or when from other causes they will not unite, use the *wire-ligature.* Bore canals in the cylindrical parts of the bone, about three-eighths of an inch long, then pass a copper wire (No. 16) through these canals, and twist it tightly till the ends of the bones come firmly in contact. This practice has generally been applied only in old ununited fractures, but it is a question whether or not it may also be used in more recent cases, *when the bones are very movable.* In military surgery it may be useful. Any kind of wire will do. (Mr. J. Russell, p. 123.)

Of the Patella.—Use a knee-cap of gutta-percha moulded to the front aspect and sides of the joint, and with a hole cut in the centre, large enough to admit the patella. The two halves of the bone are then to be brought together, and the splint, lightly padded, placed over them and secured by a bandage. (Mr. Le Gros Clark, p. 129.)

JOINTS, *Diseased.*—M. Tessier, of Lyons, has shown that prolonged rest alone, even where little or no disease previously existed—a rest for five or six months—is sufficient to cause absorption or ulceration of the cartilages of joints. This prolonged rest also aggravates existing disease. (Mr. Coulson, p. 93.)

In the early stage of inflammation of joints, apply plenty of leeches, except in scrofulous subjects. To relieve pain, use the following:—moisten a bit of lint with 20 or 40 drops of chlorinated hydrochloric ether, and apply it to the joint—it acts beautifully. After bleeding, bring the patient rapidly under the influence of calomel, or give tartar emetic. The limb should have absolute rest in the most favourable position, for 5 or 6 weeks, but not longer, for fear of stiffness, &c. The joint may be splintered with leather in the following way:—Get some thick leather, soften it in warm water, and mould it, when pliable, to the joint, and when hard, remove it and line it with wash leather. This forms an admirable splint. One may be made for each side of the joint. Evacuate any pus that may be formed, in good time. Promote the absorption of such materials as the inflammation may have deposited, by counter-irritation, by means of blisters, croton oil, discutient plasters, composed of equal parts of galbanum and mercurial plaster, pressure, friction, the cold douche, "Scott's dressing." (Mr. W. Coulson, p. 96.)

Ulceration of Articular Cartilages of.—Acute inflammation is marked by rapid effusion within the joint, and uniform swelling; in disease of cartilage, the swelling is slower, and outside the joint, not coming on till some time after the first sensations of uneasiness. In chronic degeneration of synovial membrane, the swelling, although indolent, is more rapid than when the cartilage is diseased. The swelling is sloughy, elastic, and superficial. Scrofulous disease of the heads of the bones is most likely to be confounded with disease of cartilage; but ulceration of cartilage chiefly

belongs to the middle and after periods of life, and to dyspeptics. In scrofulous disease, the pain generally comes on *after* the formation of pus. In ulceration of cartilage, the pain comes on earlier, and may continue a long time without pus forming at all. Perfect repose, accomplished by *splinting* the joint, either by Mr. Scott's method, or by leather or gutta percha splints *moulded* to the joint. Give mercury early, when not contra-indicated, colchicum, with alkaline aperients. By and by apply counter-irritation. (Mr. W. Coulson, p. 99.)

Scrofulous Disease of.—In the early stage of the disease, Sir B. Brodie says, "all kinds of counter-irritation, such as blisters, issues, setons, and tartarized antimonial ointment, are not only not useful, but actually mischievous." The only exception is in cases of hip-joint disease of a painful character. These counter-irritants certainly excite absorption of fluid which may exist in the joint, but do not act on the diseased structure. Perfect repose of the parts in the horizontal position, with leather or gutta-percha, or other kinds of splints lightly applied, is, no doubt, the best local treatment; at the same time, allowing the patient as much out-door amusement as possible. (Mr. W. Coulson, p. 103.)

To promote absorption in these cases, use the following ointment: veratrine, grains 5 to 10, dissolved in spirits of wine, and add to one ounce of axunge. This is not to be used in acute affections, but in chronic. Rub about the size of a small bean upon the joint for a quarter of an hour daily. If violent itching be produced, use a little glycerine and water. (Dr. Klingner, p. 123.)

Knee-Joint, *Amputation at the.*—First make a small anterior flap, drawing the knife across the front of the joint, and then inserting the point of the blade behind the femur, thrust it through to the other side, close to the condyles; then carrying it downwards cut the posterior flap from the calf of the leg. The saw is then applied a little above the condyles, and the flaps brought together as in an ordinary amputation. Mr. Potter, of Newcastle, saws through the bone before making the posterior flap. Take the full length of the calf for the posterior flap, as the soft parts at the back of the thigh contract very much in the course of time. (Mr. W. Fergusson, p. 110.)

Thigh, *Amputation of.*—The longer the stump the better, and, if circumstances warrant, operate at the knee-joint. Progression is greatly improved by the long stump thus left, the limb does not describe the semicircular curve which it does with a short stump. The walk is altogether better. (Mr. G. M. Jones, p. 113.)

AFFECTIONS OF THE EYE.

Cornea, *Vascular Opacity of.*—When caused by granular conjunctiva, apply various stimulants or astringents, as nitrate of silver, sulphate of copper, acetate of lead, alum, or zinc, so as to cure the granular condition of the conjunctiva, which causes the opacity. If the granulations be small, round, vascular, and sensitive, use a *mild* stimulant, as ten grains to the ounce, of nitrate of silver or sulphate of copper; if large, pale, and insensible, the solid nitrate may be applied freely and frequently. The sulphate of copper is perhaps preferable to nitrate of silver. It may be rubbed freely over the surface daily, or every other day. Weak solutions of nitrate of silver only irritate. (Mr. G. Critchett, p. 182.)

Ophthalmoscope, *Improved.*—This instrument enables us to throw a large quantity of light into the eye in such a way that the observer, without intercepting the rays which enter, may receive directly into his own eye those reflected from the retina of the explored organ. The instrument is a small round concave mirror, about two inches in diameter, with a focal distance of four and a half inches; the silvered surface being accurately adapted to a plate of blackened copper. The centre is perforated by a small hole for the observer to look through. It is obvious that the observer, by having a light at the back of his patient, can reflect it from his mirror into the eye, and by means of his little peep-hole he can distinctly observe the interior of the organ, the pupil being previously dilated by belladonna. This little instrument may be held in one hand, but Mr. Dixon adapts it to a spectacle frame, so as to have both hands at liberty. (Dr. E. Williams, p. 179.)

Ophthalmia, *Purulent.*—If there be extensive chemosis and swelling of the lids,

divide the vessels of the conjunctiva, with a small knife, so as to bleed them as recommended by Mr. Tyrrell. Keep a bit of lint dipped in cold water constantly applied to the lids. Apply a solution of nitrate of silver (five or ten grains to the ounce) between the lids *every hour, or two hours*, as the patient can bear it; but previous to each application of the drops, wash out all discharge of pus, by means of a syringe and warm water. (Mr. G. Critchett, p. 305.)

Strumous.—In the intolerance of light which generally accompanies this disease, use the following:—Coniin, gr. ½; olei amygdal. dulc. 3j. Mix to form a thick fluid, with which the lids may be daily pencilled. (M. Mauthner, p. 184.)

Tinea Tarsi.—First carefully remove all the scales and crust from the eyelid, then rub the solid nitrate of silver along the edge of the lid; repeat this twice a week. (Royal Ophthalmic Hospital, Moorfields, p. 184.)

Smear the edge of the lid every night with mild mercurial ointment (ammonio-chloride, ten grains to the ounce), and give an arsenical tonic in combination with iodide of mercury. (Mr. J. Startin, p. 185.)

MIDWIFERY AND DISEASES OF WOMEN.

Abortion, *caused by a Syphilitic Taint.*—Secondary syphilis is not curable in the pregnant female. Both parents must be affected with mercury for six or eight weeks; and this must be done in the female when not pregnant. (Dr. R. Johns, p. 201.)

Cervix Uteri.—For the exploration of the cervix uteri and using various applications, an ingenious speculum is used by M. Jobert; a very small instrument with a long handle,—sufficiently small to be passed up the common vaginal speculum. See p. 203, for a description. (M. Jobert.)

Polypus Uteri, *Detection of.*—The polypus will be best detected during the relaxation and forcing down, attendant on an attack of flooding. The bi-valved speculum assists by expanding and opening the os. Dr. Locock particularly points out that the tumour is always best found out *during hemorrhage*, and indeed it cannot be detected at all when very small, and only in the cervix, in the intervals of the attacks of hemorrhage.

For bringing a polypus into view, the ergot of rye may sometimes succeed, but *galvanism* is more safe and more efficacious. The os and cervix uteri may be dilated by means of sponge-tents,—use two or three graduated sponge-tents in succession so as to open the canal for inspection. M. Jobert de Lamballe has recently contrived an *intra-uterine* speculum. See "Cervix Uteri."

In those cases which indicate the necessity of exploration beyond the uterus, first bring the polypus in view, if you can, by dilating the os uteri by sponge-tents, and giving repeated doses of ergot of rye, assisted by the application of galvanism. If you cannot get hold of the tumour so as to encircle the neck, pull it lower down by the aid of a tenaculum, if this operation be not particularly resisted—and now try again to ligature the neck of the tumour. (Dr. R. Barnes, pp. 213, 215.)

Prurigo Ani et Vaginæ.—Take equal parts of sulphate of zinc and of alum, roughly powder them, and put them into a glazed earthenware vessel; put it on a slow fire and leave it there till bubbles of air are no longer disengaged, and till the mixture acquires a stony hardness; then powder it, and throw it by small portions at a time into boiling water. Filter, and apply to the parts with a sponge and on linen (Dr. Richart, p. 228.)

Puerperal Convulsions.—In fulness of the vascular system, blood-letting is the most powerful sedative of spinal action. Hence, when the convulsions depend on the stimulation of the spinal marrow, by excess of blood, or on mechanical pressure exerted by the blood on that organ, or on the medulla-oblongata, we must bleed. Remove, also, if possible, the *eccentric* cause of irritation, whether it exists in the womb, the rectum, or the stomach. The blood-letting must be regulated, not by the violence of the disease, but by the state of the circulation *in the interval of the fits.* Evacuate the liquor amnii, which, by relieving the distension, diminishes the size of the womb, and the quantity of blood circulating in it, and also makes the organ less irritating to the general system. Evacuating the liquor amnii, is to the womb what an emetic or an enema may be to the stomach or bowels. Do not trust to chloro-

form, as, "when the functions of the true cerebral or sentient portion of the nervous system are diminished or abolished, as is the case in anæsthesia, the irritability of the excito-motory, or true spinal system of nerves, is increased." (Dr. Tyler Smith, and Mr. A. B. Steele, p. 196.)

Sore Nipples.—Apply a bit of lint dipped in the tincture of benzoin; remove this, and re-apply from time to time so as to form a coating, which suckling does not remove. (M. Bonrdel, p. 228.)

Uterine Hemorrhage.—Give from five to ten grains of capsicum every ten minutes, until the hemorrhage ceases. (Dr. Brock, p. 206.)

Womb, *Cancer of*.—For the prevention of fetor use arsenical injections of the strength of from two to eight grains of white arsenic in a pint of water. Charcoal and other disinfectants destroy fetor, arsenic *prevents* it. (Mr. Lloyd, p. 209.)

Prolapsus of.—Diminish the size of the vaginal canal, by producing in succession numerous little cicatrices, by small forceps, which are introduced into the vagina, and *pinch* up certain portions. They are allowed to remain in the canal till detached. (Dr. Desgranges, p. 202.)

MISCELLANEA.

Charcoal.—This is an admirable *deodorizer*. Take two sheets of cotton wool and between them put a tolerably thick layer of powdered charcoal, and *quilt* the whole together in small segments the same as in making a cushion. This charcoal quilt is to be placed over any nasty smelling sore, and will prove a good disinfectant. (Mr. Wormald, p. 176.)

Chloroform, *Artificial Respiration after the use of*.—In cases of poisoning by chloroform, before having recourse to artificial respiration, *draw forwards the tongue*. As soon as breathing ceases in these cases, the tongue falls back and closes the orifice of the glottis. Pull the tongue well out of the mouth and pass a hook through the tip so as to command it while artificial respiration is carried on. (Mr. R. E. Bickersteth, p. 301.)

Cod-liver Oil, *Test for*.—Drop a little sulphuric acid, *guttatim*, into the oil. It causes a centrifugal movement, and a beautiful violet colour, which changes to purple on agitation, and ultimately to a rich sienna brown. This is best seen by adding one or two drops of the acid to half a teaspoonful of the oil spread out on a white porcelain plate. Sulphuric acid when added to olive oil causes a dirty grey colour—to poppy-oil a deep yellow colour—to ordinary fish-oil a deep brown colour. (M. De Jongh, p. 236.)

New Mode of giving.—Take the yolk of one egg; sugar, two ounces; orange-flower water, one ounce; cod-liver oil, three ounces; essence of bitter almonds, one drop. Either the sixth or eighth part will be a dose, according to the quantity of oil which is intended for the patient.—*Ann. Clin. de Montpellier*, (*Lancet*, Aug. 5, 1854, p. 104.)

Electricity.—The most *handy* kind of machine is that of Mr. Hearder. It may be placed in a box seven inches long, seven and a half inches deep, and eight inches wide. When using it neither the coil nor the battery need be removed from the box. (p. 209.)

Intense Cold, *Uses of*.—First make a freezing mixture as follows: Pound a piece of ice as big as an orange, and put the material on a sheet of paper, mix this thoroughly, with a paper folder, with half its weight of common salt. Put this mixture into a net of the size required, and as soon as it begins to melt it is ready for use. The net is now to be placed upon the part required, and frequently to be raised in order that fresh particles of the mixture may be brought into contact with the skin. The skin is benumbed by this application in a few seconds, it first becomes white, showing that the skin is influenced; if prolonged, the adipose matter under the skin begins to freeze, and the part becomes hard as well as white. The freezing may be extended even to deeper parts if necessary.

Remedial uses.—In acute lumbago, chronic rheumatism, sciatica, acute rheumatism, except where the head is implicated, ophthalmia, glandular inflammation in the neck and groin, orchitis, erysipelas, painful nodes, neuralgic head-ache, cancer, &c. (Dr. J. Arnott, pp. 248, 249.)

IODIDE OF POTASSIUM.—It is said that this medicine is more valuable when combined with ammonia, gently stimulating the stomach, diffusing the blood, and with it the medicine through the system, and, by chemical decomposition, liberating the free iodine, and thus sending it on its salutary message. (Dr. Gull, p. 300.)

OPIUM, *Poisoning by.*—Always have ready the following preparation of belladonna: Take four ounces of the leaves of belladonna, and two pints of rectified spirit, and percolate. Of this give six drachms in doses of about one drachm every half hour. In another case, ten drachms of the above tincture of belladonna were given in the course of one hour with success. (Dr. T. Anderson, p. 301.)

SPLINTS.—See page 129 for an account of felt splints, which may be very useful in some cases. (Prof. F. H. Hamilton.)

WINE, *Medicinal use of.*—As a *diuretic*, Moselle may be as useful as gin or whiskey. It contains an excess of salts, and may prove as energetic as, and less heating than, the essential oil in gin or whiskey.

In *diabetes*, use the claret wines, which are free from sugar, and contain much tannic acid.

In *dyspepsia and gout*, use the wine most free from ultimate acidity and least stimulating. The best is the *least acid claret* wine; next, strong or perfectly dry champagne. Good Mansanilla will answer the purpose, and is much cheaper than Amontillado sherry. But, after all, weak brandy and water, or some pure spirit and water, is most free from acid and sugar, and answers best in dyspepsia. (Dr. Bence Jones, p. 298.)

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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE,

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PRACTICAL MEDICINE.

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON TYPHOID PNEUMONIA, &c.

By Dr. WILLIAM STOKES, Regius Professor of Physic in the University of Dublin.

[THE remarks of this eminent physician, on the various manifestations and complications of fever, have appeared to us so important that we have largely availed ourselves of his lectures, and we hope that in some measure we may be the means of spreading the very important views of practice which these lectures point out. There is a class of cases of typhus in which the lung becomes consolidated or congested more or less.]

To these cases the general term of "typhoid pneumonia" has been given. But you will be convinced, when your experience has been enlarged, that under this term many different forms of disease have been classed; and it is very doubtful whether a true pneumonia is ever developed in the course of a typhus fever. You will meet with the physical signs which attend pneumonia; but these, as you must know, are insufficient to establish the existence of the disease; and even these very physical signs are seldom so well marked, so complete, as it were, as in simple inflammation of the lung. Nor, again, do they follow in the regular succession which we find in true pneumonia.

You know that I am not fond of fine-drawn distinctions in disease, especially when these distinctions are based on some anatomical speciality, and do not lead to any differences in our principles of treatment; and we shall arrive at more practical results sooner by reviewing some of the more striking cases, I will not say of typhoid pneumonia, but of the acute asthenic diseases of the lung which tend to consolidate that organ. I mean, when using the term "acute asthenic," to imply a disease which forms more or less rapidly, and is associated with, or secondary to, a condition of the system in which, with fever of some kind, we find signs of debility. We may recognise the following forms:—

1. Congestion, with more or less consolidation in cases of what is called "diffuse inflammation," "erysipelatous inflammation," or, by some, phlebitis.
2. Similar or nearly similar conditions (so far, at least, as we are taught by pathological anatomy) in cases of purulent absorption, with or without manifest phlebitis.
3. The intercurrent disease of the lung in cases of the eruptive fevers, when they are of the low, putrid, or malignant type.
4. Congestions and semi-consolidations of the lung, as intercurrent affections in typhus fever.
5. Analogous conditions arising in the course of the non-maculated or the so-called "typhoid fevers."
6. The disease occurring in connexion with delirium tremens from excess. In such cases, you will often find a group of asthenic local diseases, which are generally seated in the stomach, heart, the bronchial membrane, the parenchyma of the lung, and even the pleura. In some instances we find that the patient has also typhus fever.

7. Rapid, extensive, and complete consolidation of the lung occurring in the course of a typhus fever. In some instances the patient dies asphyxiated; while, in others, a portion of the pulmonary strictures falls into sphacelus, and death takes place with the symptoms of acute gangrene of the lung.

Such are some of the more prominent cases which have been classed under the head of typhoid pneumonia. There is another of which we have had many examples, and yet I do not wish you to take what I am going to say about it in any other way than in the light of suggestions. The case, as I have said before, is by no means uncommon. The patient is attacked with the usual symptoms of typhus fever, and he comes into hospital after two or three days' illness. There is nothing about him to make one think that his disease will not run the usual course of the epidemic of the day, and we are prepared to expect a fever of at least a fortnight's duration. On admission, he may have no symptom which would call attention to his chest; but, as early in some cases as the beginning of the fourth, and in others of the fifth day, it is discovered that the upper lobe of one lung is solid, or nearly so. The clavicle is quite dull on percussion, so is the scapular spine, and the dulness extends to the line of the mamma. This discovery has been so often made accidentally, that I am sure many of such cases have passed unnoticed, at least, where the attendant is not well informed as to the insidious nature of typhus local diseases, and does not make it a practice and a duty to examine daily, as far as he can, the condition of every organ.

But the most remarkable circumstance in these cases is, that the constitutional disease seems to be cut short. The expression of fever leaves the countenance; the peculiar colour or hue of typhus disappears; the eyes become bright and intelligent; the tongue cleans; and the pulse comes down to a natural state. And thus we have seen patients so altered in the course of twenty-four hours, that one had some difficulty in recognising them. All the symptoms of typhus were gone, and nothing remained but the consolidation of the lung. And this, too, is not attended with any notable suffering. There may be a little cough, some dull pain, or an inability to lie on one side; but that is all. The respiration is scarcely, if at all, accelerated; in fact, it would seem that there was no irritation or excitement of the organ; and the case is another proof of how much less the sufferings in disease are connected with the mechanical than with the vital conditions of organs.

This local disease, too, is generally easily managed. A small blister or two, the application of the tincture of iodine, and the exhibition of a little hydriodate of potass, with or without a tonic, will remove it. Indeed, the cure is often so rapid, that I have thought that our remedies had nothing to do with the result. How are we to look at such cases? That they are not examples of inflammation of the lung is plain; and it appears probable that, if this local disease had not occurred, the patient would have gone through the course of the fever of the day. Does it not seem as if the constitutional disease exhausted itself, as it were, in the production of the local affection, just as, in certain cases of simple variola, we see the fever to subside on the appearance of the pustule? I do not know whether such cases have been observed elsewhere, but of their existence we have had abundant proofs. It is worthy of remark, too, that when we compare these cases with the ordinary forms of typhus, attended with secondary disease of the lung, the local affection is here developed at an unusually early period; and it may be, that, in the more protracted cases of fevers, the nature of which is to develope local affections, the periods of this development and of the cessation of the fever may also be coincident. We do not, however, find that this is so common as to establish a rule. Let us, assuming that these curious cases were really examples of typhus with a secondary deposit, again compare this with the more ordinary forms of the disease, and we shall find that they want two important characteristics of the longer fevers; one, the successive production or the simultaneous production of various local diseases; and the other, the occurrence of that secondary inflammation or irritation of the parts in which the deposit takes place. That the latter circumstance is one of great weight in relation to the preventing or delaying of crisis, it is impossible to doubt. As to the case of successive or simultaneous productions of local diseases, this, at all events, marks a more severe and complicated disease.

I will not here enter into the wide subject of crisis in fever; yet I may point out to you, as a matter well worthy of investigation, the possibility of the occurrence of

crisis by other modes than those which are generally enumerated; thus we may have a crisis without sweating, or diuresis, or hemorrhage, or diarrhœa, but which takes place by an internal and silent change in the condition of an organ, and yet a change which will, or may, itself spontaneously disappear.

Here let me warn you against the error which so many fall into with respect to these various cases of disease of the lung, arising in the course of some form of constitutional disease or fever. They are usually set down as pneumonia, typhoid pneumonia by some. Now, the name itself would be of little moment if its adoption did not lead to errors in practice. And, although it cannot be affirmed with certainty that in none of these cases is there pneumonia, yet we have good grounds for believing that, in many of them, inflammation, as the term is commonly understood, is either absent from the first, or, if it occurs, that it is only secondary to a special lesion induced by some form of constitutional disease.

It is extremely difficult to present to you any well-defined classification of the various forms of diseases which have been described under the head of typhoid pneumonia, or to draw the line between simple asthenic inflammation of the lungs and those conditions which have been described from an early period under the terms of bilious, putrid, or typhoid pneumonia. And, observe, that when I make use of the term asthenic pneumonia, we refer more to the condition of the general system than to the activity or inactivity of the local disease. For, so far as local inflammatory action is concerned, there is abundant proof that it may originate and proceed with rapidity, and even with vehemence in the very last periods of life, so that the disease may be sthenic *quoad* the local condition, and yet the case itself be asthenic in reference to the general state of the economy. Much of the confusion with regard to this subject has arisen from the circumstance, that too great weight was attached to the presence of certain physical signs, which were taken as always indicating similar conditions. The succession of the signs of crepitus, dulness, cessation of vesicular breathing, and its replacement by bronchial respiration, has been admitted to indicate a simple pneumonia, in which the local disease is the principal pathological condition, and the fever only a secondary one. But it is certain that this train of phenomena, or some modification of them, may occur under exactly opposite circumstances, the local disease being symptomatic of the fever, and not the fever of the local disease. And there is the strongest reason for believing, that even though the mere anatomical condition of the lung in the two cases be similar, yet there is an essential, a vital difference, and that practically we cannot deal with the local disease in the latter case as if it were an original affection. This applies to all those cases of disease with the physical signs of pneumonia, which are secondary to any forms of fever, whether it be typhus or typhoid, whether it be variola or erysipelas, purulent poisoning of the blood, glanders, malignant scarlatina, or malignant measles. In these cases, even though the physical signs accurately correspond with those of the typical pneumonia, which, by the way, is by no means always the case, we must believe that we are dealing with a special condition of parts, and a condition not only special as compared with ordinary pneumonia, but having also another speciality—namely, that which is derived from the parent malady.

In the present state of our knowledge, we cannot declare that any special anatomico-pathological condition exists by which we can distinguish these secondary diseases one from the other; but we may say this much, that practically they all appear to agree in being indicative of an asthenic state of the system, and that, therefore, the supervention of their physical signs at any period of those various diseases, must not be permitted to divert your attention from the general condition of the patient, or make you proceed to treat a case as one of sthenic pneumonia because it has some, or even all, the physical signs of that condition. Do not suppose that I am taking up your time unnecessarily by insisting on these points, for they lead us directly to deal with one of the greatest, if not the greatest and most wide-spread error in the practice of medicine; namely, the treatment of all local, acute, and febrile diseases as inflammations. Here is a group of acute, local, and febrile diseases, and not only this, but a set of cases exhibiting some or all of the physical phenomena of acute pneumonia; and yet, if we subjected them to the ordinary routine treatment of inflammation, the worst consequences would almost certainly follow. You must learn to look at the antecedents and the accompanying general phenomena of these diseases, and set your face against the adoption of any

treatment which is based on the doctrine that they are original inflammations. I believe that the erroneous views to which I allude, are, I am happy to say, every day becoming less and less frequent (thanks to our improved system of clinical instruction), and the independent spirit of investigation which now animates so many of our students. Notwithstanding, they are still too often acted upon, and over and over again patients who have enough to contend with as the victims of some fell fever or other constitutional disease are lost, or assisted to their death by the adoption of a local or general antiphlogistic treatment, in consequence of the physical signs of a pneumonia being discovered; their stimulants are withheld or withdrawn; tartar emetic, or mercury, or even bloodletting is rashly resorted to; and it often happens that, even though the physical signs of the pneumonia are removed or modified, the patient sinks from the combined effect of the original disease and the exhaustion produced by this ignorant and benighted treatment. I do not think that any of you will fall into these or similar errors after what I have so often said; it will, at least, not be my fault if you do.

Let us now consider what we may term the parenchymatous affections of the lung in fever, or, if you will, the typhous disease of the pulmonary structure. It may be stated generally, that whatever be the differences in the various cases of this affection in typhus, the local disease follows the general law of other lesions secondary to the fever; that is to say, it agrees with them in its mode of invasion, its latency in the earlier periods of its development, its frequent and spontaneous retrocession, and lastly, in its pathological effects. It is quite true, that, as compared with the best marked examples of acute sthenic pneumonia, it is not wanting in any of the physical signs of that disease taken singly, but it is generally different from it in the order or arrangement, as it were, of these physical signs. And, indeed, I think it is an extremely rare circumstance to observe in the course of a typhus fever the rise, progress, and retrocession of a pneumonia which has passed into hepatization, as we so continually see in ordinary cases of the disease. I have already drawn your attention to those curious cases of consolidation of the upper lobe of the lung; now, whether these be genuine examples of an arrested typhus or not it is difficult to say, but their whole history and progress is very different from that of ordinary pneumonia; and I repeat, that there is nothing more rare than to see in the course of a typhus fever that regular succession of phenomena with which Laennec has made us so familiar, as indicating the several successive stages of an acute pneumonia occurring under what may be called its normal conditions.

The most common case is the occurrence, generally at an early period, especially in the maculated forms, and often at a later period in the non-maculated or so-called typhoid fevers, of a well-marked crepitating râle in the lower lobes of one or both lungs; it is generally much more extensive and distinctly marked in one lung that in the other. The amount of dulness is seldom very great, and we find the disease, as it were, to linger, and for days together to show no disposition either to produce solidity of the lung on the one hand, or to proceed to resolution on the other. The disease is often quite latent, and is only recognisable by careful physical examination; and its discovery, as you will readily understand from what I have said before, is sometimes an unfortunate circumstance for the patient. In the present state of our knowledge we must believe this condition of the organ to be either the result of a certain amount of typhous deposit into the lung, or of a special state, an inflammation, if you will, which is, however, under the general law of the fever, partaking of its specific character, and capable of spontaneous retrocession. It is seldom attended by pain or by hemoptysis, and it constantly exists without any important modification of the general symptoms of the case.

The second form of the disease is of a more serious character, and seems to be connected either with an original pyogenic disposition, itself secondary to typhus, or we may suppose that the typhoid deposit undergoes a rapid purulent transformation, having some resemblance to the third stage of pneumonia, as described by Laennec. I have not myself seen a sufficient number of these cases to justify me in speaking very decidedly as to their physical signs; but I think I have seen enough of them to warrant me in believing that the course of the disease is different from that of ordinary suppurative pneumonia. We have not observed the intermediate stage of well-marked hepatization between that which is characterised by the occurrence of early râle on the one hand, and the signs of interstitial suppuration on the

other. The complete dulness and the bronchial respiration which accompany the third stage of pneumonia as described by Laennec we have not observed, the physical signs being principally a persisting râle passing from a fine into a large crepitus, and semi-dulness on percussion. On dissection, the lung is found soft, friable, of a greyish-red colour, but still very permeable to air, though infiltrated with purulent matter. It is as if the purulent secretion took place coincidently, with or immediately after the first or congestive stage. Some of the patients have had sweatings and a sanguinolent and somewhat sanious expectoration; but we have not hitherto observed the ordinary prune-juice sputa in these cases. I have seen this disease in connexion with purulent deposits in the neck and posterior mediastinum, but it may occur without the formation of purulent matter in any situation other than the lung; it may supervene in the advanced periods of the case, and at a time when the patient seems about to recover; or it may come on much earlier, and when the skin is thickly covered with the petechial eruption. The last case is the most formidable; though one attended with the greatest danger, the disease is, however, not always fatal; and we have had several cases in which recovery took place. I need not say, that they were all treated upon a tonic and stimulating plan, in addition to which we employed dry cupping and blisters.

The last case of which I shall speak is by far the most formidable of the pulmonary affections of typhus; it is characterised by the sudden, complete, and singularly extensive consolidation of the lung. In the course of twenty-four, or even, sometimes, of twelve hours, the most extensive and complete dulness may be produced in a lung which had been previously free from physical signs, or, at most, had only exhibited some of the ordinary bronchial râles of typhus. We have thus the signs of complete hepatisation, unpreceded by the crepitating râle; the disease begins by consolidation, and then one of two results follows: either the patient dies speedily, generally with loose râles in the opposite lung, combined with tracheal effusion; or, after a day or two, he begins to expectorate a horribly fetid matter, and we discover by the stethoscope that a large cavity has formed in the lung. This is a true gangrenous cavity formed in the very centre of the solidified mass, and the disease has a close pathological analogy to the process of acute mortification which has been described as occurring in some of the worst cases of the typhous disease of the intestinal glands.

Let us now pass in review the circumstances in which these forms of disease occur; for, when we compare them with the ordinary conditions of acute primary pneumonia, we cannot but admit that they indicate a lesion of a very different nature.

In the first place, the physical signs are preceded by fever; and it may not be until many days have elapsed that the symptoms of lung affection, as it were, spontaneously arise. 2ndly, the fever is obviously an essential fever; it may occur with or without petechiæ, and other complications may or may not be present. 3rdly, the disease sets in without any apparent external exciting cause. 4thly, when the purulent form is observed, it appears to be, as it were, the second stage of the affection; and I may here remark, that, on dissection, we rarely, if ever, find what is termed concrete or non-concrete purulent matter. Lastly, the invasion of one form of the affection may be sudden, and the signs of extensive and complete consolidation be among its earliest phenomena. It is in this case too, that, if time be allowed, large eschars forming cavities, which may communicate with the bronchial tubes, are liable to occur.

I may remark here, that in two cases of this rapid consolidation, the gangrenous eschar did not communicate with the bronchial tubes. One was a case of typhus, in a man who had long before suffered from gangrene of the opposite lung; the other occurred in a case of what is termed the erysipelatous or diffuse inflammation.

[In another lecture Dr. Stokes gives us some very interesting comments on the case of a butcher attacked by fever. He considers that many of these butchers are very liable to attacks of typhus in various forms and complications, such as pneumonia and arthritis: their systems are too highly animalized. This patient had an attack of epistaxis.]

Now, what may we suppose is the cause of epistaxis in this case? It is extremely difficult to explain it; but as far as we know, it does not seem traceable to the causes which ordinarily induce this form of hemorrhage. We do not find it

attended with symptoms of active determination of blood to the head, with throbbing of the arteries, and the other symptoms of ordinary active epistaxis; it seems rather to be connected with a dissolved or impoverished, or in some way diseased state of the blood. When we compare typhus fever with that form which gets the name of typhoid fever, with respect to the frequency of this symptom, we find it is much more common in the latter than in the former disease; epistaxis is a common symptom in the non-maculated fevers, while it is rather, I would say, a rare symptom in the petechial typhus. You would not expect this from any *à priori* reasoning, but the fact is so; and there are other facts connected with these two forms of fever which appear to show, that in the typhoid fever there is some condition of the blood more analogous to what occurs in ordinary cases of anæmia, or in chlorosis, than in the true typhus fever. We shall see by and by, that with respect to the development of murmur of the heart, it is rare in the true typhus fever; while we have seen it over and over again in the typhoid, non-petechial, and the relapsing fevers. Other arguments might be brought forward to prove this; but, when we speak of diseased states of the blood, we are speaking on very uncertain grounds. There are two facts, at all events, which are of importance—one, that in the true petechial typhus epistaxis is rare, while it is not uncommon in the other forms of febrile disease; the other, that cardiac murmur is rare in the true petechial typhus, and not uncommon in convalescence from the typhoid disease. If you take two reports on fever, one from a British Hospital—an Hospital in London, in Scotland, or one in Ireland,—take for example Dr. Cheyne's reports, or any of our other extensive reports on fever, and compare it with a report on fever from a Parisian Hospital, you will see how much more frequent epistaxis is in the Continental fever than in ours.

All of you that have been working so diligently in the fever wards must have observed, that since the session began, petechial fever, in its common acceptation, has been a very rare thing indeed with us; almost all our cases have been of a non-petechial character. This is a very interesting point, and the question for us to consider here is, whether this patient has the epidemic of the day, of which epistaxis is a not unusual symptom, or whether, in this case, the hemorrhage may be traced to a morbid condition of the blood, such as occurs in purpura; or whether he has a pyogenic fever; and these are important considerations; or whether, again, he may not have them all mixed up together; and here we perceive one of the greatest advantages of clinical study in the actual observation of disease at the bed-side, as distinguished from the study of disease in books, viz., that in studying from books we are generally led to consider diseases in isolated forms, pictures of particular affections; but, in an Hospital, we find, that in actual and especially in acute affections, isolation is the exception, and combination is the rule; and these combinations of diseases are infinite. This is the reason why a student of six months' or twelve months' standing, who has worked diligently in a clinical ward, is, I believe, in many instances, a better practitioner than the most learned man, who has drawn his knowledge exclusively from reading, however extensive. Do not suppose from this that I want to decry the study of medical works. Far from it; you must both read and observe; but it is in the clinical ward that you will learn what has not yet been taught, but which must be taught—which has not, at least, yet been taught properly in works of practical medicine; I mean the combinations and infinite complications of diseases, both local and general, and local with general.

I was observing just now, in the ward, in the case of the boy who had the pulmonary lesion, (I will not call it pneumonia of the lung,) with a low typhoid fever, (he is under the care of Mr. Daly,) how well this case illustrates the advantage of clinical study. If you take works upon disease of the heart, you find that it is assumed by almost every writer, that the first sound of the heart and the second sound of the heart are to be easily distinguished from each other. There are some persons who, if you were to say to them, in any given case, "I think that I have had considerable difficulty in saying which was the first and which the second sound of the heart," would set you down as very deficient indeed, as one that had not been properly taught, and did not know his business. But the fact is that there are many cases in which at first it is very difficult indeed to say which is the first and which the second sound of the heart. There are cases in which the most experienced man will require repeated observation before he can make up his mind on the point.

It has happened to me over and over again, that, after I thought I had made up my mind by examining at one part of the heart, when I changed the stethoscope an inch or two, I was again thrown into doubt.

I mention this to show you how diffident we should be in our opinions upon these subjects, how slow we should be to condemn men because they do not come up to the mark laid down in books. The truth, in fact, is, that they go beyond it—that they are wiser than the authors of such books.

There are two cases in which it is often extremely difficult to say which is the first and which the second sound of the heart. One of these is, that triple combination which is so common, especially in private practice, where the patient has chronic bronchitis, a weak and irregular heart, and congestion and enlargement of the liver. This is a very common triple combination, if you speak merely of local diseases. But there is another element very commonly to be found in it, and that is, the gouty element; so that you may have a gouty man with chronic bronchitis, with a weak and irregular heart, and with an enlarged liver. In such a case, it is sometimes extremely difficult to say which is the first and which the second sound of the heart. The two sounds are closely similar one to the other; and the action is so irregular, so uncertain, that you may be often for minutes together with the stethoscope carefully applied, and yet not be able to make up your mind. That is one case. Well, take another—such as that of the boy above stairs.

This boy presents some very curious phenomena; and he illustrates, again, difficulties which you would not anticipate, if you merely depended upon the text-books for a diagnosis of disease of the heart. There are two difficulties here. It is difficult to say whether the murmur which he has belongs to the first or to the second sound. But there is a greater and a still more important difficulty in this case, viz., to determine whether this is an organic murmur or an anæmic murmur; and I am not ashamed to say that my own mind is not made up on the subject. In short, I could not take upon myself to say which it is. It would be very easy to adopt one theory or the other, and to argue in a very specious manner upon it; but I know thoroughly the difficulties of the subject; and I declare to you, that I think, at this moment, it would be hardly possible to say whether this boy has disease of the valves of his heart or not. There is one consideration connected with the case which is drawn, not from physical examination at all, but from the general history of the patient, and it is this, that, while organic murmurs are rare—very rare in the form of disease which he has had—inorganic murmurs are comparatively common.

This is a very strong point. We are here under this difficulty, which you meet with every day in private practice—that you are called on to give an opinion when the data that should guide you in that opinion are deficient. We want to know the previous history of this boy. If, instead of being in Hospital, this boy were a patient in private practice, and had been under your care, and you had been the attendant on his family for years together, and were familiar with him, and intimate with the state of his heart, you would be able to say, first, if he ever had carditis; next, whether, before his late attack, he had murmur in his heart or not; but we know nothing of all this; and the only fact we have to go on is the observation of Mr. Daly, that, when the boy was first examined, this murmur was not there at all. I myself have no doubt as to the correctness of this observation of Mr. Daly; my own opinion is, that, whether the murmur be organic or inorganic, it has been developed since the patient came into the house. Can we distinguish by acoustic signs alone,—and this is a point which bears on the subject of fever in a most important manner,—the inorganic from the organic murmur? The answer to that question is, simply,—that in the present state of our knowledge, there are many cases in which we cannot do so; that there is no special acoustic character by which you can distinguish one of these phenomena from the other. This looks like a depreciating statement, as far as diagnosis is concerned; but the cause of diagnosis would be much more injured by attributing to it powers which it does not possess, than by confessing its deficiencies. The diagnosis, in the case in question, is to be drawn from other circumstances,—generally speaking, from circumstances connected with the condition of the patient, the absence of the signs of inflammation, and a variety of other points, which we will consider more in detail on another occasion.

To come to the murmur in fever, the observation has been made in a considerable number of cases, that valvular murmur, when the patient is made to sit up, does not

disappear; but we have found in this Hospital that, in many cases in which a murmur was observed after fever, it was ascertained, that when the patient was placed in an upright position, the abnormal sound disappeared, or, if it did not disappear altogether, it became greatly less intense; so that the disappearance of the murmur in the upright position is in favour of its inorganic nature, while its persistence or aggravation is in favour of its organic origin. So far so well. But, you will ask, is this rule absolute? That is a question that must be answered in the negative; for you will meet with cases of anæmic murmurs which are not influenced by position; and I believe there are, on the other hand, cases of organic murmurs which are influenced by position. There are, doubtless, some cases of organic murmurs in which, when the heart is made to act rapidly, the murmur either disappears, or becomes lost in the other cardiac sounds, so that you cannot distinguish it.

My own impression about the patient, whose case we are at present studying, is that the murmur is inorganic. I trust it is; but I would not say so positively. I say this, because the character of the sound, although it is very aggravated,—although it approaches very closely indeed to the inorganic murmur,—is similar to a kind of murmur which I believe we, in this Hospital, were the first to describe, that is, the true muscular murmur of the heart,—a sound produced simply by the contraction of the muscular fibres when they do not contract *per saltum*, when they contract vermicularly, as it were; it possesses more this character than the character of the true valvular murmur. There is another point connected with it which is of importance. If this murmur was valvular, it would imply a great deal of disease; a rough, rasping murmur in the situation of the aortic valve implies generally a great amount of disease, and commonly of chronic disease; and, under these circumstances, you may be prepared to expect that the patient will show other signs of disease of the heart. So that we here have a diagnosis drawn, as I often observed to you before, from that most important source, the want of accordance of the symptoms. There is here, supposing the case to be organic, a want of accordance of thesymptoms; for we should expect, that with this great amount of valvular disease, there would be signs of dilatation of the left ventricle; symptoms which commonly attend upon this form of disease of the heart. And here is a most curious fact, that even when this boy had one of his lungs almost entirely obstructed—a condition which often acts in developing latent disease of the heart,—even at that time the symptoms of heart suffering were not at all remarkable. So that there is here, to a great degree, this want of accordance in the symptoms; and this is against the opinion of the disease being organic, and in favour of the view that the murmur is of a functional nature. The great mistake, that was made,—I am happy to say that it is now going out very fast,—in connection with auscultation generally, was this, it was supposed that every disease had its special acoustic sign, and consequently the attention of students and physicians was directed to the study of those signs in a purely mechanical point of view,—merely to the observation of their acoustic characters.

There can be no doubt that it is of the greatest possible importance to study carefully everything connected with a diseased organ, both its physical and its vital phenomena; but what you have to learn specially is this, not so much how to detect the sign, or how to recognise it, as to know how to reason upon a particular sign when you have discovered it. It is here that the clinical student of long practice and experience has the greatest superiority over the mere reader. His mind is trained to reason upon the phenomena which he observes. Here we have a group of phenomena; and if we did not give ourselves the trouble of turning every possible point of the case over in our minds, we should come, I am sure, to a very imperfect and erroneous conclusion about it.

Bear this in mind always, that there is no pathognomic physical sign of any disease whatsoever. This cannot be too strongly stated; and I believe that we might go further, and say, that there is no combination of mere physical signs which, excluding the history and vital symptoms, can be justly considered to be pathognomic; at all events, if there be such a combination, it is one of extreme rarity indeed. We hear of certain murmurs being pathognomic signs of this and that disease of the heart,—of friction sounds being pathognomic of pleurisy—of crepitating râles being pathognomic of pneumonia—of amphoric sounds being pathognomic of effusion into the pleura. All this is wrong; it is based upon error; and you must expunge it

altogether from your minds, if you wish to be accomplished physicians, investigators of truth, and faithful observers of disease as it is found at the bedside.

[The following remarks of Dr. Stokes on the connexion between typhus and tubercle, are exceedingly interesting. He says,]

I suggested that tubercular matter might be considered as one of the secondary deposits of typhus; and that we are justified in believing that, under certain circumstances, there may, with the same original or essential disease, be a variety in the nature of its secondary deposits. You will remember, that we examined two cases of tubercle in connexion with typhus; in one, the deposit occurred at the height of the fever, at the time when all the characteristic phenomena were best developed; in another, there was an interval of apyrexia between the subsidence of the typhus state and the appearance of the tubercular disease. Some might suppose then, that there was no connexion between the second set of symptoms and the first; but is it not more probable, or, at least, is it not possible that the interval of apyrexia was analogous to the intermissions of an ague, and that the deposits in the second attack were still the result of the original disease; or, again, that the first attack had, as it were, prepared the entire organism for the elaboration of another form of deposit? I have before alluded to the fact, that, in the epidemic in which these accidents were most frequently observed, there was a strong tendency to relapse. And, from all these circumstances, I conclude, that the tubercular disease, in the second attack, may still be considered as the result of the typhus poison.

There are other points which this study of the connexion between typhus fever and tubercle helps to illustrate. One of the most important is, that we are led to look at tuberculosis under the aspect of an acute disease. Our views of it are, therefore, enlarged; for although what has been termed (though often improperly) acute phthisis has been described in books,—yet, in the minds of most men, the idea of chronicity is attached to tubercular disease. But we have seen, that a patient may die with typhus fever and its consequent tubercular deposit within fourteen days. And again,—we find, that in the case of the consequent tubercle, that is where the disease has exploded after an apyrexia period, the patient may die with tuberculisation of all his organs, and without suppuration of the tubercle; or he may actually have innumerable softened tubercles in the lung and die of the disease within a fortnight after the commencement of the new attack. Such a patient may be said to have died from acute tubercle; but the term acute phthisis is improper, for the symptoms of consumption are by no means necessarily present.

In speaking of the case in which we made the diagnosis of the formation of tubercular deposits during the course of a petechial fever, I believe that I forgot to mention one remarkable symptom, namely, the extraordinary sweating to which the patient was liable. He had several paroxysms of sweating in the course of each day, and to such an extent, that when we removed the bedclothes clouds of vapour rose from the surface.

It is of importance that you should not allow erroneous ideas as to the constant chronicity of these affections to take possession of your minds. Even cancer, in some of its forms, may be a singularly rapid disease. I have observed large cancerous tumours to form under the integuments within a very few days before death. They were filled with the soft encephaloid matter. In this case, however, the patient had long laboured under a vast cancerous deposit within the chest. But I well remember another case which still better illustrates the occurrence of cancer as an acute disease. The patient was under the care of my friend the late Dr. Little, of Sligo. The case is an example of the rapidity with which organs may become cancerous, such as few persons who have not studied medicine at the bedside could have any idea of. It was that of a lad aged about 18. He had long suffered from neglected ulcer of the leg. On admission into hospital, he had a vast corroding ulceration, engaging the whole of the leg from the knee downwards; and so extensive had been the destructive process, and so greatly had the system been exhausted by the suppuration and irritation of the enormous ulcer, that it was determined to remove the limb. For a certain time everything went on well, the wound healed kindly, and the patient not only recovered his appearance, but actually became fat. He was soon to be dismissed from the hospital, when he was attacked with pain of the right side, and symptoms of intense pulmonary irritation. The disease resisted all treat-

ment, and he died within a fortnight from the period of the attack of the pain of the side.

On dissection, the lungs were found almost completely filled with masses of the soft white cancer. These masses varied in size from that of a pea to that of a large walnut. I presented the parts to the Pathological Society. Now, all this singular formation, or at least a very large portion of it, must have occurred in the short time that elapsed between the invasion of the pain and his death; for no one could suppose that with such an amount of disease there could have been perfect freedom from cough or difficulty of breathing.

Before we leave this subject, let me present it to you in another point of view. In these cases we see tubercular disease occurring as a general affection, and not merely as a disease of the lung. In these instances the affection of the lung was simply the production in that organ of a pathological state which engaged other viscera often equally and simultaneously. Now, although we do not say that there is no difference except as to rapidity and extent of deposit between these cases and those of ordinary phthisis, yet it is useful to bear them in mind in all our dealings with that fell disease. The progress of medical opinion is every day tending to the doctrine, that to consider consumption, as the word is usually understood, as an original disease of the lungs is an error. Why it is that in so many cases the deposit appears to be first formed in the lungs, we do not know; nor why an attack of pulmonary irritation appears so often to be its exciting cause. Perhaps, as in the long silent periods of an intermittent fever, the disease, even though it be a pure neurosis, still exists; there may be conditions in which the system is ready for the production of organic disease for an indefinite time. This may be true, not only with respect to tubercular disease, but also in many other essential affections, including a variety of fevers. And this disposition, this silent state of the malady, may itself pass away, and none may ever know how near the individual was to the invasion of a dangerous or fatal disease. I do not allude to what is called the latent periods of fever, but to a condition in which the occurrence of a fever or other essential disease is a possible or even probable, but not a necessary consequence. Be this as it may, of one thing we may be sure, that the less we consider phthisis as a pure disease of the lung, the better we shall treat it, and the less, day by day, will it be ranked among the opprobria of medicine. If consumption be not, strictly speaking, a disease of the lungs, but a morbid state affecting the whole economy, you can understand one cause, at least, for the too frequent failures of the attempts to cure it. If the pulmonary lesion be but the second step in the morbid process, or if not only the lung, but other organs are engaged,—what are you to expect from adopting the absurd, and often cruel, treatment so commonly used in cases of the disease? The blisterings, the leechings, the setons, issues, and eruptions by tartar emetic ointment, which still disgrace the practice of medicine, will be soon unknown among our more enlightened brethren. You cannot conceive the amount of suffering inflicted, and of positive mischief done, by the adoption of the doctrine that phthisis is to be treated as a localized, original, and irritative disease of the lung. Ask a practical physician as to his experience of phthisis, and he will often tell you, that, among the few cases in which a recovery took place, there were certain instances in which the patient had, either from despair or some other cause, pursued a course very different from that which is so often advised. I have known myself several most remarkable instances in which a temporary, or even a permanent, cure had taken place, yet in which the patients had acted in every way contrary to rules. They had exposed themselves to all vicissitudes of weather; they had taken violent exercise, lived freely, and even drank hard. They recovered; and it is still a question whether this result was owing to the invigoration of the system by the return to their former habits, or to the escape from the enervating effects of warm rooms, relaxing climates, cough mixtures, sedatives, and slop diet: to say nothing of the barbarous issue or seton.

Let me give you an illustrative case. Some years ago I saw a gentleman, who came to town labouring under all the symptoms of well marked phthisis. The disease had been of some months' standing, and the patient was a perfect picture of consumption. He had a rapid pulse, hectic sweating, purulent expectoration, and all the usual physical signs of tubercular deposit, and of a cavity under the right clavicle. I may also state, that the history of the disease was in accordance, in all particulars, with this opinion. I saw this patient in consultation with a gentleman of

the highest station in the profession, and we both agreed that there was nothing to be done. This opinion was communicated to the patient's friends, and he was advised to return to the country. In about eighteen months afterwards a tall and healthy-looking man, weighing at least twelve stone, entered my study, with a very comical expression of countenance:—"You don't know me, doctor," he said. I apologised, pleading an inaptitude that belongs to me for recollecting faces. "I am," he said, "the person whom you and Dr. —— sent home to die last year. I am quite well, and I thought I would come and show myself to you." I examined him with great interest, and found every sign of disease had disappeared, except that there was a slight flattening under the clavicle. "Tell me," said I, "what you have been doing?" "Oh!" he replied, "I found out from the mistress what your opinion was, and I thought as I was to die I might as well enjoy myself while I lasted, and so I just went back to my old ways." "What was your system of living?" said I. "Nothing particular," he said, "I just took whatever was going." "Did you take wine?" "Not a drop," he replied; "but I had my glass of punch, as usual." "Did you ever take more than the one tumbler?" "Indeed I often did." "How many? Three or four?" "Aye, and more than that,—I seldom went to bed under seven!" "What was your exercise?" "Shooting," he said, "every day that I could go out." "And what kind of shooting?" "Oh, I would not give you a farthing for any shooting but the one!" "What is that?" "Duck shooting." "But you must have often wetted your feet?" "I was not very particular about the *feet*," says he, "for I had to stand up to my hips in the Shannon for four and five hours of a winter's day, following the birds."

So this patient spent his day standing in the river, and went to bed after drinking seven tumblers of punch every night; and if ever a man recoverered from phthisis he had done so when I saw him on that occasion. Suppose, now, that he had been confined to an equable temperature, and a regulated diet, and had been treated in all respects *secundum artem*, what would have been the result? Any of you can answer the question. In point of fact, this very treatment had been adopted during the first three months of his illness, and his recovery may be fairly attributed not so much to the duck-shooting and whiskey-punch, but to the general tonic and undepressing treatment which he adopted for himself, and which his system so much required to enable him to throw off the disease.

[Sometimes typhus, in its secondary form, attacks the windpipe, giving rise to what Rokitansky calls laryngo-typhus. It is in fact secondary typhus developed in the windpipe. Dr. Stokes says,]

In those cases which we have seen, the symptoms were, loss of voice, or a certain degree of hoarseness; the cough very rarely, indeed, had the laryngeal character, and I do not remember any instance of stridor in connexion with the other laryngeal symptoms. In some cases, the weakness or hoarseness of voice continued for a considerable time, and did not disappear until convalescence was far advanced. I have often thought, that, in these cases, the lesion of voice was to be attributed more to the weakness or paralysis of the laryngeal muscles than to any form of irritation or inflammation of the mucous surface. For, although we cannot bring any observations from dissection to throw light on this point, we may fairly believe that the laryngeal muscles are liable to be affected in typhus, just as the muscular fibres of the heart are found to be, and that the same process which causes a typhous deposit in the mucous surface may be repeated in the vocal muscles; and that a certain time must elapse before these organs recover their healthy condition. They may also be weakened quite independently of any structural change, just as we see to occur in the heart; for in this latter organ there are doubtless two forms of debility in connexion with typhus fever,—in the one we have weakness with distinct softening of structure; in the other, a debility which appears to be purely nervous. This leads me further to draw your attention to the probable existence of similar conditions in the circular, and perhaps, also, in the longitudinal fibres of the bronchial tubes in the secondary disease of typhus. I have little doubt that such a condition exists in many instances, and that a weakness, with or without softening of these structures, becomes an important element in the bronchial disease of fever. If, as some modern authorities have lately urged, these circular fibres are really the expectorating muscles, we can readily see how any weakness or paralysis affecting them would greatly increase the danger of a patient in fever, already suffering under

copious secretion into the bronchial tubes. We can further understand, not only how this condition would superinduce what is termed effusion into the chest in fever, but also why it is that in the treatment of the bronchial disease of typhus there is such danger from the employment of the antiphlogistic method, and how, on the other hand, such admirable results follow from the bold use of tonics and stimulants. I have before drawn your attention to the fact, that in many of our most remarkable examples of typhous softening of the heart, there was a great amount of secondary bronchial disease; and in such cases you will constantly see the associated diseases of the heart and the lung, progressing or retrograding simultaneously, the treatment adapted to the one being also adapted to the other. In the heart, so far as we know, we have only to deal with the affection of the muscular structure; in the lung we have at least two different forms of anatomical structure affected—the muscular and the mucous tissues. But the existence of the essential typhous state so far assimilates them that the vital condition of both is depressed in a somewhat similar manner; and it happens, that whatever is tonic and stimulant to the one is equally so to the other, and that to support and augment the vital energy of both structures, is the only means by which we can add the assistance of art to the efforts of nature in throwing off the disease.

[Sometimes typhus, in its secondary attacks, gives rise to *deafness*.]

Now, although I cannot give you any anatomical observations upon this point, yet I think I am justified in suggesting to you, that this symptom may proceed from a condition which is one of the secondary affections of fever, and that it is either a simple alteration of the power of the auditory muscles, or that this lesion is combined with structural softening, or, again, that it is a combination of a muscular with a mucous disease, both under the influence of the typhous state. Of one thing, at all events, we may be certain, that this symptom in typhus is not to be taken as a sign of inflammation either of the brain or its membrane, or of the ear itself. Like the other secondary affections of typhus, it supervenes in the course of the disease, spontaneously and silently. I have used this phrase before, and you understand what I mean by it. It usually comes on in persons who have had no predominance of nervous symptoms, in patients in whom we have had no occasion to direct attention to the head; it is rarely, if ever, preceded by, or accompanied by pain in the ear; it almost always affects both ears, though, in some cases, one organ is more affected than the other; it subsides without treatment, and I do not remember a case in which otorrhœa occurred as a symptom of the affection. You may meet cases of purulent discharges from the ears, especially in young persons of a strumous habit, who have been attacked with ear-ache during their imperfect convalescence; but such cases have no relation to the true typhous deafness with which we are now occupied. As I have said before, I have only clinical observation to bring to bear on this matter; we have no dissections to throw light upon it. One of the traditional prognostics in our fever hospitals is, that this deafness may be regarded as a favourable sign. However this may be, we have rarely, if ever, seen a case in which it occurred prove fatal. A French author has lately published a memoir on the subject of deafness in connexion with the fever of Paris, and he refers to the results of several dissections to show that it is a symptom of internal otitis. We can only say, that, whatever may be the case in the typhoid fevers of the continent, such a view is quite inconsistent with our experience; and, even though we have no dissections to guide us, we may yet safely believe, that the deafness which occurs in the typhus of this country, is no proof of the existence of a local inflammation; and that in its rise, progress, and spontaneous subsidence, it is perfectly analogous to those varied affections which belong to the group of secondary effects of the one essential disease. We have, I repeat, only clinical observation to guide us here: but this is, in itself, a source of knowledge which cannot be too highly prized; one which, under favourable circumstances, gives us more trustworthy results than the most careful *post-mortem* examination. It is often more valuable to determine the state of the living organ, though we cannot see it, than to minutely scrutinize the dead parts. What would medicine be, if its great principles, maxims, and rules were founded solely upon what we see in the dead body.

You must not suppose that I undervalue pathological anatomy; but let me warn you against the error of considering it as a guide in medicine superior to the study of the living man, or, as I might say, the living disease. The study of vital con-

ditions is often found to excel *post-mortem* investigations in the evolution of useful results; and there is no class of cases in which the superiority comes out more clearly than in the essential diseases, and especially fevers; no matter whether we seek for a theory of the disease, or for rules of treatment. If we could infer the symptoms of fever from dissection of the bodies of its victims; if we could say that in this case there must have been one class of symptoms, and in another a different order and nature of symptoms; if we could, as it were, mentally re-construct the fever from its organic remains, then, indeed, what I have said would be an unsafe and hurtful doctrine. But it is not so; and the truth appears to be, whether we consider fever in its totality, or having reference to particular symptoms, that the worst fevers are those which produce the least organic change, and that when these changes are to be found, they are, as I have so often urged upon you, inconstant, unnecessary, and altogether incompetent to explain the nature or character of the disease.

Bear in mind, that medicine had attained a great value before pathological anatomy was cultivated. Its rise and progress were solely due to clinical observation; or, in other words, to the study of the vital conditions in disease, and the effects of remedies upon them. And admitting to the fullest extent the importance of the most minute study of the dead structures, especially when connected with the symptoms and history of disease, we must still, I believe, place pathological anatomy in the second rank, as a means of extending our medical knowledge.

On the other hand, to hold that pathological anatomy has not advanced our knowledge of fever would be an error. If it has not told us what fever is, it has shown us what it is not; and this alone is a great boon. But it has done much more. It has taught us to know the singular though varying phenomena of the secondary diseases; it has shown them in their rise, progress, and spontaneous retrocession; it has taught us the re-action of new local organic disease upon them, and also their influence in modifying the laws of periodicity. It has not, however, enabled us to say when we find an organ free from change—that it was not functionally affected during life; nor, again, that where we do find structural alteration—that the disease may not have been latent, that is, unattended by any special or distinctive symptoms.—*Med. Times and Gazette, Feb.* 3, 1855, *p.* 103; *Dec.* 23, 1854, *p.* 633; *Nov.* 11, *pp.* 486, 485.

2.—ON FEVER.

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[One of the most eminent men in Europe, Professor Virchow, of Wurzburg, has defined fever as follows: "Fever consists essentially in elevation of temperature, which must arise from an increased tissue change, and have its immediate cause in alterations of the nervous system." Dr. Parkes at once admits the first part of this definition as we all must do. The second clause gives rise to a most interesting lecture from Dr. Parkes. He asks, what proof is there that the preternatural heat of fever must arise from an increased tissue change? We cannot prove this simply from the loss of weight of the body. How is the increased tissue change to be proved? We cannot collect two of the excretions—the cutaneous and the pulmonary—so as to measure them accurately.]

The other two excretions—the urine and the intestinal discharges—can be measured with accuracy, and it is fortunate that the urine in particular is a valuable measure of the metamorphosis of tissue. The urea alone represents two-thirds of the whole quantity of nitrogen which passes off, the sulphuric acid (the sulphates of the food being accounted for) represents almost entirely the oxidation of sulphur, and the oxidized phosphorus of the body passes out, in great measure, though not altogether, as urinary phosphoric acid. Therefore a careful examination of the urine, and of the intestinal discharges, with an approximative estimate of the pulmonary and cutaneous excretions, give us sufficiently extensive and accurate materials for the question at issue.

At present, opinions are widely divided, as to the conditions of the excretions in

fevers. Some excellent observers hold that they are always, and of necessity, increased; others, no less exact, affirm that they are invariably, or almost always diminished.

If the pulmonary excretion—on which I have no personal observations—be excluded, I think I am in a position to prove that, in certain febrile cases, at any rate, some of the excretions are increased, in consequence merely of the febrile state, apart altogether from the specific disease which may be present; and I shall adduce, as briefly as possible, some examples of this:—The temperature of the body in these cases was judged of by measurement in the mouth of the axilla, and the normal heat of those parts is assumed to be 98° Fah. It is scarcely necessary to observe that, in speaking of the urine, I refer always to the amount of its solid constituents, and not the water.

Thus, a man, aged 26, had rheumatic fever. The temperature for several days averaged at least 3° Fah. above the healthy standard. During these days the patient sweated profusely; he excreted on an average in each twenty-four hours 100 more grains of solid matters in the urine than in health, and this increase was particularly owing to augmentation of the urea, and the sulphuric acids. The intestinal excretion was as copious as usual. The food taken during this time was extremely small in amount, and the man was evidently and rapidly losing weight. Here, then, there was evidently a very great increase of tissue change, with coincident abnormal temperature.

In another case of rheumatic fever, the temperature was also for some days 3° Fah. over the standard; there was profuse sweating; the urinary solids averaged in each twenty-four hours 200 grains more than in the state of health, the urea and the sulphuric acid being particularly increased; the intestinal excreta were not apparently diminished.

In this instance, if we consider that no food was taken, and yet that the urinary ingredients derived from the daily metamorphosis of tissue were nearly one-fourth more than in the healthy state, when some 20 ounces of food were daily taken, the extraordinary disintegration going on in some tissue or other is evident.

But this is not peculiar to rheumatic fever.

In a case of typhoid fever, without increased intestinal excretion, the temperature was several degrees higher than natural; the skin was moist; the respirations were rapid, though there was none of the usual bronchitis, and the urinary solids were augmented in each twenty-four hours by about 60 grains, on an average, over the healthy amount.

Here, then, very little food was taken, the patient, as in the other case, was quiet in bed; and yet the urinary and cutaneous excretions were greater than usual.

In a case of erysipelas of the head and face, with a temperature in the axilla of 1° over health, the skin was moist; the respirations were quick (without bronchitis); the intestinal excreta were unaltered; the urinary solids (and among these the sulphuric acid, the uric acid in a less degree, and probably the urea) were augmented in each twenty-four hours very considerably over the healthy amount.

I have selected these cases, and those afterwards to be mentioned, because there appear to have been no modifying circumstances. No medicines were taken to complicate the result, nor were there any profuse discharges from the stomach and bowels, which are difficult to collect and estimate, and yet which reduce the amount of the urinary constituents.

Two at least of the excretions (the urinary and cutaneous) were then increased in these cases, while the other excretions were not apparently diminished. In some other cases, only one excretion is thus augmented:—

A man, aged 21, was admitted on the eleventh day of a disease, which was diagnosed as febricula. There was no local disease whatever, and there was no evidence of typhus, typhoid, or relapsing fever. On the eighteenth day, all symptoms had disappeared. Now, from the twelfth to the seventeenth day inclusive, the average temperature of the mouth was 101·12° Fah.; and it fell gradually during this time from 102·25° to 99·5°. It averaged thus 3° over the normal amount. During this time, the bowels were confined; the respirations were not quickened (16—18 per minute), and the urinary solids were below the healthy standard. Thus, in each twenty-four hours during these six days, there were excreted by the kidneys only 476·416 grains; whereas the average of four days, when the man was perfectly

well, was 567·455 grains in each twenty-four hours, or about 90 grains more, although at that time the temperature was only 97·9° Fah. Thus, neither in the pulmonary, the intestinal, nor the urinary excretions, was there evidence of augmented chemical disintegration during the febrile period, but rather the reverse. But, then, the cutaneous excretion was evidently enormously augmented, for the sweating, day and night, was most profuse. It is not reasonable to suppose that, in this case, it so happened that there was really increased disintegration of some tissue or other; but, that the products formed were of such a kind, as to be eliminated rather by the skin than by the bowels or the kidneys?

Again, in a case of typhoid fever, there was an average temperature of 101·29° Fah., from the fifteenth to the twenty-fourth days of disease inclusive. The average daily excretion of urinary solids during this time, amounted to only 362·971 grains; while six weeks afterwards, when the patient was well, and the temperature was normal, the urinary solids, examined during six days, averaged 436·001 grains. The urinary excretion was then diminished; but, then there was constant and considerable diarrhœa, and also, at one time, sweating and sudamina, so that the increase in these two excretions may be reasonably held to account for the urinary diminution.

In support of this proposition, that the excretions may be increased in fever, I may allude to some observations by Dr. Alfred Vogel; although, unfortunately, they refer only to the elimination of urea, and are unaccompanied by any statement of the temperature. Thus, in a case of typhoid fever, with diarrhœa, fatal on the sixteenth day, there were excreted on the thirteenth and fourteenth days respectively, 740 and 889·5 grains of urea—a very great amount, considering there was also diarrhœa. In another case, on the ninth and tenth days there were excreted 818 grains on each day; while, in a third case, in twenty-four hours the excretion of urea reached the astonishing amount of 1065·5 grains,—or considerably more than twice the amount that would be excreted by a strong, healthy man, on exercise and abundant food. In this case also, there were two liquid motions daily; no observation is made about the skin. The average amount of urea in all the cases of typhoid fever examined by Alfred Vogel, is of course not so great as this; but even including cases of convalescence, during which time the urea is much smaller, the average excretion in twelve cases (thirty-five observations being altogether made) was 560 grains, or considerably above any healthy average, except that given by Professor Bischoff, which is a little less than this, but is decidedly too high a standard for health.

In a case of pyæmia (a boy 19 years old), Vogel found in twenty-four hours no less than 1235 grains of urea, or nearly three times as much as in health. In a case of extended pneumonia he found, during the period of hepatization, and at a time when the respirations were 70—80 per minute, and the pulse about 100, the excretion of urea, on an average of two days, to be 695 grains, or more than 200 grains over the standard.

Now, as the urea is the chief representative of the nitrogenous tissues, it is impossible not to see, in the enormous amount excreted, a very rapid disintegration of these tissues; although, in Vogel's observations, the condition of the other urinary ingredients (except the chloride of sodium), and of the skin, is disregarded.

Without referring to additional observations, though there are several others,—among which I would specially mention those made by Heller,—I think I may be permitted to conclude from the facts recorded in these six different febrile diseases, *that increase of temperature may be attended with increased elimination; and, therefore, presumably, with increased tissue change.*

But is this observation to be generalized? Does it hold good universally? Are the excretions increased in all cases of febrile heat? A most decided answer in the negative can be given. In many febrile cases there is diminution, instead of augmentation, of the excretions. I select the most striking examples of this which I have observed.

A man, aged 30, was affected with simple bronchitis of both lungs. The temperature averaged, during six days, 100·64° Fah.; the urinary solids, during the same time, amounted to only 294·174 grains in each twenty-four hours, or not one-half of the healthy amount. Afterwards, during convalescence, when the body had not recovered altogether its proper tone, the temperature of the mouth averaged, during four days, 97·56°, and the urinary solids amounted to 406 grains in each twenty-four

hours, or 112 grains more. Nor was the diminution of the urinary secretions in the febrile period compensated by increased elimination from the skin or bowels. There were indeed some sudamina on the skin, but there was much less sweating than during the convalescent period, when the urinary solids were increased one-fourth. With respect to the pulmonary secretion, it so happened that the respirations were not quickened (twenty per minute), and we may presume that the rule laid down by Hervier and Saint Sayer, that the carbonic acid is decreased in pulmonary inflammations, held good also for this case.

To take another example:—A man aged 48 was attacked with acute sthenic pneumonia, and came under treatment on the 9th day of the disease. The temperature for three days averaged 102·650; the urinary solids averaged during the same time only 369·17 grains, although, during convalescence, they rose to 589·45 grains; the skin was only slightly moist; the bowels were confined; the respirations averaged thirty per minute; the expectoration was scanty. Here, then, was a man whose temperature was nearly 5° over normal blood heat, and yet the urinary excretion was 200 grains less per diem than during the convalescent period, when it was still lower than in health. It is highly improbable, especially after the observations of Hervier, just referred to, that we are to look to the pulmonary excretion alone for the products of metamorphosis equal to 5° of heat.

I have other cases of pneumonia, showing the diminution of urinary ingredients, while the case quoted from Vogel, and others, which I could adduce from my own observation, prove that this diminution is neither constant, nor peculiar to pneumonia.

It so happens that these two cases are both pulmonary affections; but the same fact can be found in other cases.

This is an unequivocal case of typhoid fever in a man, aged 33, in which there happened to be no diarrhœa, no sweating, and no bronchitis. The urinary ingredients were for many days below the normal amount, though the elevation of temperature was considerable.

To take one more case:—A girl, aged 19, with acute rheumatism, and with a temperature averaging 102° Fah. in the mouth, passed for several days an extraordinarily small amount of urinary solids; there was no compensation for this in other ways, for she was breathing tranquilly, the bowels were quiet, and there was only inconsiderable perspiration.

In all these cases, it is of course to be understood that there was no reason to suspect, either during the illness or after it, any condition of the excreting organs which would have impeded their functions had the substances they ordinarily excrete passed through their capillaries.

The conclusion is irresistible, *that the products of metamorphosis, as judged of by the excreta, may be diminished in febrile cases*, in which the heat of the blood is intense; and yet, in no inconsiderable number of cases of the same disease, with temperatures no higher, the products of tissue change, the excreta are very greatly increased.

Since Nature knows no contradictions, how are these two classes of facts to be reconciled? Are the increased metamorphosis, and the increased heat, mere coincidences, and not really connected as cause and effect? All we know of the phenomena of normal heat forbids such a solution of the problem.

Other explanations are therefore called for, and of these there are several.

It is, in the first place, perfectly evident, that more chemical change may go on in the body than is represented in the excreta. The metamorphosis of blood, or of tissues, may not be carried to the point of forming those principles which can alone pass through the membranes of the eliminating organs. A vast amount of imperfectly oxidized compounds may be formed and retained in the system, circulating with the blood, or being thrown upon certain organs. We can, in fact, quite readily conceive increased metamorphosis with lessened elimination.

It is quite true, that direct analysis of the blood has not yet shown, in such cases, any actual retention and accumulation of the products of an imperfect oxidation; but there are some arguments in favour of such a view, which appear to me to have no mean weight.

Thus, it seems to me to be especially, though not solely, in these febrile cases with diminished excreta, that, at a later period of the disease, copious discharges

from one or other of the eliminating organs suddenly occur. Thus, in the case of pneumonia, with lessened excretion, to which I have referred, severe spontaneous diarrhœa came on. In another case of pneumonia, with similar diminution in the excretions, violent purging and sweating subsequently came on. In other cases, diuresis has occurred, and an increase of urea, of sulphuric acid, and probably of uric acid, has been poured out.

It seems a reasonable explanation of these sudden discharges (usually termed critical from being coincident with more or less sudden fall of temperature and improvement in the other symptoms) to suppose that a large amount of partially metamorphosed substances have been retained in the body, and at length have been brought to that point of oxidation or change which permits their elimination by one or other organ. Then, by their discharge, the system is suddenly freed from the noxious compounds which weighed upon it; and the metamorphosis having reached its acme, the temperature immediately falls. Considered in this light, the so-called critical discharges, and the lowered heat, are mere coincidences.

I do not mean to assert, that it is only in these cases, in which diminished excretion is found during the early period, that the so-called critical discharges occur. I believe they may then be generally expected; but, certainly, when the excreta are throughout abundant, such discharges may occur. We can suppose, then, that even the great amount of oxidation and elimination in such cases, does not suffice to get rid of all the material which has to undergo metamorphosis.

Another argument, which seems to me strongly in favour of the hypothesis of increased metamorphosis, with retention of the products, is this:—Secondary inflammatory affections appear to be more common in such cases, as if the blood were more contaminated. Sometimes, the connection between retention of excreta and local disease can be drawn closer; thus, sometimes in a patient, whose excreting organs are acting copiously, there occur a diminution of excretion, and a simultaneous and subsequent development of local disease.

Thus, in a case of rheumatic fever, the temperature of the mouth averaged, during three days, (fifth, sixth, and seventh of disease,) 100·37° Fah., the average excretion of urinary solids during the same time amounted to 1095·74 grains in each twenty-four hours, or at least 400 more than in health; and there was also profuse sweating. On the eighth day of the disease, the urinary solids suddenly fell to 493 grains in the twenty-four hours, and the sweating evidently lessened. There was no intestinal discharge to account for this. The temperature was only ·62 of a degree lower than before, and there was no alleviation in the general symptoms. Coincidently, however, with this lessened excretion, which affected all the ingredients in various degrees, a local inflammation (Angina faucium) became developed. On the following day, without change of treatment, the urinary solids augmented again, the angina lessened, and, in a day or two, disappeared. Does not the inference seem probable that, from some cause, there was, at a certain point, a sudden arrest of oxidation, and a consequent retention in the blood of hurtful substances which affected the nutrition and structure of a special tissue? It does not seem so reasonable to suppose that the angina faucium was the primary disease, and produced the diminution of excretion.

In a case of protracted typhoid fever, the urine in which was regularly analysed during no less than fifty days, the average excretion of urinary solids, from the twenty-ninth to the thirty-seventh days inclusive, was 422·348 grains. Diarrhœa, which had been profuse, had ceased. There had been great sweating, but this had much lessened. In spite of the late period of the disease, the temperature during these eight days averaged 103·4° Fah. On the three last of these days, (the thirty-fifth, thirty-sixth, and thirty-seventh,) the urinary solids gradually diminished. On the thirty-eighth day, pleurisy came on, and lasted for about five days. During this time, the temperature was almost the same, viz., 103·44° Fah.; but the urinary solids averaged only 344·153 grains, viz., 78 grains less than during the previous eight days, though there was no increased sweating or diarrhœa to account for the decline.

On reference to the different urinary ingredients, the diminution was found to be most marked in the urea, the sulphuric, and the phosphoric acids, and to be less marked in the water and the chloride of sodium.

To take a third case:—A man, almost convalescent from a subacute attack of

rheumatic fever, was passing a very considerable quantity of urine, which contained an excess of solids, and among these, of urea. The skin sweated moderately; the intestinal excreta were unchanged. Suddenly, without any very evident alteration in the cutaneous and the intestinal excretions, the urine decreased in amount both as far as the solid and fluid parts were concerned, to an extraordinary degree. Coincidently, there was a return of the joint affection, and on the next day there occurred a sharp attack of catching pain in the side, which may have been pleurisy, or perhaps pleurodynia, as friction sound could not be detected. Both the pleural or intercostal pain and joint affections rapidly disappeared, and coincidently, the amount of urinary excretion rose to its former standard.

These are the only cases in which I have been able to note the exact condition of the excretions in a febrile affection, at the very moment when a local disease appeared. I have not found any analogous fact in authors, but I think the precise nature of these observations forbids the supposition that the coincidence of the two phenomena was merely fortuitous.

The cause of this sudden arrest in excretion was not in any case apparent; but the effect was necessarily to cause disease of the blood by retaining in it substances which were really effete.

By the side of these cases, which show the retention of urinary ingredients, it is interesting to place the recent observation of Dr. Grohe, that in many effusions into the pleura and pericardium, in patients not labouring from Bright's disease, urea is discovered.

These various considerations appear to me to render the supposition most highly probable, that the diminished excreta in fever are to be referred to retention, but not to want of formation; that the excreta may be in fact small, though tissue change be great; and, if this be the case, Virchow's statement still holds good.

To account for the retention, many hypotheses present themselves: the fault may lie in a disproportion between the substances to be oxidized and the oxygen; a vast amount of albuminous substance, for example, may undergo partial change, and the oxygen, which can only reach to a certain amount, may be in insufficient quantity to convert all this changing substance into the principles of the excretions. Or the oxygen may be in some particular condition; for example, if it be true that in the body it is in an allotropic state, similar to or identical with ozone, and therefore in more powerfully oxidizing a condition, it may, in these cases, not have passed into that potent state, and therefore be incapable of causing so complete oxidation. Or, there may be a deficiency in the saline substances, without which oxidation is checked: or, finally, there may be some special condition of the nervous system, which modifies transudation through the capillaries of the excretory organs. It is evidently impossible, at present, to discuss any of these suppositions, for their solutions lie, at present, beyond the powers of chemistry.

Although we can thus account satisfactorily, it appears to me, for the diminution of excreta in fever, it may be useful to see what explanations could be given of the preternatural heat, if the diminished excretions had sufficed to prove diminished tissue change.

It might be argued, in the first place, that the preternatural heat may still be referred to chemical change; but that this chemical action may be peculiar, and may lead, not to the formation of urea or sulphuric acid, but to that of some other compound, small in quantity, and perhaps unrecognised in analysis. Thus, we know that the combinations of some elements—of hydrogen, for example—take place, with the evolution of extraordinary heat; and yet the resulting compound may be small in bulk. It is impossible to say whether this hypothesis has any, or how much truth. It is quite conceivably true, but it is at present supported by no facts.

Another explanation may however be advanced.

As in certain cases we have no evidence in the excretions of increased metamorphosis, and as the hypothesis of some unusual combinations of small bulk causing great heat is based on no facts, should we not abandon the strict chemical theory of animal heat? As in health some physiologists have looked for a second and recondite cause which produces some small degree of heat, unaccounted for, according to them, by chemical interchange, so in fever may it not be this particular cause which is especially perverted?

To this it seems sufficient to reply, that though the influence of the nervous

system on chemical action, and through it, on animal heat, be fully admitted, physiologists seem unwilling to give a *locus standi* to the hypothesis which throws aside chemical action altogether. And as the diminution of excreta is susceptible of explanation in a way more accordant with the phenomena of other cases, and with physiological tenets, it is clearly unnecessary to entertain an hypothesis which has neither a physiological nor pathological basis.

To recapitulate so far the results of the discussion:—Preternatural, like normal heat, arises from chemical action; which, in the former case, is augmented over the standard of the latter; this increased tissue change is evident from the loss of weight, and from the increase of one or other excretions; there are cases, however, and these are not uncommon, in which the excretions are diminished rather than increased; but this does not arise from lessened tissue change, but from retention of the products; the cause of the retention remains to be discovered, but principles, evidently effete, must circulate in the blood and organs; and it appears that it is in such cases that local diseases are most prone to occur; in such cases also, though not only in such, great discharges from the skin, bowels, or kidneys occur, and constitute critical discharges. Finally, it would seem probable that the sudden check to excretion, which may occur in the course of febrile diseases, is attended with the same result as retention of the excretions from the first, and local inflammations occur.

[Dr. Parkes next considers some of the chemical points connected with fever. He continues:]

On looking over the materials which I have had the opportunity of collecting, one fact is very evidently indicated, viz.: that the degree of febrile heat cannot be measured by the amount of the excretions, as a whole, or by any special ingredient. That is to say six patients, of pretty much the same weight and height, and labouring under the same or with different febrile diseases, and all having a temperature, we will say, of 102° Fah., will pass very different quantities of urea, uric acid, sulphuric acid, phosphoric acid, cutaneous and intestinal excretions. This is exactly what prevails in health, for, with an equal temperature, no two men pass exactly the same amount of excretory products. And besides this, in fever the excretions, as we have seen, do not represent exactly the amount of metamorphosis, for even with increased excretion there may also be for a time a collection in the system of substances which are really effete.

The relative amounts of different excretory products, such as urea, uric acid, sulphuric acid, phosphoric acid, &c., vary so remarkably in the same disease in different persons, that it is difficult to know whether or not their varying quantity is connected with the febrile state alone, or with the specific nature of that state. This, perhaps, arises from the fact that we can never know the exact amount of urea, or sulphuric acid, which is *formed*, but only that which is *discharged*, and also, as already stated, from the circumstance that the febrile metamorphosis is still in some measure regulated by the law, whatever that may be, which determines the very different amount of tissue change in healthy individuals.

On looking at the nature of the excretory products of fever as far as these are known, it is at once evident, that they indicate chiefly a destruction of the albuminous or nitrogenized tissues. Thus it is yet doubtful whether the carbonic acid emitted from the lungs, the representative of destroyed fat and starch, is ever increased; but it is quite certain that the urea, the uric acid, the sulphuric acid, and the urinary pigment, the chief representatives of the azotized structures, are often very greatly so.

It would be interesting to determine, if it were possible, where the increased destruction of the albuminous structures occurs, whether in the blood or in the organs. We know, most certainly, that, at the end of a febrile disease, the albumen, and the red corpuscles of the blood, are both lessened in amount. But this does not prove that the albumen has been destroyed at once in the blood; it may be that the rapidly disintegrating organs have simply taken more of it, while it has not been recruited by the addition of food. The red corpuscles, however, do certainly undergo a more rapid destruction in the blood itself, or in organs, as the spleen or liver. Of the various tissues, none appear to waste so fast as the muscles, especially the involuntary ones. The heart is well known to be often especially affected in some forms of fever, such as typhus; and of this a plausible explanation can, perhaps, be given.

Whether the liver, the spleen, the kidneys, and the lungs also waste, has not yet been determined; for several of the organs, and especially the spleen, are so affected by the specific disease, that it is difficult to know how far their structure is altered by the febrile affection, apart from the congestion or exudation which so frequently occurs in them.

Although the albuminous structures are thus especially destroyed, the fat of the body is also rapidly absorbed in fevers. It has not been shown, however, by accurate experiments, whether its disappearance is more rapid than would be the case in inanition alone.

I pass on now to a consideration of, perhaps, the most important chemical condition of the febrile body; a condition which has been long partially observed, but has never been regarded as being of such interest as it really appears to be. I refer to the remarkable retention of water in the febrile system.

That the water of the urine is lessened in an extraordinary degree, sometimes even to one-half or one-quarter of its bulk, in spite of the vast quantities of fluid which are taken to quench the insatiable thirst, is a long-known fact. Professor Vogel, of Giessen, has lately directed particular attention to it, and has found the diminution to occur at the very commencement of pyrexia—an observation which I can confirm by many observations. It has been referred by some simply to the increased perspiration which was supposed to coexist, but accurate inquiry shows that this explanation is untenable. The urinary water is equally diminished when the skin is dry, and is often scantiest when the skin is driest. It has seemed to me, that in many cases the concentration of the urine is almost as good an index of the amount of fever as the temperature itself.

In all fevers, however, a time comes, when, though the heat is still preternatural, the amount of urinary water increases to its normal amount, or passes beyond it. In the specific fevers this occurs generally, either first, when the force of the cause has, so to speak, partially spent itself, or when the substances on which it acted have been expended, and when convalescence is approaching; or, secondly, when certain products of the metamorphosis are forming. In rheumatic fever, the increase of urinary water is seldom seen before the joint affection has almost disappeared; in typhoid fever it occurs earlier, in mild cases about the 14th to the 18th or 20th day, although the febrile heat is still one or two degrees over the standard. If there be great thirst, as is often still the case in typhoid fever at this time, the amount of urine is sometimes excessive; its specific gravity is low, and its absolute acidity is less. In a case of relapsing fever I found the urine in small amount during the febrile period, and abundant at the period of remission, when the skin also was very moist. In pneumonia the quantity of urine seldom becomes large till the softening is far advanced, although sweating occurs earlier.

If we remember the large amount of fluid which is drunk in fever, the retention of the water will appear still more remarkable. But there is another order of facts equally interesting.

Great attention has been directed during the last few years to the amount of fluid which is poured into the alimentary canal from the various glands and membranes. It is now known that, in varying degrees, there is a constant transit of fluid from the blood into the alimentary canal, and as rapid reabsorption. The amount thus poured out and absorbed in twenty-four hours is almost incredible, and of itself constitutes a secondary or intermediate circulation never dreamt of by Harvey. The amount of gastric juice alone, passing into the stomach in a day, and then reabsorbed, amounted in the case lately examined by Grunewald, to nearly twenty-three imperial pints. If we put it at twelve pints we shall certainly be within the mark. The pancreas, according to Kroeger, furnishes twelve and a half pints in twenty-four hours, while the salivary glands pour out at least three pints in the same time. The amount of the bile is probably over two pints. The amount given out by the intestinal mucous membrane cannot be guessed at, but must be enormous. Altogether, the amount of fluid effused into the alimentary canal in twenty-four hours amounts to much more than the whole amount of blood in the body; in other words, every portion of the blood may, and possibly does pass, several times into the alimentary canal in twenty-four hours. The effect of this continual outpouring is supposed to be to aid metamorphosis; the same substance, more or less changed, seems to be thrown out and reabsorbed until it be adapted for the repair of tissue or become

effete. Some medicines thus pass from, and repass to, the blood many times in determinate parts of the canal before a combination occurs which facilitates their exit from the frame. The exact steps of this wonderful extravascular circulation are at present unknown, but its extent obliges us to consider it as one of the greatest importance.

Now, in fever, this intermediate circulation undergoes a great diminution, if not, in some cases, an almost total arrest. Thus, very early in fever the buccal mucous membrane becomes sickly, and the amount of saliva diminishes. So constant is this, that an old writer includes dryness of the mouth in his definition of fever. We well know, that a very early symptom of improvement is the moist tongue, caused by the return of salivary flow, and by the increased quantity of mucus. The decrease in the quantity of gastric juice is quite as certain. Independent of the fact that loss of appetite is constant in fever, we learn, from the experiments of Dr. Beaumont, that, when St. Martin was feverish, no flow of gastric juice could be excited in any way, although at other times even the mere introduction of the thermometer into the stomach caused copious secretion. The amount of pancreatic fluid in fever is quite unknown; but, as little food is taken, it cannot be so great as in health, and, judging from analogy, it is diminished like the saliva. The succus entericus, like the gastric juice, is probably diminished, for the stimulus of food is taken away; and we also know, that, in febrile affections (cases in which there is specific affection of the mucous membrane being excluded), there is constipation, and the fæces are dry. The resort to purgative medicines is, therefore, almost invariable.

We are, therefore, entitled to conclude that, as in the case of the urine and the sweat, the intermediate circulation in the alimentary canal is more or less diminished.

The consequences of such an abnormal state of things are yet obscure. If the object of this extravascular circulation be to prevent abnormal, and to aid normal metamorphosis, as is supposed by Schmidt, the most serious impediment to this must, of course, arise. And here we have one of the many anomalies of fever—we have, as coincident phenomena, increased tissue change, and lessened amount of that circulation which is supposed to aid metamorphosis.

It is easy to conceive, also, how the nutrition of the mucous membrane of the alimentary tract must suffer, when that transit, with which, of course, their anatomical structure is in unison, is thus impaired.

Are we to stop here, and suppose that the lessened transudation of water is confined to the skin, kidneys, and alimentary mucous membrane? or are we to suppose that the extravascular circulation is still more profoundly altered? In health, we know, that in every liquid, in every organ in the body, there is, from molecule to molecule, from cell to intercellular fluid, from fluid to cell, a continual and rapid movement of fluid, arising, not simply from transudation or endosmose, but by the agency of a chemical action on the surface of membranes, which repels and (attracts?) with an almost inconceivable power.

Without venturing to pass the cautious limit which the great chemist who has discovered the osmotic force and its laws has prescribed, we may yet well suppose that the chemical changes and the developement of heat are intimately connected with this molecular movement. Then at once comes the question, whether this vast circulation, to which the circulation discovered by Harvey is but the servant, is altered in fever? We can scarcely avoid concluding that it is so, when we observe so many evidences of altered metamorphosis, of altered heat, and of altered transudation, in those parts which are accessible to observation. And how can metamorphosis and animal heat be altered, without the process with which they are connected being also changed? But at present it is impossible to do more than presume, that as physics advance, and our means of research become more adequate to the task, we may be enabled to pass into these penetralia, whither, at present, no clue leads.

The retention of water in the system, to which I have referred as the most general chemical fact connected with fever, is at present incapable of explanation. Like the augmented metamorphosis of fever it seems to be connected with some unusual condition of the nervous system, and, even in so-called health, it has been shown by the elaborate researches of Beneke, that when the nervous system is depressed, and

a semifebrile state is produced, the flow of water by the kidneys is at once diminished. But at present it is impossible to determine the immediate physical cause of the retention. Is it to be looked for in some chemical condition of the vessels or the membranes of the excretory organs? If so, every excreting and secreting organ must alike suffer, and this is in accordance with facts. But then most certainly different organs suffer unequally, a phenomenon which can only be met by the further hypothesis, that different organs are unequally affected?

Another explanation may be suggested. It is now well known that during endosmosis different organic substances absorb or take very different quantities of water through membranes. For example, the endosmotic equivalent of sugar is a large amount of water, and in consequence of this physical law, sugar attracts water from the blood, and carries it from the system. Hence the unquenchable thirst of diabetes; for every structure is robbed of its water by the powerful attraction of the diabetic sugar. Is it possible that in fever some substance is produced which has as powerful an attraction for water as sugar, and the action of which may cause the universal thirst?

A fact mentioned to me by Mr. Graham, will put my meaning in a clearer light. Mr. Graham has discovered that gelatine has an extraordinary attraction for water, so that it will even take it from alcohol, and render alcohol almost, if not quite, anhydrous. This property, manifested at all temperatures, is particularly marked at the temperature of 98° to 100° Fah.

Albumen, on the other hand, has little attraction for water, and yields it up at once to alcohol.

Now supposing that, in the rapid metamorphosis of albuminous substances in fever, gelatinous compounds, or something approaching to them, were formed,—and this is by no means unlikely,—then, as a consequence of a physical law, the gelatine would at once take water from the albuminous tissues and would necessarily give rise to intense thirst. Then, unlike sugar, the gelatinous substance would not be discharged, but must be converted into urea and uric acid, as ordinary gelatine is when it is taken as food.

I mention this hypothesis merely as an example of how water might be retained in the febrile body. At present, of course we do not know whether any such compound is or is not formed, for the transition steps, and they may be numerous, between organised albumen and urea, are not yet known.

At any rate, whatever be its antecedent phenomena, the retention of water in the febrile system is one of the most remarkable of its chemical phenomena.

There seems some reason to think that besides water, chloride of sodium is to a certain extent also retained in fever, or, at any rate, that it passes off less readily than usual with the urine. I say some reason, only, because this point cannot be said to be at present properly worked out. In all fevers, the amount of chloride of sodium in the urine rapidly falls. This is owing partly to the withdrawal of food; butit cannot be altogether and always so owing, because I have found the chlorides absent almost, or altogether, in several febrile diseases, in some cases of typhoid fever, of rheumatic fever, of erysipelas, of bronchitis, and have seen them return again when the fever lessened, before food was again given. In these cases the chlorides did not pass off by the intestines or the lungs, and in the special cases of typhoid fever and erysipelas there was no sweating, so that they did not escape from the skin.

In the paper by Alfred Vogel, to which I have formerly referred, the chlorides were found to be always diminished. This is ascribed, in the history of the cases, to deprivation of food; but when Vogel sums up his conclusions, he seems almost disposed to take a different view, and says that, allowing for the food, the chlorides seem to be rather diminished than increased.

Passing now from these general statements, I ought to consider in proper order, the chemistry of the excretions, of the secretions, of the blood and of the organs.

But it would be a mere pretence of knowledge, were I to assert that anything satisfactory is known of the chemistry of the secretions or of organs. I pass on then to a consideration of the excretions, and the blood.

I. *The excretions.*—The general characters of the urine proper to the febrile state have been already incidentally referred to, and need be merely recapitulated; they are, deficiency of water, increase of solids, if there be no retention; among the solids, the urea, uric, hippuric, sulphuric, and phosphoric acids, and the pigments are

increased; the condition of the oxalic acid is uncertain; the chloride of sodium is diminished; the condition of the creatine and creatinite is not known; unusual products, as leucin or albumen, or modifications of it may be present, but are not essential.

There are only two additional points to which it is necessary to refer.

The deep colour of febrile urine has usually been attributed to its concentration, but it is now certain that this is only a partial explanation; if febrile urine be diluted to the usual amount of healthy urine, it is still darker than usual. Professor Vogel has lately shown that the colouring matter is increased sometimes four-fold. This dark pigment contains apparently more carbon than usual; some portion of it rapidly takes oxygen when exposed to the air, and forms lactic acid, and when the urates fall, which they soon do, in consequence of the greater concentration and acidity of the urine, as well as, in some cases, from their own greater amount, this colouring matter falls with, and variously paints them. When these deposits are washed with alcohol, they yield some portion of the colouring matter to it, and this does not give any of the reactions of bile pigment. Indigo can, I believe, be formed from it, as it is from common pigment, according to Dr. Hassall's interesting discovery.

The importance of the pigment consists in the fact, that it may apparently be considered as a measure of the metamorphosis of the blood globules, and from its amount, we are led to conclude that, in some cases of fever, the disintegration of the blood particles may be four times as rapid as in health.

The other fact connected with the urine is the augmentation of the free acidity as measured by neutralization with soda. Unfortunately, at present, it is not absolutely known whether the excess of acidity is present when the urine is passed, or whether it is caused by subsequent acid fermentation of the pigment. But, considering the increase in the uric, hippuric, sulphuric, and phosphoric acids, it is probable that it is in excess before any changes occur in the urine from exposure. The causes of the increase remain, of course, to be investigated when the fact is certain.

II. The exact chemical constitution of the cutaneous, and of the intestinal excretions, is yet unknown. In certain specific fevers, indeed, both excretions have been partially studied; but to this subject I shall, of course, not refer.

The pulmonary excretion has been very imperfectly examined. In all exquisite inflammations, except those of the lungs and heart, Hervier and St. Sager found the carbonic acid augmented. Lehman, however, criticises these observations, and, apparently on good grounds, denies their accuracy. In typhus, it seems certain, from the observations of Malcolm, that the carbonic acid is very greatly diminished. In a case of pleurisy, lately examined by Gorup-Besanez, it appeared to be increased.

III. *The blood.*—The composition of the febrile blood has attracted the greatest attention and interest; but, owing to the extreme difficulty of separating its constituents, its chemistry, in febrile affections, as in almost all other diseases, has yet really to be investigated. The observations of Andral and Gavarret, Simon, Becquerel and Rodier, Denis, and others, valuable as they are in many respects, belong really to the very infancy of science, and are utterly incapable of solving the mysteries of a fluid like the blood, in which are found the most complex combinations of the most complex principles, and in which circulate materials both for nutrition and for elimination. The blood is, indeed, like the sea, which feeds the clouds, and receives back the rivers laden with the impurities of the earth. The venous system especially, from the blood of which analyses are generally made, is the great sewer of the body, into which all organs drain their disintegrating effete structures. How can the analysis of such a fluid represent the composition of the blood at large; and how is it possible in the present state of chemistry, to attempt such an analysis with any success?

There are, however, some facts of interest. It would certainly appear from the observations of Cahen, who has, we are told, made more than 300 experiments, that the alkalinity of the serum of the blood is diminished considerably, by about one-third, in inflammations. And, by the side of this statement we may place the analyses of Becquerel and Rodier, which show in inflammation a diminution of the alkaline salts.

Cahen states that the lessened alkalinity does not occur in typhoid fever, and it has even been asserted that the alkaline salts, and therefore, presumably, the alkali-

nity, are increased in this disease. Cahen appears to found his statement, that the alkalinity is not decreased in typhoid, on a single case; I have a single case to put in opposition to this, in which by neutralization with acid the alkalinity of the serum was found to be decidedly lessened. The statement that the alkaline salts are increased in typhoid is quite erroneous, they are slightly decreased.

Whatever be the case in typhoid fever the alkalinity of the serum is lessened in rheumatic fever during life; and Professor Vogel has noticed also acidity of the serum after death, in a case of rheumatic fever, in which death occurred from another cause. From observations after death the serum appears to be sometimes even acid in pyæmia and puerperal fever. In these cases, however, the acidity may have been produced by post-mortem changes.

The diminution of the alkalinity runs parallel with the chemical fact, that the alkaline salts and the chloride of sodium are diminished in the venous blood in inflammation, in rheumatic fever, in typhus fever, in typhoid fever, and, perhaps, in typhus.

Another established fact connected with the serum of the febrile affections is the diminution of the albumen after the fever has lasted some time, and the commensurate increase in the water of the serum. Whether the albumen is altered in quality or in composition is not known. The observations of Frerichs on the presence of leucin have been already sufficiently mentioned.

The augmentation of the fibrine in many diseases is one of the few well-ascertained facts in the chemistry of the blood. It is now well known not to be peculiar to, though most constant in, inflammations; it will occur in the specific fevers as in relapsing fevers; it is a constant condition of the blood in rheumatic fever, and I had an opportunity of showing, some years ago, that it may precede for several days the affection of the joints or heart, although Andral and Gavarret vainly sought after such a case. It is evidently, however, not necessary to the febrile condition, and therefore need not be discussed here.

The alterations of the red particles in febrile diseases are almost totally unknown. They certainly diminish in numbers during the course of the disease, but whether their individual constitution suffers change is not yet known. In certain specific fevers, the malarious for example, they may be destroyed without being eliminated, and form pigment cells and masses, as lately pointed out by Meckel and Planer.

It may appear disheartening to confess, that the results have been so incommensurate with the labour bestowed on the analysis of the blood. But on this subject the chemist's first words have scarcely yet been spoken, and we must wait almost for his last, before his conclusions are available for pathology. We must remember the intricate nature of the problem, and not too impatiently blame the inevitable tardiness of science. But a few years since, we should have thought it impossible to detect the wonderful difference in the salts of the serum and the red particles, or to count the myriad multitudes of these latter structures. Now, these marvels are almost among the simplicities of science. But a few years since the detection of uric acid in blood was looked upon as chimerical, and now, thanks to the ingenious process of an esteemed colleague, the merest tyro can do what the profoundest chemist formerly thought impossible.

We may be sure that chemistry has hidden powers yet to be revealed which will be competent to solve even this most difficult of tasks. Till it is so solved, however, the chemistry of the blood in pyrexia must remain what it is now, a chaos of uncertainty and contradictory assertions.—*Med. Times and Gazette, April* 7, 1855, *p.* 333.

3.—TREATMENT OF YELLOW FEVER BY TURPENTINE.

By James Laird, Esq., Surgeon, R.N.

From various documents which have been forwarded to us, we gather the following interesting facts respecting the remarkably successful treatment adopted during a recent outbreak of yellow fever on H.M.S. Medea. The disease broke out in the beginning of August, whilst cruising on the South Coast of Cuba, and continued to rage for upwards of a fortnight, during which a sixth part of the ship's company suffered from it. During this epidemic, and a subsequent relapse, about sixty cases

came under Mr. Laird's care, and of these but four died. The principal treatment pursued consisted in the exhibition of turpentine in small doses, as first recommended by Dr. Gilmore King. The supposed modus operandi of the remedy was by elimination, in virtue of its action on the skin and kidneys. The dose usually ordered was fifteen minims, with a little nitric ether and camphor mixture given every three or four hours, and continued until a remission was apparent. As soon as the remission occurred, quinine was resorted to, together with a more nutritious diet. As adjuvants, castor-oil, occasional doses of blue pill, evaporating lotions, sinapisms, cupping, blistering, &c., were employed. Regarding the further details of the treatment, &c., the following extracts of a letter from Mr. Laird will be the best explanation:

"With regard to blood-letting, I have to state that this remedial agent was had recourse to in a few of the cases, chiefly because the practice appeared to be of service during the Bermuda epidemic of 1843.

"On the important question, however, of bloodletting in yellow fever, I have been led from my late experience considerably to change my opinion, and although I took blood from the arm only to a moderate extent in nine instances out of more than sixty cases of yellow fever treated on board, I am now convinced, limited as my practice in this respect was, that venesection in this disease, however early and moderately practised, is a remedy extremely hazardous, and one which might, except in some particular cases, be safely dispensed with.

"Local bleeding, by means of cupping over the epigastrium or nape of the neck, as found requisite, will, I now believe, generally answer the purpose required.

"In respect to the therapeutic effects of turpentine in yellow fever, particularly its almost certain action on the skin and kidneys, I cannot speak too highly.

"Except in cases attended with much irritability of stomach, the draught I gave did not appear to cause nausea, and that annoying symptom, strangury, only occurred in six cases. In the last eleven cases put on the sick list, after leaving Port Royal for Halifax, I gave the turpentine in doses of ten instead of fifteen drops with equally good effect, and without the occurrence of strangury in a single instance of this number.

"In no case did this remedy appear to act as a stimulant by exciting the pulse, an objection which I have heard was made to its use by some of the medical officers who served during the Bermuda epidemic of last year; but this objection I believe to be erroneous, from repeated observations I made on the pulse fifteen and twenty minutes after the administration of the turpentine drught.

"When applied externally as was done in the case of a sailor-lad with black vomit and suppression of urine, who would not take the draught,—turpentine mixed with equal parts of olive oil, and rubbed in over the body and limbs, seemed to act even better than if it had been taken by the mouth. Three hours after the frictions were applied, this patient made a pint of healthy-looking urine, and, after an interval of eight hours, he passed a pint and a half more urine of the same character, at one micturition. Early next morning, black vomit, which had been present for about thirty-six hours, entirely ceased, and the lad appeared otherwise much improved. The frictions were continued at intervals, but no medicine whatever was given by the mouth. He made urine freely; the pulse had become stronger, and he appeared as if quite certain of recovery, when, after struggling violently to resist the administration of nutritive enema, to which was added ℥ xxv of tincture of opium, to secure its being retained, he was soon after seized with symptoms of congestion of the brain, under which he ultimately sank.

"On dissection no trace of black vomit could be detected, unless a little dark green matter found in the ileum might be considered as such. The mucous lining of the stomach was much congested, and discoloured dark red; but its epithelium was perfectly intact, and although the liver presented the usual fawn colour, the skin was not tinged yellow, as was the case with the other patients who died of black vomit. This case, I might be excused saying, ought to have been a recovery from black vomit, had not the unlooked-for head affection come on, with which I cannot but regret to think the opium might have had something to do, notwithstanding the small quantity administered.

"In another case of black vomit, I gave turpentine in doses of 40 drops, combined with medium doses of hydrocyanic acid, with the evident effect of controlling

this fatal symptom, although the patient was carried off soon afterwards, by profuse and uncontrollable hemorrhage of fluid-looking blood from the mouth and nose.—*Med. Times and Gazette, April* 14, 1855, *p.* 358.

4.—ON SOME POINTS IN THE TREATMENT OF GOUT AND RHEUMATISM.

By Dr. Fuller.

[The following is the abstract of a paper read before the Western Medical Society of London.]

Dr. Fuller commenced by stating, that in all diseases, but especially in those which, like gout, rheumatism, and rheumatic gout, are attributable to faulty assimilation, and the consequent formation of a morbid matter in the blood, the success of treatment must necessarily depend upon the care and judgment bestowed on four several points:—first, the history of the case; secondly, the appearance and general condition of the patient; thirdly, the character of the excretions; and fourthly, the selection and adaptation of proper remedies to meet the various exigencies of the case. He then proceeded to point out the bearing of each of these considerations upon the course of treatment to be pursued; and after drawing attention to the close relationship subsisting between the amount and character of the various excretions, and the condition of the system generally, he insisted on the necessity of providing for the full and perfect action of *all* the excretory organs. He instanced cases which had fallen under his care at St. George's Hospital, in which the long continuance of the disease was evidently referable to the sluggishness of some one particular organ; and others, in which the obstinacy of the symptoms was attributable to the imperfect action of *all* the excretory organs, rather than to extreme sluggishness in *any one*. These latter cases, when not attended by fever, and when characterized by affection of the muscular structures, are often benefited by bark and hydrochlorate of ammonia, which, under such circumstances, have the effect of promoting an increased flow of all excretions, and so of eliminating the materies morbi, and checking the disease. Dr. Fuller, however, dwelt strongly on the fact, that very different remedies are needed to regulate the various excretions under different conditions of the general health. He showed that when the system is low and deficient in vital energy, tonics—such as bark, quina, sarsaparilla, steel, and cod-liver oil, together with change of air, change of scene, freedom from anxiety, and the use of proper baths, and a due amount of exercise—form the most effectual means of checking the malassimilation on which the formation of the poison depends, and of increasing the action of the excretory organs, whereby its elimination is brought about, and that unless such remedies are exhibited, and the tone of the system improved before colchicum, guaiacum, and other so-called "specifics" are administered, no good results can be produced. He attributed the failure of treatment, in many instances, to want of care in tracing out the history of the case, whereby it happens that purely neuralgic pains, as well as the pains which follow the incautious use of mercury, and the pains and swellings resulting from a gonorrhœal taint, are confounded with gout and rheumatism, and are wrongly treated accordingly. He regards the too frequent habit of treating disease in accordance with its name rather than according to the condition of the patient, and the character of the secretions, as another cause of failure, inasmuch as it leads to the practice of prescribing each of the so-called specifics separately and apart from other remedies. He pointed out, that in most cases the action of *all* the excretory organs is disordered, and that whereas no single remedy will excite them all equally to action, the judicious practitioner should employ such a combination of remedies as will prove effectual for the purpose.

Dr. Fuller combated the opinion advanced by Dr. Garrod as to the non-existence of such a disorder as rheumatic gout, and avowed his belief that the three forms of disease which form the subject of this paper, are referable to different forms of malassimilation, and the consequent formation of different poisons in the blood; and he urged the remarkable differences observable in the age, sex, and physical condition of the sufferers from rheumatic gout, in the ascertained causes of the disease, the mode in which it makes its attack, its symptoms, its behaviour under treatment, and the pathological changes to which it gives rise, as evidence in favour of its being

a disease essentially distinct both from gout and rheumatism. He admitted the value of Dr. Garrod's uric acid test as a means of distinguishing, in many instances, between gout and rheumatism, as well as between true and rheumatic gout; but he did not admit the absence of uric acid from the blood as proof of the disease being true rheumatism. Moreover, admitting the value of the test theoretically, he denied its practical utility. Its only indicational value is in regard to the administration of colchicum, and even in this respect it cannot be trusted. In no cases is lithic acid more abundant in the blood than in those old cachectic cases of gout, accompanied by the formation of chalk-stones in the joints, and by granular degeneration of the kidneys; yet in none is colchicum of less avail, or more ill borne by the system. On the other hand, when the blood is free from lithic acid, as it usually is in true rheumatism, colchicum is often of great service, by acting as an evacuant, and modifying the secretions of the kidneys, liver, and bowels. Colchicum, therefore, should not be relied upon in all cases of gout, neither should it be discarded in all cases of rheumatism; and in the one, as in the other, the history of the case, the condition of the patient, the stage of the disease, and the character of the excretions, must be our guides to the administration of that as of other remedies.—*Lancet, Feb.* 3, 1855, p. 127.

Dr. Fuller, in his recent work upon rheumatism, regards the pathology of the disease as depending upon the presence of a "*materies morbi*," which he supposes to be an excess of lactic acid, owing to defective assimilation or suppressed perspiration. With the view to the neutralization and elimination of this acid, he recommends the free use of alkalies, by which means he "hopes the average duration of an attack may be reduced, from a month or six weeks, to ten days or a fortnight."

Dr. John B. Chapin states (*New York Medical Times*, August, 1854), that, during the attendance of Dr. Swett, at the New York Hospital, the present year, all the patients admitted with acute articular rheumatism, were put under the "alkaline treatment;" and he gives a statistical table of twenty-five cases treated by this method.

The salt chosen was the tartrate of soda and potassa, a neutral salt, possessing the property of rendering the urine alkaline.

"The plan of treatment usually pursued was, if the patient presented himself with unusual excitement of the skin and pulse, to administer a mixture of sulphate of magnesia and tartarised antimony, until the skin was relaxed, and the pulse reduced to a more natural standard. The Rochelle salt was then directed, in drachm doses, every two or three hours during the day-time, till the urine was rendered alkaline, when it was gradually suspended. A lotion of carb. potass. ℥j. with opium ℥ij. to the pint of water, was directed as an external application. The administration of the salt was not attended with disagreeable consequences, with the exception occasionally of some ulceration about the fauces; in no case was its action so severe upon the bowels as to require its entire suspension. The persons attacked were in the full vigour of health, and the character of the disease acute in its form. The frequency of administration of the remedy was governed very much by the reaction of the urine.

"On the admission of the patient, the urine was tested, and in all cases was found to be of acid reaction, and the secretion of the skin presented the usual acid odour. The treatment was generally commenced the second or third day after admission, and the urine was rendered of decided alkaline reaction in an average of five days after its commencement; the longest period it resisted the alkaline reaction having been twenty days, and the shortest two. The secretions of the skin have not, I believe, been noticed to alter. In one case, attended with profuse perspiration, which yielded readily to treatment, the coloured shirt the patient wore entirely lost its colour; and it was suggested whether the same change did not take place in the perspiration as in the urine. The average amount of the salt administered was from five to seven ounces.

"The average date of commencing improvement was seven days after commencement of treatment, coinciding, in the large majority of the cases, with the commencing alkalinity of the urine. The improvement was invariably permanent, and, after the urine was rendered alkaline, no new articulations were affected, as a general rule.

"The average period of convalescence was twelve days after admission; and the

whole duration of the disease, including the period previous to admission, was twenty-two days. Of thirty cases treated by Dr. Swett, during April and May, 1850, during which time no uniform course of treatment was pursued, the average duration was five and a half weeks. One of the most gratifying results of the alkaline treatment was the diminished frequency of cardiac complications. Twenty-one of the twenty-five were free from any complication; three were admitted with aortic obstruction, and one with mitral regurgitation. Not one patient was attacked with any heart complication during the treatment of the disease. Comparing this result with the practice last year, it was found that four had mitral regurgitation, six aortic complication, and three suffered from pericarditis; thirteen in all, out of thirty."—*Med. Times and Gazette, Dec.* 16, 1854, p. 621.

5.—REMARKS ON GOUT.

By Dr. A. B. Garrod, Physician to University College Hospital, &c.

[Dr. Garrod has arrived at the following conclusions on this subject, after making upwards of 200 examinations of the blood for the purpose:]

1. That in all cases of genuine acute gout—that is, in all patients where the great toe is specially affected, and where the other symptoms are such as to leave no possible doubt as to the diagnosis—uric acid exists in the blood in abnormal quantities.

2. That in all cases of acute rheumatism where the symptoms are characteristically marked, with or without heart affection, no abnormal amount of uric acid is present.

3. That in all acute or chronic forms of the same diseases—even when not so characteristically marked at the time of the examination—where the maladies can be distinctly traced to true gout or rheumatism, the same difference exists in the composition of the blood.

4. That the effused fluid, such as the serum, obtained from a blister applied to a non-influenced part, contains uric acid when the blood is abnormally loaded with this body.

The method for the most part made use of, and which is found to yield very trustworthy results, is very simple in its nature, and capable of being clinically applied, has been named the "Uric Acid Thread Experiment," consisting in the immersion of a single fibre or two of unwashed huckaback linen into a drachm or so of the serum of the blood, previously acidulated with a few drops of acetic acid, put into a flat glass dish, and placed aside, in order to allow spontaneous evaporation, when, if an abnormal amount of uric acid be present, the fibres will be covered with the crystals of this body. The blister serum may be treated in the same manner, using only a rather larger quantity—not less than two fluid drachms. Another mode of diagnosis found very valuable, is the searching for small deposits of urate of soda, or miniature chalk stones or tophi, which are frequently formed on the cartilages of the ears; sometimes, but less frequently, about the integuments around the eyes; sometimes on the palmar surface of the tips of the fingers. These concretions may, and do very often, exist in these parts, but especially in the ears, without being present at any other part; and as no deposit of urate of soda has ever been found in any patient without the blood containing an abnormal amount, and no case of genuine rheumatism was ever accompanied or followed by their formation, their presence may be regarded as an indisputable or pathognomonic sign of a gouty diathesis. Dr. Garrod detailed a case affording many points of interest. Gouty patients are frequently extremely susceptible to the influence of mercury, and, in the above case, a few grains of blue pill. Another point of great interest is, that this case shows clearly that even during the first four days of inflammatory action, urate of soda may be effused into the tissues, and indicates the importance of cutting short the inflammation, if possible, to prevent the mischief which may otherwise take place in the joints. This, however, must be done by relieving the system, and not by suppressing the local affection, or worse results will probably occur. In another case, there was, and had been in previous attacks, absence of the great toe affection; at least, the patient did not remember that that part had

ever been specially inflamed. Here was also the absence of any deposits in the ears or elsewhere, and it was solely from the examination of the blood that the author could assert that it was acute gout rather than acute rheumatism. However, in subsequent attacks, of which she has had two during the last two years, he has been enabled fully to test the accuracy of my first diagnosis. The blood has uniformly given an abnormal amount of uric acid crystals. The blister serum has contained the same.—*Med. Times and Gazette, Jan.* 13, 1855, *p.* 44.

Dr. Gairdner "cannot look on the disappearance of urea and uric acid in the urine, and their accumulation in the blood, as anything else than a very frequent symptom and consequence of gout, itself again being the cause of other important phenomena." It is venous congestion which he regards as the first condition essential to the formation of the gouty diathesis. We may select the following passage as presenting a summary of his views:—

"This hyperæmic condition of the viscera, and loaded state of the blood-vessels, is very unfavourable to secretion. The functions of the body are imperfectly performed under such oppression. The consequences are a diminished flow of bile and loaded bowels; suppression of the matters usually evacuated by the kidneys; the disappearance of the unctuous moisture which bedews the interstices of the fingers, toes, and arm-pits; and a rapid addition to the general plethora. The heart now feels the usual load, and by palpitation and struggle seeks to rid itself of the incumbrance; the veins become more turgid, the foot grows tender, the valves lose their office, and the capillaries must sustain unaided the whole weight of the superincumbent column of blood. If this state be not checked, a paroxysm of the disease soon occurs. This termination is sometimes promoted by standing on the affected limb, sometimes by the heat of the bed during sleep. The pain of the fit continues unabated till the swelling and œdema begin; in fact, till the strained and distended vessels have relieved themselves by the extravasation of their fluids. Patients even observe that the relief of the pain is coetaneous with the appearance of œdema, and the return of moisture to the skin; the swelling, so long as it is dry, hard, and elastic, affording no relief. At the same time, the solid matters of the urine begin to reappear in that secretion, and the bowels, which have hitherto required the strongest cathartics to move them, are now readily opened by the gentlest aperients; the flow of bile, in fact, is re-established. This, I believe, is the true *ratio signorum* of a fit of the gout."—*Monthly Journ. of Med., Dec.* 1854, *p.* 537.

6.—*Gout treated with Piper Methysticum.* By Dr. E. W. Pritchard, Hunmanby, Scarbro'.—[Dr. Pritchard's patient had tried the usual remedies for a severe attack of gout, but found no relief.]

Seeing it was a well-marked case of podagra regularis, and that my patient, though corpulent, showed signs of much debility from previous treatment, and moreover was exceedingly irritable, I ordered the limbs to be enveloped in piline, saturated with the tincture of piper methysticum, enjoined perfect tranquillity and freedom from business matters, and to take the tincture of piper methysticum every four hours in the following form:—

℞. Tinct. pip. meth. ʒ ss.; aquæ purse ʒ vj. Capiat quart. part. quâque horâ.

To drink lemonade, and to have for his dinner manna croup puddings; for his tea cocoa-nibs, boiled in milk, one half-pint.

On the 4th, found he had slept uninterruptedly since taking the fourth dose of his medicine. Did not see him till the evening, when he was feverish and anxious, chiefly on account of resting so long, declaring I had over-dosed him with opium. After a good deal of persuasion, I induced him to continue the treatment.

5th. He ate a hearty breakfast of fried bacon and toast, with boiled milk, and expressed himself much better; that he enjoyed his breakfast like he did months ago, before he was afflicted with this distressing malady. Constipation troubled him a good deal. I ordered the following pill:—℞. Pil. rhei. comp. gr. iv., cap. j., nocteque mane, and that he was not to mind his bowels acting at present, for the action of the piper is more marked when retained in the system. This was merely to calm him, for as a principle I do not [illegible]e aperients in gout.

7th. He expressed himself [illegible]other man, and from this time he went on improv-

ing, and continued under treatment about a fortnight; recovery was complete, and yesterday I received a letter from him, saying, I ought to publish the case, as he was quite well, and, though 65 years of age, as active and in good spirits as ever he expected to be.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 591.

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7. *Treatment of Acute Rheumatism.*—[Many of the London physicians now seem to treat rheumatism chiefly by the neutral salts, either by the acetate, tartrate, or nitrate of potash. Lemon-juice is still held in esteem by some, but by others it is used as an adjuvant without relying on it alone.]

Dr. Barlow, of Guy's Hospital, is accustomed to state to his clinique, that the most rapid recovery from acute rheumatism that he ever witnessed was under treatment by the acetate of potash. We may quote the following case as a fair example of the usual treatment pursued by that excellent practical physician, and also of its average results. The plan of treatment is, however, by no means peculiar to Dr. Barlow. H. T., a strong, robust man, aged 33, was admitted on the third day of his first attack of acute rheumatism. The disease was severe. Ordered to take a draught containing half a drachm of acetate of potash, ten grains of the nitrate of potash, and ten minims of the vinum opii, diluted with barley-water. For nine days he was kept on low diet, and on the tenth the improvement was so far advanced, that the decoction of bark was substituted for the barley-water in the prescription, and a better diet was allowed. On the fourteenth day he was convalescent about the ward, and marked for discharge in a few days.—*Med. Times and Gazette, March* 3, 1855, *p.* 213.

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8. *Acute Rheumatism treated by Bicarbonate of Potash.*—[Dr. GARROD, within the last three years, has had to treat, in University College Hospital, 51 cases of this disease.]

The main part of his plan of treatment consists in the administration, in a diluted form, of two-scruple doses of bicarbonate of potash, every two hours, day and night, until the patient has been free from all articular affection and febrile disturbance for two or three days, using local depletion over the heart's region, if any cardiac disease is present or threatened. The author detailed three cases of rheumatic fever, illustrating this mode of treatment; the first, a girl, ten years old, in which the duration under treatment was five days, the total duration eight; the second, a young man, aged twenty, with a complication of heart disease, where the duration under treatment was eight, the total duration fifteen days; the third, a young woman, aged eighteen years, in the fifth attack, the former ones having always lasted for a month or five weeks, but which, by the adoption of his plan, yielded in nine days, total duration being but thirteen days, four having elapsed before her admission into the hospital. He afterwards gave a table of fifty-one cases of acute rheumatism; and of each patient the following particulars are noted:—The age, occupation, hereditary predisposition; the number and causes of attack; the symptoms before admission; the symptoms during treatment; the nature of treatment; and the duration of the disease. From these cases, the following deductions are made —viz., that in twenty males the duration of the disease under treatment averaged between six and seven days, and the total duration between eleven and twelve days; and in thirty-one females, the disease under treatment averaged from seven to eight days, and the total duration between fifteen and sixteen days—giving in all an average under treatment of seven days and a half; and, for the total duration, about thirteen days and a half. The author then alluded to the influence of the bicarbonate of potash when administered in large and frequent doses upon the different organs and functions of the body; and remarked, that it produces neither nausea, vomiting, nor purging—in fact, no symptom of gastro-intestinal irritation. It now induces a strongly alkaline condition of the urine, causes it to effervesce freely, with excess of acid, but does not appear to promote an increase in the quantity of the secretion. It appears to render the secretion of the skin less acid—sometimes almost neutral. That it acts as a powerful controller of the heart's action, reducing greatly the frequency of the pulse, but without causing the faintness often produced

by digitalis, colchicum, &c. That it probably increases the alkalinity of the serum of the blood, and diminishes the coagulability of the altered fibrine occurring in rheumatic fever; and hence, probably, checking or preventing the deposits of lymph on the endo- or peri-cardium. He (Dr. Garrod) stated his opinion, that the influence of the bicarbonate was felt not only in shortening the duration of the articular affection, but also in preventing or moderating the cardiac disease. After enumerating many details of the method adopted, and the value of certain adjuncts, as opium, calomel, and occasional general depletion, he proceeded to recommend a plan of treatment which, from his experience, he considered calculated to ensure the greatest amount of success, and thought it probable that the total duration of the disease might, on the average, be reduced to about ten days—provided that the treatment was adopted early, and no serious complication existed.

Dr. COPLAND inquired whether the author of the paper had used the bicarbonate of soda, and what were the results. The alkaline treatment of rheumatism had been long in use. In his treatise on that disease he had recommended, after the proper evacuation of the bowels, that the decoction of bark with alkalies should be resorted to. He mentioned some cases in which the disease appeared to have been cut short by the following plan of treatment:—A powder, consisting of half a drachm of magnesia and the same quantity of milk of sulphur, was given night and morning, and bark with alkalies, as mentioned above, three times during the day. Alkalies had been recommended many years since by a friend of his, in a work entitled, "The Simple Treatment of Disease."—*Lancet, March* 3, 1855, p. 239.

9.—DISCUSSION ON CANCER.

By MM. VELPEAU, ROBERT, and DELAFOND.

[Considerable difference of opinion exists in Paris and London on the use which may be derived from the microscope in the diagnosis of cancer. M. Velpeau does not place confidence in it. The following are M. Robert's remarks on the same subject. He says,]

You are aware that, according to the microscopists, the cancer-cell is the specific characteristic of cancer. I will not pause to discuss, as M. Velpeau has done, the relative value of the cells, the nuclei, and the nucleoli; for, although I have certain convictions of my own on the subject, I think it is better to leave questions like this to those men who have studied them *specially*, and not to meddle with topics about which we know little practically. You will perhaps say that I am adopting the definition of cancer given by the microscopists, totally forgetting that my opponent refuses to accept their opinions. M. Velpeau has said that the cancer-cell cannot be the specific anatomical character of cancer, because it has been found in non-cancerous tumours. I would ask my learned colleague by what authority he declares the tumours which contained the cells in question, not to have been cancerous? Because, he will reply, they did not return after extirpation. From this it would follow that every tumour which does not recur after removal, although it contains myriads of so-called cancer-cells, is not cancer; and from this I should also be correct in concluding that the so-called cases recorded in M. Velpeau's work, at p. 585, were not cancers at all. And, by carrying this mode of reasoning still further, I could prove that my colleague has not, any more than Munro and Macfarlan, cured a single case of cancer.

The physiological are not less marked than the anatomical characters of cancer. The disease has a constant tendency to become generalized, and manifested elsewhere, either in the neighbouring lymphatics, in some other organ, or in the viscera themselves, especially in the lungs and the liver. Before cancerous tumours ulcerate, and injure the constitution by profuse suppuration and hemorrhages, we often see the signs of general cachexia become manifested. The malady constantly returns after extirpation, and the progress of cancer becomes much more rapidly fatal in exact proportion to the frequency of its recurrence, and the operations for its removal. I have often noticed this fact, but was never more struck by it than in the case of a woman, aged 38, who after four successive ablations of a small tumour of the mamma, had three recurrences of the malady in less than two years, and who returned

to the hospital three months after the fourth operation, where she died from the recurrence of a multitude of similar tumours within the chest.

Fibro-plastic tumours are always circumscribed, and have not the irregular ramified (*rameuse*) form which characterises certain cancers, especially scirrhus. On their first appearance they possess a firm, slightly elastic consistence; they are homogeneous in structure, and contain no juice. Their tissue is termed by Lebert, *fibro-plastic*, and it is composed of cells, and of fibres which exactly resemble the elements of cellular tissue during their development. These tumours advance but slowly, and they are, for a long time, quite compatible with all the appearances of good health. Nevertheless they may become softened, ulcerated, and even generalised, and they may occasion death by being developed in the viscera. Of these facts M. Velpeau has given us several examples. But even among those rare cases in which the malignity of the fibro-plastic tissue rivals that of true cancer, we find consoling facts, which M. Velpeau ought not to have passed over in silence. Of these facts, which are now very numerous, I shall mention a few.

Paget (in his 'Lectures on Tumours,' 1853) mentions a case quoted from Maclagan, of a girl, who, in 1832, was operated on for a firm, elastic, fibrous-looking tumour, in the left lumbar region. It returned after a year, and was again removed in 1834. A second recurrence happened after a period of eighteen months, and the third operation was completely successful; for in 1850—fifteen years after it—she was, and had continued until then, in perfect health.

In 1839 I removed from the right axilla of a young man, a large tumour, the size of both fists, which was circumscribed, lobulated, but firm and elastic, and appeared to be composed of fibrous tissue intermixed with fat. In five years thereafter it returned, and I operated again in October, 1844, removing a tumour, the size of my fist, possessing the very same appearance as the first. Again it returned after the lapse of other four years, and for the third time it was extirpated, in January, 1849. M. Lebert examined the tumour, and pronounced it to be fibro-plastic tissue mixed with fat. Since this time the patient has enjoyed perfect health.

Again, Mr. Syme, in 1839, removed a tumour, situated over the first rib, from a man aged 48. A second operation was necessary in 1843, a third tumour was removed in 1847, and a fourth in 1849, and a fifth in 1851. Professor Bennett examined these tumours microscopically, and found them to be composed of fibro-plastic tissue. Shortly after this fifth operation another tumour, of a considerable size, was formed under the clavicle. During all this time the patient's general health was never impaired. Ultimately this tumour ulcerated; hemorrhage took place; and, worn out by it, the patient died in July, 1852. *There was not a trace of the malady to be found in any of the internal organs.*

In September 1849, M. Chassaignac removed a tumour from beneath the left clavicle. In a few months it reappeared, and a second operation was performed in December, 1850, a third in July, 1851, a fourth in January, 1852, a fifth in July the same year, and finally, a sixth in October, 1853. Since that date, M. Chassaignac has not seen the patient. During the course of these operations the general health continued excellent, and there was no implication of any of the lymphatics. The tumours were examined each time by M. Lebert, who pronounced their structure to be purely fibro-plastic, mixed with a little gelatinous matter.

When we reflect on the course of all these fibro-plastic tumours, on the obstinacy of their recurrence, and on the fatal termination of many of them, we certainly cannot regard them as of a benign nature. But, on the other hand, are we correct if we class them with cancerous tumours? Has an encephaloid or a scirrhous tumour ever been extirpated for four, five, or six recurrences, without the disease becoming generalised, and the constitution cachectic? And can the instances of fibro-plastic recurrence in the viscera be compared, for frequency of manifestation, to the more constant return of cancer in the glands, and within the cavities of the chest and abdomen?

I would next speak of *epithelial affections*, which are composed anatomically of epithelium cells or of the epidermis which normally covers the skin and mucous membranes. I cannot conceive how M. Velpeau can say that the structure of such tumours (if we are to believe the microscope) renders them akin to benign growths, such as warts, corns, &c. What does it signify, that two tumours have in common a normal anatomical element, if this element be not disposed and combined in the

same manner in each? In warts and corns, the epidermis, though more abundant, stands in exactly the same relations to the other parts of the skin as it does in the normal condition of the parts. We shall find on examination that this is not the case in an epithelial tumour. M. Velpeau's assimilation, then, is more specious than real.

Epithelial tumours appear principally on tegumentary surfaces, and ought to be studied separately according as they are situated on the skin or the mucous membranes. On the skin they begin by small crusts (*boutons crouteux*), warts, or fissures, and they form ulcers called *cancroid*, on account of their differing in appearance from cancer, progressing more slowly, and having less tendency to infect the system. The utmost evil that they do is to affect the neighbouring glands. When treated by energetic appliances, calculated rapidly to destroy the indurated parts, they often become healed definitely and radically. The epithelial tumours of the mucous surfaces (as types of which I shall specify those of the tongue and cervix uteri) are different in their progress; their course is more rapid; they reappear almost invariably after extirpation, and they terminate by causing death. Their tendency to generalization, however, is very far inferior to that of true cancer. If we examine the bodies of those women who have died of these deep ulcers of the cervix, we may often find the uterus partly destroyed; and the bladder, peritoneum, and great blood-vessels may be perforated by the disease. But, in such cases, we never find epithelial masses deposited in the viscera; the utmost traces of the disease which we can discover are the slightly enlarged glands in the neighbourhood of the uterus. It is the same with epithelial ulcers of the tongue; the glands of the neck alone become affected; and when the patients die, it is rather from being exhausted by hemorrhages, and from the long continued deglutition of putrid matters, than from any generalization of the disease having occurred.

At the orifices of the body, where the skin and mucous membrane become blended together, epithelial tumours possess, what I call, *mixed* characters, which they communicate to the complexed structure of the tissues affected. But there, as on the skin, the tumours continue for a long while in the condition of local maladies; and although it may be proved that their extirpation is less frequently curative than of those on the skin, it can also be demonstrated that the radical cures effected by the operation are very highly encouraging. I am quite aware that epithelial tumours *may* become propagated or reproduced in the lymphatic glands, and even in the viscera; but such instances of generalization are very rare.

These, then, are the differences, both of the minute structure and the mode of progress, of the three lesions which are generally confounded together under the term —cancer. We cannot consider epithelial and fibro-plastic tumours as benign in their nature, and we certainly cannot, in point of malignity, rank them with cancerous maladies properly so called; M. Velpeau does not admit these distinctions, for to him all these lesions are cancers, and he does not recognise the speciality of any of their elements. If the patient *must* sink, "what does it matter," he asks, "in *what manner* he dies,—whether by the disorganization of the diseased parts or from the results of general infection?" I would answer, in the first place, that all these maladies are not alike in their progress and severity; but even supposing that they *were* all equally fatal, is it not important to study them, in order to establish the differences between them, and to endeavour, sooner or later, to apply the knowledge thus gained to their scientific treatment? I feel convinced, that at present, the only question concerning cancer that is at all settled, is that of prognosis. Is it nothing for us to know, that although the removal of a cancer leaves the patient exposed to the danger of an almost certain recurrence, sooner or later followed by death,—that the extirpation of an epithelial or fibro-plastic tumour is much more hopeful and satisfactory in its results? If, in the latter case, a return of the disease should take place, the surgeon may operate again with greater hope and courage, knowing well that a radical cure is not impossible.

Let us, then, study these diseases more and more, and let us endeavour to distinguish them from those maladies with which they are often confounded. Let us strive to learn still more accurately their clinical differences; and, in doubtful cases, let us always avail ourselves of the aid afforded us by the microscope.

M. Robert expressed his dissent from what had been stated by M. Delafond, that the cancer-cell was not characteristic or specific in its appearance and shape. M.

Delafond had asserted that the form of this cell was not constant in its form; that it was spherical or ovoid when it was not compressed by the surrounding tissues, and that it was elongated or fusiform when it existed under opposite conditions. These plausible statements were not borne out by facts, for he had often seen ovoid and spherical cells in the densest scirrhus, as well as elongated cells in softened encephaloma; and moreover, fusiform bodies were common in subcutaneous fibro-plastic tumours, which were quite exempt from compression. M. Delafond had stated that the volume of the cells varied according to the phase of their development, but this was quite unproven. M. Robert exhibited a series of drawings illustrative of the appearance of the cancerous, fibro-plastic, and epithelial elements, and also of pus-globules, and he showed that, although occasionally they might present somewhat abnormal aspects, there could be no difficulty, in the great majority of instances, in recognising them, and distinguishing them from one another.

Another objection raised against the specific nature of the cancer-cell was, that in several of the normal tissues of the body, we found cells which presented a very great analogy, if not a perfect similitude, to those of cancer. This view originated with Virchow, and for a time was popular in Germany. According to that pathologist, the pavement epithelium of the mucous membrane of the ureters, the bladder, the lungs, and the conjunctivæ, resembled very much the cells of cancer; and scarcely a year ago M. Michel, of Strasbourg, had traced the same analogy in the cells of the medulla of the bones of young children. M. Robert had examined all these structures, assisted by able microscopists, in order to ascertain the truth of the statements; and after repeatedly examining and comparing together the tissues mentioned above and the elements of cancerous growths, he never experienced any difficulty in distinguishing the different structures. It might be asked, Why have distinguished histologists in other countries arrived at different results? Might it not be due to differences in the power of the microscopes used? A microscope of high powers revealed structural appearances which were invisible to any one using an instrument of lower powers. Cancerous elements appeared very differently when viewed under powers of 300 and 500 diameters. He thought, therefore, that the objections raised against the specific character of the cancer element were more specious than real, and he maintained that an experienced eye could always distinguish them from other structures, whether normal or pathological.

M. Velpeau had principally contested the specific nature of the cancer elements in a pathological and clinical point of view; and he alleged that he had found cancer-cells in non-cancerous tumours, which were extirpated without the occurrence of any relapse. But M. Robert held that, if M. Velpeau admitted the curability of true cancer by operation, the non-recurrence of the tumours in question did not prove them to have been non-malignant. M. Velpeau said he did not found his diagnosis on the non-recurrence of tumours, but on the general appearance of the patient. But among all the admitted clinical signs of cancer, there was not a single pathognomonic unmistakeable symptom, when the cachexia was absent, and generalization of the disease was not yet manifested. Modern histology was daily showing us that the symptoms reputed to be characteristic of cancer could not be relied upon as such, since they were being constantly met with in morbid productions of a totally different nature. For example, it had been held, from time immemorial, that retraction of the nipple, occurring in tumours of the breast, was a pathognomonic sign of scirrhus. But M. Robin had lately described a form of mammary hypertrophy, in which this retraction of the nipple was very well marked, and which consisted simply in a hypertrophy of the glandular *culs de sac*, and in atrophy of the excretury ducts, which give to the breast the aspect of scirrhus. Two cases of this, which had recently been witnessed within a very short time, led him to believe that this affection was by no means rare.

Again, when a mammary tumour was extensively ulcerated, when its edges were fungous, sprouting (*vegetans*), and its base was greyish, sanious, discharging fœtid pus in great abundance; and when this was accompanied by hemorrhage and exhaustion, we said that we had to deal with a serious case of cancer. But, to M. Robert's own knowledge, a combination of all these alarming symptoms had occurred in two cases of simple hypertrophic tumours. In one of these cases he himself had operated with complete success, and the patient had for five years enjoyed perfect health. The other case was similarly successful in the hands of a celebrated surgeon in Paris.

But M. Velpeau had further asserted that the specific cancerous elements had been found absent in truly cancerous tumours. He had searched M. Velpeau's book in vain to find proofs of this.

M. Velpeau had also stated that tumours which at first contained no cancer cells, had been known to recur, having these cells in the secondary tumours; and he had cited several cases of this nature; but in all these cases the *primary* tumours had not been microscopically examined, and, therefore, there was no proof of their non-cancerous nature.

As to the benignity or malignity of the tumours, he thought that it was better to direct our attention to their prognosis and manner of evolution than to their anatomical structure. *All tumours were relatively benign or malignant.* A true scirrhus, which had existed eight or ten years, without implicating the glands, or causing pain or cachexia, and which, after its removal, did not recur, or did so only after the lapse of a few years, was extremely benign when compared to an encephaloma, which ran through all its phases, and killed the patient in less than a year. The same was true of epithelial, fibro-plastic, and other tumours not so well known, which recurred after ablation. A *noli me tangere* of the cheek, or ala nasi, was a hundred times more benign than an epithelioma of the under lip, of the rectum, or of the cervix uteri. And further, such a scirrhus as he had described was much more malignant than a simple adenoid tumour, of large size, which was ulcerated, and discharging fetid pus profusely. If, therefore, in the majority of cases, structural characters were constant and easily demonstrated, nothing could be more variable and uncertain than the prognosis and diagnosis we might base simply upon the evolution of morbid products.

We had then two problems to solve; that of structure, and that of prognosis. The solutions of these were not antagonistic, and we ought not to seek only to solve one of the questions. But the science of the present must precede that of the future, and anatomical diagnosis must precede prognosis.

This view appeared to him a specific and a practical one. What had been done respectively by the anatomical and clinical men, at the bedsides of the patients? The former had divided tumours into cancerous, epithelial, fibro-plastic, vascular, glandular, &c. By long and patient study they had become acquainted with the delicate characters of the tumours, which were long passed by unobserved; and they had tried to discover in what particular lesions particular elements were found, and thus they had frequently been successful in establishing a correct diagnosis. They knew that one tumour might remain for a long time unremoved, without serious injury to the system; that another could not heal spontaneously; and that the former was more benign than the latter, &c. They thus formed a mental estimate of the patients' history, weighed their good and bad chances, and finally were able to say in what cases it was right to operate, and in what other cases it was prudent to leave the disease undisturbed.

But what had the purely clinical men done? Was their judgment, based on traditions of doubtful value and on pure empiricism, better than the more complete and exact views of the anatomical school? When one of them was called to treat a tumour, if he found that the patient had a bad aspect, and sinister physiognomy, he removed the growth like an anatomist. But while the latter, in such circumstances, strove first to confirm, by means of the scalpel and the microscope, his first impressions as to the nature of the disease, in order to strengthen his practice if he was in the right, and to modify his first opinions if he had been deceived, the former, limiting the circle of his researches, made an uncertain diagnosis and a doubtful prognosis.

It was more philosophical and conformable to science to base our classifications of tumours on their anatomical composition and structure. If the microscope had dispelled many illusions as to the curability of true cancer, it had also revealed to us the possibility of curing diseases which often, but for it, were allowed to proceed to a fatal termination. The microscope could not, any more than pathological anatomy in general, decide emphatically in every case, the benignity or malignity of a morbid production, but it enabled us, better than mere clinical observation, to foresee the result of treatment. In certain cases it gave us caution, and in others boldness. By means of the microscope we had made great advances in the study of all accidental growths; and if we became hereafter perfectly acquainted with the obscure question

under consideration, much of the praise would be due to those who had brought so much ardour and laudable perseverance to these truly difficult researches.

M. Delafond discussed at a great length the various cell-theories, and quoted the opinions of a great many authors in support of the doctrine of cellular unity. Besides the authority of the German microscopists, Virchow, Schwann, Förster, Müller, he cited passages from the works of MM. Lebert and Broca themselves, which were favourable to his opinion. He terminated his address by enunciating the following conclusions, which he thought were proven:—

1st. Independently of *normal* cells, there exist in the organism *pathological* cells, which cannot be distinguished by fixed and constant characters.

2nd. In the origin of their formation, the three cancer-cells—(the cancer-cell properly so called, the epithelial, and the fibro-plastic cells), present no marked differences among one another.

3d. If, under such circumstances, these cells, especially fibro-plastic cells, present differences of form and dimensions, these differences are only due to the varying densities of the tissues in which they are developed.

4th. The chemical characters, by means of which it has been endeavoured to distinguish cancer-cells, are not sufficiently well-marked to constitute a trustworthy distinction.

5th. The effects of chemical reagents are equally inadequate to afford a sufficient ground of distinction.

6th. Numerous facts have proved that true cancers do not always contain the cells called cancer-cells, while these cells have been found in tumours and tissues which were not cancerous.

7th. Still more numerous facts prove that epithelial tumours, which are not considered to be true cancers, are capable of recurrence not only at the same place, but at a distance from it.

M. Velpeau.—I have just published a work in which I affirm that cancer is sometimes curable, and that it can be very often diagnosed without the aid of the microscope. M. Robert having disputed these propositions, I have tried to defend them. *Firstly*, I have endeavoured to prove that cancer can be diagnosed independently of the aid of the microscope; but I have not meant by this to say that the microscope was useless, and that the works of microscopists were of no value, as has been imputed to me. *Secondly*, I have discussed the alleged existence of cancers having no cancer-cells. And *Thirdly*, the question whether cancer was curable. The first point has never been seriously contested, and the curability of cancer in certain cases does not appear to me to have been refuted. As regards the question of the specific nature of the cancer-cell, I am not competent to solve it in a microscopical point of view. I have only attacked it clinically. The recurrence of tumours containing no cancer-cells has convinced me that the cell is not the pathognomonic sign of cancer. M. Robert has contested my facts, and asked how I knew that it was a cancer by resting my opinion on its recurrence. Let me, once for all, answer the question.

Its non-recurrence is to me a proof that a tumour is not cancerous only when the fact is found existing in conjunction with other circumstances possessing an equal negative value. I have always founded my diagnosis on the combination of these negative characters. I admit that I may have been sometimes deceived, but how can I have been constantly deceived throughout the numerous series of cases? M. Robert has spoken of the retraction of the nipple not being a pathognomonic sign of cancer, but on what grounds does he allege that M. Robin's two cases of mammary tumours were not cancers? You answer,—On the absence of cancer-cells. But I know both cases, one of which was my own, and in it the malady recurred two months and a half after the operation. How can this be said not to have been cancer, or how can confidence be placed on the absence of the cell as a diagnostic mark? No one has pretended that retraction of the nipple *alone* constituted a specific mark of cancer; for how can we diagnose a malady by a single character?—*Monthly Journal of Med.*, *Dec.* 1854, *p.* 550, *and Feb.* 1855, *p.* 167.

10.—*On Necræmia.* By Dr. C. H. Jones, F.R.S.—This term is applied by Dr. Williams to that condition of the blood, in which it appears to be itself primarily

and specially affected, and to lose its vital properties. It is, in fact, death beginning with the blood. The appearance of petechiæ and vibices on the external surface, the occurrence of more extensive hemorrhages in the internal parts, the general fluidity of the blood, and frequently its unusually dark or otherwise altered aspect, its poisonous properties, as exhibited in its deleterious operations on other animals, and its proneness to pass into decomposition, point out the blood as the first seat of disorder; and, by the failure of its natural properties and functions, as the vivifier of all structure and function, it is plainly the medium by which death begins in the body. The blood, the natural source of life to the whole body, is itself dead, and spreads death instead of life. The heart's action is faltering and feeble; the atonic vessels become the seat of congestions, and readily permit extravasations. The brain, insufficiently stimulated, after slight delirium, lapses into stupor; the medulla no longer regularly responds to the *besoin de respirer;* and the respiratory movements become irregular. Muscular strength is utterly lost; offensive colliquative diarrhœa, or passive intestinal hemorrhage often occurs; sloughing sores, or actual gangrene of various parts are easily produced; and putrefaction commences almost as soon as life is extinct. The track of the superficial veins is marked by bloody stains; hypostatic congestion takes place to a great extent; the blood remains fluid, and stains the lining membranes of the vessels. Rokitansky describes the blood as often foamy, from the development of gas, and of a dirty red raspberry-jelly colour; its serum dark from exuded hæmatine; and its globules swollen up by endosmosis. Coagula are either totally absent, or are very soft and small. The exudations are of a dirty red—turbid, thin. There is scarcely any rigor mortis; the tissue of the heart and of other organs is flaccid and softened, and stained by imbitition of the serum. Gas is quickly formed in the vessels and in the areolar tissue, giving rise to a kind of emphysema. It is very remarkable that this necræmic condition, or one closely resembling it, may be brought on by violent shocks inflicted on the nervous system, as well as by the introduction of miasmatic or animal poisons into the circulation. Violent convulsions, overwhelming emotions, the shock of an amputation, a stroke of lightning, even a severe exhausting labour, are mentioned by the German pathologist as having produced this effect. More common causes are, however, malignant scarlatina and typhus, yellow fever, the plague, and the disease called glanders. It may be said, generally, that the early appearance of sinking and prostration in any fever, indicates that the blood is thus seriously affected. We are ignorant what is the exact nature of the changes which take place in this condition of the blood. Probably they are more of a vital than merely chemical kind—that is, they affect the properties of the blood more than its composition. The blood globules do not appear to be destroyed; but they circulate probably some time before death, as so many dead particles prove to be enlarged and to stagnate in the capillaries, and to part with their contained hæmatine. The fibrine is in great part destroyed; but how this comes to pass we are ignorant. We can perceive, on the whole, scarce anything more than that the powers of vital chemistry rapidly decay, and those of ordinary chemical affinity supply their place.—*Association Journal, Dec.* 22, 1854, *p.* 1144.

DISEASES OF THE NERVOUS SYSTEM.

11.—ON THE NERVOUS SYSTEM.

By Dr. Marshall Hall, F.R.S., &c.

The cerebrum, the cerebellum, and, in a certain sense, the medulla oblongata, may be regarded as the Centre of the Cerebral system, of the nerves of special sense, and of volition, of intellect, and of the passions, of pleasure and of pain. By its means we are placed in relation to all that is *external, psychically.* By its means we perceive, by its means we seize and use external objects.

There is good reason to believe that intellect is seated high, the emotions lower, and the faculty of pleasure and of pain lower still, in their general cerebral centre.

Below the centre of the cerebral system is that of the spinal system, and its analogue in the articulata, &c. The spinal centre includes the medulla oblongata and medulla spinalis. It may be divided into—

1. The Medulla Oblongata, the spinal centre principally of the acts of ingestion and of egestion, by which the preservation of the individual is effected, and especially those of deglutition, respiration, &c. With this centre, the pneumogastric, the diaphragmatic, &c., of such momentous importance in these acts, are essentially linked;

2. The Brachial Enlargement;

3. The Femoral Enlargement; and

4. What, I think, may be designated the *Lower* Medulla Oblongata, the analogue with its nerves of the organs of generation, of the upper with its pneumogastric; and the spinal centre in the acts for the perpetuation of the species.

In each of the acts of ingestion and egestion to which I have alluded, there is the essential agency of a diastaltic spinal arc. The pharynx and larynx are excited by eisodic and exodic branches of the pneumogastric; the pneumogastric is the internal excitor of inspiration, acting through the spinal centre and the diaphragmatic and intercostals.

Similar diastaltic nervous arcs constitute the nervous media through which emission, conception, parturition, &c., are effected. In all these are nervous *origins* in exciting surfaces, an eisodic nerve, the spinal centre, an exodic nerve, and muscular terminations. The *law of action*, the mode of action, the channel of action, are in all identical or similar. All have the same object or objects—ingestion, retention, egestion, exclusion.

Can anything be more simple and scientifically beautiful?

All these acts are *physical*. As the cerebral acts depend on the immortal ψυχη, these depend on the *vis nervosa;* as those relate to external objects psychically,—viz., to objects *without*, these relate to them physically, acting *from without to within, and from within to without.*

The spinal centre, with its nervous arcs, and the muscles in which these terminate, are the dynamic agents which accomplish that "*tourbillon*" so eloquently described by Cuvier.

The ganglionic system relates to all that is *within*, governing the *peri*staltic actions, assimilation, the secretions, the excretions, &c.

But of these functions I must treat as cursorily as I have done of the cerebral system.

They are greatly under the influence of emotion, and of sympathetic or synergic action through the spinal centre. We have only to observe the effect of derangement of the stomach, or of eroded viscera, on the action of the heart, the skin, &c., in connexion with experiment, to arrive at this conclusion.

The experiments to which I allude are the following:—Let the head be removed in a frog, the spinal marrow remaining, and the circulation be ready to fail: if we now crush the stomach or a limb with a hammer, the action of the heart ceases. Let the conditions be the same in another frog, with the addition that the spinal marrow is also removed; in this case no influence is perceived, on crushing a limb or the stomach, on the action of the heart. Now, the difference is the presence or absence of the spinal centre. This experiment, however, requires careful repetition.

[The *Spinal System* is the source and seat of the class of *Convulsive diseases.* Dr. Marshall Hall commences an interesting lecture on this subject by saying:]

I must commence my explanation by drawing your attention to three most important physiological facts:—

If we puncture or lacerate, or otherwise injure the cerebrum or cerebellum, or the cerebral nerves of special sense, in every possible manner, we observe *no results, no phenomena*, no expression of pain, no excited movements.

The cerebral system in all its parts is, in this respect, inexcitable.

But if we touch any part of the spinal system, and especially the spinal centre, with the point of a needle, ever so slightly, there are immediately excited muscular movements, spasm, convulsion.

The spinal system is, in all its parts—*excitor, excito-motor.*

These important facts constitute the foundation of all our knowledge of the diseases of the nervous system, and are the very source of all *diagnosis* in regard to them.

For the same facts, as is proved by clinical observation, hold in regard to the human subject.

By experiment and by observation, then, we are led to these conclusions:

No lesion of the cerebral system, limited to the cerebrum, *can* be attended by spasm or convulsion.

No structural lesion of the spinal system, short of destruction, *can* occur *without* exciting spasm or convulsion.

If, in affections of the cerebral system, we observe spasm or convulsion, it is because it is *not* limited, in itself or in its effects, to the cerebral system. Thus, congestion of the centre of the cerebral system may, as in hanging, become extended to that of the spinal system, and then spasm or convulsion supervenes.

Or affections of the spinal centre may consist in gently applied *pressure*, not in lesion of tissue, and then paralysis, and not spasm or convulsion, will be observed. Or it may consist in sudden or violent *shock*, or utter destruction, and then, I need scarcely say, that paralysis, and not spasm or convulsion, will occur. All this we have seen demonstrated by experiment; all this you will see hereafter in your observation in the clinical, and especially the surgical ward.

What a means of *diagnosis*, then, have we obtained by these simple physiological facts! How has physiology become our guide in practice!

I have thus explained to you how disease of the cerebral centre may, by pressure downwards, affect the spinal centre. But another question arises: How does a disease of the spinal system, as a pure convulsive malady, affect the cerebral?—for such is the frequent event, as we observe in epilepsy.

Observe what occurs in the most *marked* cases of this dire malady: The head becomes fixed, or there is torticollis, by the action of *the muscles of the Neck*,—trachelismus; the jugular and other veins of the neck are compressed; the capillary system of the neck, the face, the intra-cranial structures congested; the veins start, the arteries throb; the *cerebral* centre becomes implicated. *All is intelligible! All is explained!*

Affection of the centre of the cerebral system is extended to that of the spinal system—the medulla oblongata—by downward pressure: affections of the spinal system are extended to the cerebrum by *trachelismus!*

That *all* this is so, is demonstrable. That it is *always* so, is, perhaps, what I ought not, however disposed to do so, to assert. And does it not constitute a beautiful specimen of the *physiology of disease?* But such is the whole of our recent views of epilepsy and of the *Class* of convulsive diseases, as observed in the clinical ward, to which I must now recur.

And first, pray observe with me an experiment or two.

I have removed the cerebral centre in this frog. Its spinal system is still most lively and energetic. I take and rub the toe between my thumb and finger; you observe the almost convulsive movements produced: I now touch the upper part of the spinal centre with this needle; the animal is thrown into the most violent convulsions!

These experiments are *types* of disease; they are the types of epilepsy and other convulsive diseases.

The *first* is the type of *ex-centric* epilepsy;

The *second* is the type of *centric* epilepsy.

For the first was induced by an excitant applied to a part of the spinal system at a distance from its centre; the second to that centre itself.

So it is in epilepsy; so it is in convulsive diseases generally, in the human subject! Do not these simple and illustrative facts interest you? They are of the deepest interest.

Do you not now perceive *how* dentition, and gastric and enteric and uterine irritation, may excite spasm or convulsion, even epilepsy? And do you not now perceive that ex-centric epilepsy is no more an *opprobrium medicorum* than infantile or puerperal convulsion; for that is equally intelligible?

You see again the violent convulsions which I excite by again touching the spinal centre in this frog by this needle.

Such is the dire state of things in centric epilepsy, whether this be of organic origin, or have been organic in its course; whether it be congenital, on one hand, or inveterate on the other.

The cases of epilepsy which occur in private practice are, for the most part, of the former or ex-centric kind, and *curable*, however, for many reasons, difficult of cure.

The cases which we see in the lunatic asylum, and especially in the workhouse, are generally centric, and—must I not add?—*too* generally incurable.

Allow me to draw your attention to this *Table*. It has cost me much labour, but it contains in a page a volume. You will find it in the 'Lancet,' July 20, 1850, —(*see Retrospect*, vol. xxii.) Procure it, or copy it, and study it well. For, allow me to say, I know of no such specimen of this most interesting pathology. The *chain* of events in epilepsy and other convulsive diseases, is pursued in it, *link by link*. The *causes* are arranged in this first column, the principles and modes of treatment in this last; the *rationale*, the various stages, forms, and complications, in the intermediate ones. It would occupy us too long to enter into its details. I have only time for a rapid sketch.

The ex-centric causes act on eisodic nerves, thence on the spinal centre, and thence along exodic nerves, on the various muscles. Of these few, many, or all may be affected, and the malady may be the very slightest or the very direst. Amongst the rest—I was about to say, chief amongst the rest—are the muscles of *the Neck and of the Larynx*.

By the contraction of the muscles of the neck the veins of the neck are compressed, as I have already noticed; and the extra-cranial and intra-cranial tissues and organs become affected with venous congestion and all its consequences. Nay, I am disposed to say that, in every case in which there is such venous congestion of the neck, face, eyes, brain, it arises from this trachelismus, latent or evident. I do not wish you, however, to adopt this opinion without examination; but rather to subject it to a most careful investigation for yourselves.

By the contraction of the muscles of the larynx in laryngismus, this vital inlet to the respiration, combined, as it usually is, with *breath-struggles*, I believe the direst form of epilepsy, with its direst effects, as coma, mania, dementia, &c., to be produced.

I do not say that these effects may *not* arise in cases in which there is only trachelismus and no laryngismus; but I am convinced that they are *chiefly* the effects of laryngismus; and hence they have subsided in cases in which the laryngismus has been disarmed by tracheotomy.

And now you perceive in what sense epilepsy may be regarded as a cerebral disease. Cerebral in its very *origin* it can never be. It may be intra-cranial in its origin, because within the cranium there are many tissues, as the membranes, from which eisodic nerves arise, many eisodic nerves, the fifth especially, pursuing there a part of their course; and the medulla oblongata,—all which may be excited by the presence of an exostosis, a tumour, a variety of diseases. But I repeat that no disease of the cerebral centre, limited to that centre in itself and its effects, does or can produce epilepsy.

Exciting causes: the Spinal system; the Neck; the encephalon; such are the links of this chain of pathology. There remains another to which I must now draw your especial attention: it is—the medulla oblongata, with its *pneumogastric nerve*.

I have already adverted to laryngismus as giving to epilepsy, and convulsions generally, its most formidable character. This form of laryngismus is *spasmodic* or *convulsive*, excited through the spinal system.

But there is another form of laryngismus. After a severe epileptic convulsion, the patient is left in a state of *coma*, or, as it may be termed, of *simple apoplexy*. This condition may prove fatal. The respiration becomes stertorous—that is, there is laryngismus. But this laryngismus is very different from that formerly described: it is less complete, but more persistent; it is, indeed, permanent; some one said, in rather familiar phrase, "The patient snores his life away." This laryngismus is *not*, like the former, spasmodic, but paralytic. It is owing to compression or congestion —an apoplectic state—of the medulla oblongata, and *paralysis* of the pneumogastric nerve, in which every branch, as well as the recurrent laryngeal, is implicated.

Apoplexy of the medulla oblongata; paralysis of the pneumogastric nerve!

Legallois—the illustrious Legallois—divided the pneumogastric above the origin of the recurrent. The larynx became partially closed. The breathing was *stertorous*. In a word, every phenomenon was obscured which I am about to describe to you as occurring in the coma or simple apoplexy observed after a severe epileptic seizure.

And what do we observe? We observe paralytic laryngismus, impeded respiration, augmented apoplexy; we also observe—or, rather, I have observed—*a diffused*

bronchial rattle, the effect of *paralysis* of the bronchial and pulmonary branches of the pneumogastric!

Here a most interesting phenomenon presents itself. The cerebrum is not essential to *life*. How then does apoplexy, simple, cerebral apoplexy, destroy life? It does not do so. Apoplexy only destroys life when, by its congestion by downward pressure, it implicates the medulla oblongata and its pneumogastric nerve. It is by the apoplexy of this medulla and the paralysis of this nerve, that life is, in effect, destroyed!

Generally the patient dies of paralytic laryngismus, or of laryngeal asphyxia. From this danger he is rescued by early tracheotomy.

But if tracheotomy *be* performed, but performed too tardily, the patient, however saved from *laryngeal* asphyxia, dies of *bronchial* asphyxia!

There are other branches of the pneumogastric nerve—the cardiac and the gastric. The heart and the stomach participate in the paralysis of the general pneumogastric.

The observant physician will observe palpitation as a symptom in some cases of epilepsy. This is explained by excitement of the cardiac branches of the pneumogastric.

In other cases, there are hiccup, eructation, acidity of the stomach; the effects of excitement of the gastric branches of this composite nerve.

As in these cases there is morbid *action* in the heart and stomach, so in deep epileptic coma there is *failure* of the powers of these organs.

The pneumogastric nerve is not more involved in epilepsy than in other convulsive diseases. All the phenomena just described as observed in the former, are again met with in the latter, only, of course, modified by the exciting causes, and the conditions of age and sex.

In infants there is the same or similar action of the muscles of the neck, and of the larynx and respiration. In puerperal convulsions the symptoms are identically those of epilepsy.

There is a form of epilepsy which I have not yet noticed. It is the *epilepsia syncopalis*. The patient, instead of turning purple, turns pale and ghastly. Sometimes this syncope is *fatal!*

So it is in the laryngismus stridulus of infants. The little patient not unfrequently dies suddenly—too suddenly to be the effect of asphyxia. The case is cardiac, syncopal.

Both these cases are affections [*shock?*] produced through the medium of the pneumogastric nerve.

But, you remember that I spoke, in my lecture of yesterday, of a *lower* medulla oblongata, and of a *lower* analogue of the pneumogastric. These, too, are influenced by the convulsive seizure.

The laryngismus stridulus is frequently excited by enteric irritation. This is also frequently attended by fret of the bladder.

Epilepsy is frequently induced by enteric and uterine irritation, and is sometimes attended by involuntary evacuations of the bladder, rectum, &c.

The urine is frequently morbid in both cases.

There is something in all this *analogous* to the modes of action of the medulla oblongata and the pneumogastric nerve.

It is obvious that both these portions of the nervous system form ONE. Both are liable to be affected in one and the same convulsive seizure.

Perhaps the *lower* portion of this system is most clearly isolated in what relates to *Obstetrics*. To excite the cervix uteri is frequently to excite abortion. To excite the cervix uteri during parturition is to excite the uterine efforts. But to excite the cervix uteri during parturition is, also, frequently to excite a puerperal convulsion! These are facts familiar to every experienced practitioner. They are *proofs* that "a meddlesome midwifery is bad."

Other facts of the same kind are constantly elicited in practice: to excite the rectum by the pressure of the fingers backwards, excites uterine action; and it is well known that the excitement of the internal surface of the uterus by the hand, introduced in certain forms of hemorrhage from inertia, excites the uterus to energetic contraction.

All this is accomplished through the *lower* medulla and its analogue of the pneumogastric.

I think that a subject very inscrutable hitherto will receive elucidation from these studies—that of sterility, inertia of the uterus, &c., &c.

[Dr. Hall proceeds to give us some interesting remarks on the *treatment of Convulsive diseases, and Epilepsy in particular.*]

The excitements, the irritations must be, as far as possible, avoided. The morbid conditions of the blood must be remedied. A low posture and deep sleep must be carefully shunned. The neck must be cautiously guarded against a tight collar.

One event we must constantly suspect: an undue acidity in the stomach. Against this, the bicarbonate of potass is the present remedy, and should be taken on every recurrence of a threatening symptom. Many patients have carried about with them a solution of twenty-five grains of this bicarbonate in two ounces of water, and taken it in such circumstances with the greatest advantage.

Another event to be suspected is the formation and retention of scybala in the colon. These are only effectually removed by an enema of three pints of warm water, slowly administered. But in this manner, too, the threatened attack has been averted.

One patient succeeded in replacing an attack by a fit of vomiting, taking for that purpose, on any threatening, half a drachm of ipecacuanha. To this I have sometimes *added* an equal quantity of the bicarbonate of potass.

The bowels should be kept well relieved daily; but I have known a dose of purgative medicine constantly to produce a seizure.

One patient took many remedies empirically. None did good. But the turpentine acted injuriously.

Of all our remedies, none is more important than a well-regulated, simple, digestible, nutritious diet.

Against the undue excitability of the spinal centre in epilepsy, well-appointed walking exercise is the specific remedy. But *effort* or *fatigue* adds to the evil.

I think there is as the cause of sleep a latent trachelismus. The usual posture during sleep is the recumbent. Both these must conduce to the epileptic seizure. I have constantly recommended a posture during sleep at an angle of fifty degrees. And I think that I have seen advantage from gentle movements and noises in the patient's room during the first sleep, to prevent deep sleep.

All empirical remedies may be safely discarded. It is very improbable that *any* individual agent should be able to accomplish all that is required in a case of epilepsy. We may well ask, now that this malady is somewhat understood, why epilepsy should be treated empirically more than apoplexy, or than infantine or puerperal convulsion?

It has appeared to me that the hyoscyamus may allay the nervous excitability so frequently observed in epilepsy. As a little spirit obviously does good in the undue cerebral excitement of delirium tremens, I have thought that minute doses of strychnia may be useful in the undue spinal excitability in epilepsy. For the organic changes induced by epilepsy, mercury is the remedy.—*Lancet, Nov.* 11, 18, *and Dec.* 2, 1854, pp. 392, 411, 451.

12.—ON THE USE OF COTYLEDON UMBILICUS IN EPILEPSY.

By Dr. E. H. Sieveking, Assistant Physician to St. Mary's Hospital.

[The active principle of this drug has not yet been discovered, but would seem to be analogous to digitalis, from its slightly sedative and diuretic action. The only organs acted upon by it appear to be the kidneys. In the first case of epilepsy reported by Dr. Sieveking, the patient had been subject to the fits for six years. Before giving the medicine, she had six fits in about eight days. The following was prescribed:]

℞. Liq. cotyl. umbil. 3j. ter die., continued to Feb. 15, with one slight fit, which only lasted ten minutes.

The cotyledon was persevered in, with an occasional colocynth pill, and a little cough medicine, to the 29th of March, only one fit having taken place during that period. On the 6th she was discharged, but the cure was not permanent; for, on the 19th of April, 1853, she returned, stating that the fits had returned the previous week, and that seven paroxysms had occurred since.

The liq. cotyl. umbil. was again ordered in the same dose.
May 3. One fit since last night. Pergat.
20th. No fit for a fortnight. Pergat.
June 11. One fit since last visit. Pergat.
July 5. One fit in the night, the week before last, from excitement; none since.
Electuar. ferri 3j. ter die.
12th. Pergat.
25th. One fit since last visit.
℞. Nitr. arg. gr. ss., extr. gent. gr. iv., pil. ter die sumenda.
Aug. 2. Had a fit during the last visit, lasting a short time, and a slight fit last night. Rep. pil.
16th. Has had two fits. Rep. liq. cotyled. umbil. 3j. ter die.
25th. No fits. Pergat.
Sept. 28th. One fit since Aug. 25. Pergat.

After this there was no return of fits during the whole period she continued to take the medicine; she also lost what she called the slight attacks—brief, momentary fits of vertigo and semi-unconsciousness, and was discharged, cured, on Nov. 18th. The cure, however, again was not permanent; and the last notes of the case, I regret to say, appear to show that the cotyledon had lost whatever efficacy it may previously have possessed. She returned in Jan. 1854, after a free interval of three months. She was now pregnant, and continued to have the fits up to the period of delivery, without relief from the cotyledon.

Case 2.—Aug. 2, 1853. Thomas C., aged 44, a widower and messenger, has for three days past been subject to dizziness; three days ago there was loss of consciousness, and paralysis of the hands for a quarter of an hour, of which there is now no trace. There is pain in the forehead; pulse 84; tongue furred. On Sept. 3 he had a fit, in which he lost his senses for about half an hour. Two fits occurred again between Aug. 19th and Sept. 23rd; another on Sept. 25th, lasting ten minutes. The treatment up to this time consisted mainly in counter-irritation and purging, the symptoms being regarded as indicating a congestive, if not inflammatory condition of the brain.

Oct. 7. Last Sunday (five days ago) his hand became pale and "drawn;" there was a sensation passing up to the head, and he then fell down in a fit, and remained unconscious for about ten minutes. The same symptoms have occurred once a week. He was recommended to apply a ligature round the arm, and tighten it as soon as he feels the commencement of the aura, and take liq. cotyled. umb. 3ij. ter die.
Oct. 14. States that he has derived a great deal of good from the last medicine. Pergat.
21st. Has a sort of cramp in his hands when he wakes up in the morning, with a "tapping" on the head. Pergat.
28th. Can't get rid of the ticking in the head; complains of nothing else, except a slight increase of deafness. Says the cotyledon umbilicus acts as a diuretic. Rep. cotyled. umbilicus; empl. vesicat. auricul.
Nov. 11th. No fit had recurred since the first employment of the cotyledon umbilicus. I have not seen this patient since.

Case 3.—This case might have seemed a peculiarly favourable case for the employment of cotyledon umbilicus, from the apparent absence of all complications; but it failed, like the various other remedies which were employed, in effecting any permanent impression. It was that of an intelligent lad, aged 17, who, for three years before coming under treatment had been subject to fits, occurring on an average once or twice a month. There was no hereditary or other taint traceable; masturbation was absolutely denied by the father, who had closely watched his son, and the former attributed the malady solely to overreading. The cotyledon was given alone, or with taraxacum, from Oct. 18th to Nov. 29th; and although the headache of which he complained was relieved by a seton, the monthly fits were not postponed, and have of late even become more numerous.

Case 4 was one that would be at once pronounced hopeless as regards the efficacy of any drug. The patient was a porter, aged 30, who had been subject to epileptic fits from his infancy, whose father was paralytic and brother epileptic; they were preceded by a sense of his "head going round," and occurred once or twice

monthly; and, during the last twelve months, he had also been affected with choreic movements of the left arm. He took the extract of the cotyledon, first, gr. xv.; afterwards, gr. xxx., three times a-day, while under treatment, viz., from Feb. 7, 1854, to April 4, with occasional purges. During that period he had four fits.

Case 5.—March 14, 1853. W. L., a lad, aged 15, of a scrofulous habit; had no disease till the appearance of the fits, to which he has been subject about a year and a half. The fits commence with a scream, and the unconsciousness sometimes lasts two hours. They are followed by frontal headache. He has not bitten his tongue. The pulse is 80, strong; the bowels open; there are no worms; he passes much urine before the fits; he now has five to six fits daily. He never had a blow on the head as a child. Masturbation denied. Ext. cotyled. umbil. gr. xv. ter die. Mist. magn. et rhei 3j. alterna mane.

28th. Has had no fits since last visit; feels much better; has no headache. Pergat usu misturæ et pilul.

April 7. Has had no return of fits till last night, when he had a "dreadful" fit, and another this morning. Wets his bed at night. The free interval was regarded by the mother as unusually long. Unfortunately, this was the last opportunity I had of seeing the boy; but the case, so far as it goes, may be used as evidence in favour of the cotyledon, the more so, as the patient's age and sex, as well as the duration of the illness, do not favour the assumption of any mere mental impression.

Case 6.—In this case, a young clerk, of 17 years of age, epileptic fits had occurred, with gradually increasing frequency, for a year before he came under treatment. Gradually increasing doses of sulphate of zinc, attaining gr. x., effected what appeared to be a cure; but, at the commencement of the treatment, the cotyledon was tried from April 21 to May 9, during which time he had two fits, which were said to have been less strong than the previous, though about the same rate of frequency.

Case 7.—This case was one in which, not only from the ancient date of the disease, but the probability of centric disorganisazion (tubercle) of the brain, and the impoverished circumstances of the patient, treatment promised to be of little avail. He was a porter, aged 18, who suffered from the first fit at the age of five. The fits had remained in abeyance till within about eight months before coming under treatment. It was commenced with the nitrate of silver, but the fits became more frequent; the silver was therefore exchanged for the cotyledon, and the previous interval was restored, and intermissions subsequently protracted longer than they had been before treatment. A severe attack of pleuritis prevented the further attendance of the patient, and he was lost sight of.

The above cases, I think, hold out some encouragement to the further employment of this certainly innocuous remedy. Whatever its active properties, much undoubtedly depends upon the manner in which the extract is prepared, as in all vegetable extracts; much must also depend, if the active principle is remedial in epilepsy, upon a proper diagnosis of the exact form of the disease to which it is applicable. A good deal remains to be done before these desiderata will be supplied. —*Med. Times and Gazette, Dec.* 2, 1854, *p.* 564.

13.—ON INFANTILE CONVULSIONS ARISING FROM EXCENTRIC IRRITATION.

By Dr. Robert Molloy.

[Dr. Molloy dissents altogether from the opinion that dentition is a frequent cause of convulsions in children. These convulsions may arise at all periods of dentition, and often without the slightest indication of irritation.]

As, therefore, these varying conditions of dentition bear no constant or real relation to the epileptic convulsions, it is of the highest importance to notice what other morbid symptoms accompany them, in order to trace out the effective cause of so much mischief. This, I believe, will be found almost invariably in the *gastro-intestinal secretions*, which are constantly at fault. My attention was first directed to this subject more particularly by the case of a child, in which I had an opportunity of watching, not only the symptoms present in these convulsive attacks, but all

which preceded and followed them. I remarked on the first epileptic attack to which he was ever subjected, that while under the relaxing influence of the warm bath, an involuntary and very copious offensive discharge from the bowels took place, with *immediate* remission of the convulsive moments. On the second occasion a similar result occurred; and here the change was so instantaneous and striking, from an apparently dying condition to one of promised recovery, that it determined me to adopt the plan of treatment which I have since found singularly successful. This consists in the use of warm water injections, which may be administered to a great extent, and, even in the case of infants, more copiously than any one might believe who has never tried them. I have not unfrequently passed into the bowels twelve syringefuls (about twenty-four ounces) before they could be made to act properly, but in every case with immediate relief, as soon as even a small quantity had been injected. This I have commonly seen:—A child is labouring under fearful convulsive movements; two or three syringefuls of fluid are thrown into the intestines, and on the instant the spasms subside, although but for a short time. I have awaited their return, and again injected more, with a like result; but of necessity the patient is not permanently secure from their recurrence until the bowels have been well emptied, all the offensive discharges removed, and a more healthy condition obtained.

The fæcal evacuations in these cases *contain little or no bile*, and are usually very offensive. In children thus predisposed, a daily inspection of the intestinal excreta should take place, as a necessary part of the medical attendant's duty; for I have very frequently been enabled to foretell the approach of these epileptic attacks by the appearances they presented. An additional argument, derived from physiology, may not inaptly be mentioned here. The perspiration of infants is very trifling; their urine, too, is comparatively free from saline constituents. Now, in consequence of the law of compensation, it is probable the greater part of these secretions may have to pass off through the intestines, rendering their contents normally more irritating than, in process of growth, they afterwards become; while the greater relative size of the liver, doing duty, as it were, for the lungs, as well as for itself, compels an incessant functional activity, and requires equally the discharge of bile into, and its speedy withdrawal from, the bowels, to maintain the complete integrity of its functions.

The notes I have taken at different times would enable me to cite numberless cases in corroboration of these statements; but I shall content myself with giving outline sketches of a few of them, in which the symptoms were either strongly marked, or had appeared after other plans of treatment had been tried.

A girl, three years of age, semi-idiotic, with congenitally malformed head, had been subject to frequently-recurring convulsive attacks, which, at that time, were referred to the irritation of dentition, and treated in the usual manner by warm baths, incisions of the gums, and application of leeches to the head. One afternoon, at a period when the child was considered unusually well, an epileptic seizure came suddenly on, without any premonitory symptom whatever. Application being made to me, I gave an emetic, which acted in about twenty minutes, ejecting from the stomach a large quantity of undigested salt beef and greens, she had been allowed to eat at dinner. Upon this there was immediate cessation of all convulsive movements; neither did they again return.

Another, four years old, the favoured daughter of highly-respectable people, who indulged her to an extent which assigned to her complete mastery over them—a child, fat and gross, with a colour in her cheeks that might vie with any country rustic's—was seized one morning, about eight o'clock, with an attack of epilepsy, the first she had ever experienced. The convulsions were certainly the most fearful I have yet seen in one so young; and I was informed that she had been drooping for a few days, was listless and sleepy, but that no aperient medicine had been given for a long time, in consequence of the resistance she made whenever it was attempted. In this instance the effect of the injections was curious and extremely interesting; three or four plunges of the syringe stilled at once, as though by magic, the violent convulsive movements, and although the effect was only temporary, it is of importance to remark, that it was invariable, and always followed by the same calm. It appeared as if the distension of the water removed the surface of the bowels from contact with any irritating matter which was present. As no liquid medicine could

be administered by the mouth, I placed some calomel powders on the tongue, and persevered with injections until six in the evening, when the bowels were for the first time relieved, bringing away an enormous quantity of round masses of fæcal matter, *entirely free from bile*, and resembling nothing more nearly than the once-favoured therapeutic remedy—"album græcum." There scybala were literally alive with thread-worms, in themselves a most fertile source of intestinal irritation. On this occurrence the convulsions remitted, and shortly afterwards disappeared altogether; nor has she to this date ever had a return.

The third case refers also to a girl, upwards of three years of age, who had completed dentition, and was the only living child of four, the others having died at an early age, either from cerebral excitement or decided hydrocephalus. She was a sharp, precocious child, so that attention would more naturally be directed to the state of the brain in consequence of this circumstance, and of her previous history. The first epileptic attack occurred eighteen months since, at a time when she had been taken a short trip into the country, for change of air, and continued for some hours after she was brought home. She was under treatment longer than usual, principally on account of the time which had elapsed before any continuous plan could be had recourse to. A second epileptic seizure, a few weeks ago, was promptly and decidedly removed by injections. Neither of these attacks differed in any respect from those previously detailed.

The many facts I have witnessed strongly inclined me to the belief, that this predisposition to excentric epilepsy is hereditary; and one case in particular afforded a very peculiar confirmation of this view. A lady, who eventually became my patient, had three children, all girls, none of them of the strongest. They were sorely tried in passing through the customary diseases, but never had any convulsive attack. After being a widow for a few years, she married again, and gave birth to a boy, who suffered long and severely from convulsions. On inquiry, I learnt, not only that the father had been similarly afflicted when he was a child, but that none of the wife's or her first husband's family had ever been subject to them. This was the more conspicuous, because I have generally remarked that girls are more predisposed than boys to epilepsy.

But perhaps the real value of the plan of treatment here advocated was more advantageously displayed in the following case than in those already reported. It made a deep impression upon me at the time, not only on account of the intolerable state of the intestinal secretions in a child of such tender age, but from the immediate and almost instantaneous relief afforded by the injection, which active aperients had failed to obtain. A young child, in whom dentition was complete, was attacked one evening with convulsions, the first he had experienced, and before I could reach the house had two severe fits. A plentiful enema was at once administered, which, in the course of a few minutes, brought away an enormous quantity of extremely offensive evacuation, so that the room was hardly bearable. No return. About two months subsequently, threatening symptoms of convulsion again made their appearance. Purgative medicines were given, by which the bowels were copiously relieved; but, from the persistence of the symptoms, it was deemed advisable to employ an enema, with the immediate effect of emptying the lower intestines of a very great quantity of slate-coloured and most unhealthy secretion. He was quite well the next day, and has continued so ever since.—*Lancet, Jan.* 20, 1855, *p.* 63.

14.—TREATMENT OF HEMIPLEGIA FROM SOFTENING OF THE BRAIN.

By Dr. J. Hughes Bennett, Edinburgh.

I believe that the most important end for the practitioner to aim at, in the early treatment of these cases, is to keep down the frequency and force of the heart's action. For this purpose the strict maintenance of the horizontal posture is of the highest moment; and when the patient is conscious, it is most desirable that the mind should be tranquillized by every means. It will, of course, likewise be necessary to remove all local impediments to the easy flow of the circulating fluid; and it is as well that the head should be slightly raised, sufficient to prevent gravi-

tation favouring the escape of blood from the ruptured vessels, but not so as to create any impediment to the flow which might embarrass the action of the heart.

To remove any source of nervous irritation which may be operating injuriously on the brain, the bowels should be cleared; and in order that there may be as little effort as possible on the part of the patient in the expulsion of the contents of the bowels, it is expedient that this should be done by enema; but if this fail, and the vital power of the patient do not forbid it, you may give croton oil, a drop or two of which, placed on the tongue, will operate freely; or calomel, in powder, to the extent of five or ten grains, which may be similarly administered. I would advise you to limit the further administration of drugs, to giving some slight corrective, as an alkali—ammonia being, on the whole, the most appropriate—unless, indeed, you find the patient in an extremely prostrate condition, when it will be necessary to combine with it the cautious exhibition of other stimulants and restoratives, as chloric æther, brandy, &c.

The question of bleeding will arise; and, under the popular notion, that all head attacks are accompanied and caused by the rush of blood to the head, you will be pressed to have recourse to this expedient. There are three objects to be attained by bleeding; *first*, to diminish an undue amount of blood in the head; *secondly*, to check hemorrhage, or to prevent it; and *lastly*, to quiet the heart's action.

If the patient be cold and collapsed, it is clear you should not take blood; nor should you have recourse to this practice if the heart's action be very feeble or intermittent; nor if there be an anæmic state; nor if the patient be of very advanced age; nor if the evidence of extensive disease of the arterial system, or of the heart, leave no doubt on the subject; nor would it be desirable to bleed if it were clear that already a large amount of hemorrhage had taken place into the brain. Should none of these objections exist, then you will have to consider whether any or all of the indications above named need to be fulfilled, and whether bleeding (local or general) promises to fulfil them. As to the first indication, namely, the diminution of an undue amount of blood in the brain, I think modern investigation of the actual state of that organ clearly points out, that the brain is not in a hyperæmic state, in the cases in which the form of hemiplegia under discussion is likely to occur. Will taking blood check or prevent hemorrhage? The sudden or rapid abstraction of a moderate quantity of blood, either from the arm or temple, or by skilful cupping from the nape of the neck, may, I can conceive, check hemorrhage; and with this object it is, sometimes, a very justifiable practice, but the quantity taken should not be large. Now and then bleeding helps to diminish the frequency and force of the heart's action; but here, again, the quantity of blood withdrawn should be moderate, for the removal of much blood is apt to quicken the heart's action and render the blood poor. I would have you to look upon this question, to bleed or not to bleed, as almost the most important one you will have to decide; and, judging from my own experience on this point, as well as from the results of the practice in a large number of cases collected from various sources, as I pointed out in a former lecture, I have come to the conclusion that, in cases of white softening, with or without hemorrhage, you are less likely to err by omitting rather than by adopting the practice.

You will often be consulted as to "some expedient for promoting the restoration of the paralyzed limbs to their normal condition." To this question, after having given a fair trial to the various means which have been proposed, I must reply, that I know of nothing which more decidedly benefits the paralyzed limbs than a regulated system of exercise; active, when the patient is capable of it; passive, if otherwise. As to the use of electricity, which is now much in vogue, or the employment of strychnia, which has been strongly recommended, I feel satisfied, as the result of a large experience, that the former requires to be used with much caution, and that the latter is very apt to do mischief, and never does good. I have seen cases in which, after the employment of electricity for some time, that agent has apparently brought on pain in the head, and has excited something like an inflammatory process in the brain. And so strychnia also will induce an analogous condition of brain, and will increase the rigidity of the paralyzed muscles.—*Monthly Journal of Medicine, March, 1855, p. 244.*

15.—ON THE TREATMENT OF APOPLEXY.

By Dr. R. B. Todd, F.R.S., Physician to King's College Hospital.

[The following constitutes Dr. Todd's directions with regard to *bleeding* in apoplexy:—]

The treatment of patients with apoplectic symptoms must not be regarded as a matter of routine, but as a question of grave import, and which demands the most anxious consideration of the practitioner. Let me add, that it sometimes requires the exercise of no small courage and self-possession to resist adopting that practice; for the popular feeling, led by a formerly too prevalent medical practice, is entirely in favour of it, and would readily condemn a practitioner as guilty of the death of his patient who suffered him to die unbled. It is a far more dashing and courageous thing to open a vein on the spot, and in the presence of a number of anxious friends, than to adopt less showy, and apparently less active measures.

But, indeed, you need not be inactive, even if you decide against adopting the plan of bleeding. Having placed your patient in an easy position, in which no excitement of muscular action is likely to take place (for you must bear in mind that reflex actions may often be readily excited in these apoplectic cases), you should immediately direct your attention to the state of the stomach and intestinal canal. Sometimes in these cases the stomach is overloaded, or the bowels are confined, and the administration of a quickly acting emetic, or even of some purgative medicine, will often provoke a moderate sickness, which unloads the former. Nor can there be any objection to adopting measures to clear out the bowels, either by an active purgative administered by the mouth, such as calomel or croton oil, or by a stimulating and purgative enema, or both.

If upon full inquiry into all the particulars of the case, you find that your patient is of full plethoric habit, with too much blood in his body, and with a sufficiently strong heart, you may bleed him with every chance of success; but if he has been of intemperate habits, is labouring under organic disease of the heart and arteries, is of gouty or rheumatic constitution, then, whatever popular or medical custom may say, my advice to you is, hesitate much before you deplete by bleeding.

The objects which it is proposed to gain by bleeding, are, a diminution of the cerebral congestion, and the stoppage of the hemorrhage into the brain; and where it is quite clear that cerebral congestion does exist, and that that congestion causes the cerebral hemorrhage, this is clearly a rational practice. But you must bear in mind that in a large number of the cases—probably the majority—there is in reality no cerebral congestion, and that the hemorrhage is of a kind not likely to be stopped by taking away blood—by establishing another hemorrhage elsewhere.

On the whole, then, I think that the results of experience denote that the majority of cases of apoplexy are best treated by purging, shaving the head and keeping it cool—perhaps blistering, and that bleeding is rarely applicable, except to the young, vigorous, strong, and plethoric.—*Monthly Journal of Med., March*, 1855, *p.* 239.

16.—*On Cerebral Softening.* By Dr. R. B. Todd, F.R.S.—In looking into the pathology of this condition of the minute blood-vessels, you may fairly ask, is it, indeed, the primary and essential disease? or is it not rather an effect of a depraved nutrition of the tissues to which these canals are the carriers of nutriment? Now, a little reflection would lead you to say that both these questions may be answered in the affirmative, and the evil of which we speak may either commence in the brain-tissue, the brain-fibre, or the brain-cell, which consequently cease to draw upon the blood-vessels, to their wonted extent, and these blood-vessels, therefore, deliver to them their nutrient matter in diminished quantity: their activity is, consequently, proportionably diminished, and an atrophic state ensues. Or the evil may commence in the blood-vessels; and this will probably be the favourite view. We know that the larger vessels are the seat of numerous atheromatous deposits, and not only the cerebral vessels but the aorta itself, and the cardial arteries, as we can often learn by feeling the pulse in the living individual. Why should not this atheromatous state extend to the minutest vessels? I do not

express any strong opinion in favour of either of these views; but I would say that it seems to me that pathologists do not enough regard the tissue itself as being the starting point of morbid change; and are too apt to overlook the fact that the power of attraction of the tissue for certain elements of the blood, is not the least important agent in determining the greater or less flow of blood to the organ or tissue.

And, indeed, whichever of these two views we adopt, who will say that in the blood itself we do not find the chief source of evil, which may tell either upon the blood-vessels or upon the elements of the tissues?

Without waiting to decide upon this point, we may, then, lay it down that, whether in consequence of the disease of the blood-vessels or otherwise, the nutrition of the brain becomes impaired, and this shows itself mainly in altered consistence of the nervous matter. Its colour does not undergo any appreciable alteration, and if you look at a portion through the microscope you will not detect any obvious change, the vesicles remain the same, and the fibres remain unchanged; but, as I said, the consistence is diminished, instead of being firm, the tissue of the organ has become soft and pulpy, and in some cases almost diffluent and of the consistence of cream. The diseased blood-vessels lie in the midst of this pulpy mass for some time without undergoing any further change, but sooner or later under some mental emotion, or during some increased heart's action depending either upon mental emotion, upon derangement of the digestive organs, some bodily exertion, or increased mental effort of any kind, the blood is sent with undue force, or in unusual quantity, into the vessels, and in consequence the vascular canals in the pulpy portion of the cerebral tissue being deprived of their usual support, give way, and blood is effused into the softened part of the brain, which it breaks up, and the more readily in consequence of its already diminished consistence. This is the *rationale* of the development of many an attack of apoplexy, from which the patient may or may not recover, according to the extent of brain previously softened, and according to the amount of blood effused.—*Monthly Journal of Med., March*, 1855, *p.* 240.

17.—ON HYSTERIC DISEASES.

By Frederic C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[We all know that with respect to hysteria the diagnosis is sometimes difficult. It is often as difficult to say what a certain disease is *not*, as to say what it really *is*. Of this singular disease, Mr. Skey gives us some good examples:]

Eliza N., aged seventeen, a healthy looking girl, by occupation a domestic servant, was admitted into Treasurer's ward on Oct. 25th. She had been suffering from severe shooting pain in the left gluteal region. The slightest touch upon, or movement of, the joint, caused the most excruciating pain, and she complained especially of pressure over the insertions of the psoas and iliacus muscles. She was quite unable to walk; gentle application of the hand gave severe pain, but on Mr. Skey taking the limb up and raising it gently, while directing her attention to other subjects, she uttered no complaint, and the pressure was then made forcibly in every direction without causing suffering. Ordered common diet. Fluid extract of opium, two drachms; compound soap liniment, one ounce and a half: make a liniment to be applied to the hips. Citrate of iron and quinine, eight grains; tincture of orange, one drachm; in water, twice a day. Subsequently some tincture of valerian was added to the above, and she was discharged on Dec. 5th, able to walk perfectly well.

Emma Y., aged nineteen, was admitted on Nov. 19th last, into Lucas ward, suffering from supposed disease of the hip-joint, in which she remembered to have had considerable pain for many years, but which had recently increased greatly in severity. She had been treated for this disease by a great variety of applications, but without relief. She menstruated last in the month of May, the catamenia being for the above period entirely interrupted. She complained of severe pain on the slightest movement of the limb, and expressed by her countenance great apprehension at the slightest examination. On applying the hand to the surface over the joint she shrank, but shortly allowed Mr. Skey to take the limb into his hands, and

considerable pressure was made around and over the articulation, without occasioning additional pain, nor was any caused by rather violent friction of the hand over the trochanter. Mr. Skey considered the malady to be that of hysterical neuralgia. She was ordered aloes and myrrh pill, ten grains, every other night. Fluid extract of opium diluted in compound soap liniment, to be rubbed over the entire joint, and compound iron mixture was ordered three times in each day. She was placed on meat diet and allowed a pint of porter daily. Under this treatment all symptoms of local pain subsided in three weeks, and she walked about the ward without lameness.

Caroline W., aged thirty-three years, a pale, emaciated woman, was admitted into Lucas ward on Nov. 9th. She had been confined about six months previously, and had never fully recovered from the *debilitating effects of a profuse hemorrhage which followed her delivery.* Her thigh was bent at a right angle to her body, and turned over towards the opposite side. It had been in this state for about a fortnight, but had been gradually contracting for more than a month.

She complained of most intense pain on the slightest pressure in the region of the joint. A week after her admission she was placed under the influence of chloroform, and the leg was gradually, but cautiously, brought down to a straight line, and bandaged to a side thigh-splint. At the same time she was ordered ammoniated citrate of iron, eight grains; tincture of valerian, one drachm, in an ounce of water, thrice a day; wine, four ounces daily.

After being kept in this position for about five days, the bandage was taken off, and the leg remained in the natural position, without the slightest tendency of the muscles to displace it. She was discharged on Dec. 1st, complaining of some slight pain, but greatly improved in health and condition.

It is quite astonishing with what exactitude the details of true affections are represented, not only by the presence of pain and other concomitants, but by the physical manifestations of the disease imitated. In their treatment I have no hesitation in expressing my belief that they are too frequently confounded, and means the most unsuitable and unappropriate are resorted to for the cure. There is no locality more fruitful of this error than that of the spine. Within twenty years of the present time, our sea-bathing places of resort were crowded by hundreds of young women who were confined to the horizontal or semi-recumbent posture for years, were excluded from society, debarred their education, restricted in their food, and compelled to adopt the miserable substitute of a medicated diet for years, simply because a hot sponge created a sensation of uneasiness, or, if you prefer it, of pain, at a given vertebra, when passed downwards along with the spine, or on some other frivolous evidence of actual disease. Now true disease of the spinal column is very rare in any class of society; but these cases were one and all cases of hysteria—cases of impaired health in young women, varying in age from seventeen to twenty-five, or older—operating on constitutions, of which temporary debility, coupled with permanent excitability of the nervous system, were the especial characteristics. Diseases of the spine were the rage and fashion of the day.

Let us examine for one moment the value of this evidence which sanctions treatment so large—both physically and morally, so injurious. By disease of the spine, we understand disease of the spinal column. This column is, except under very unusual circumstances, the subject and seat of but one disease—viz., caries, which consists of a softening, and subsequently a crumbling, of the bodies of the vertebræ. The loss of substance to the upright column, in persons whose health admits of exercise, is necessarily followed by curvature and loss of height to the person. The spinous processes project backward, as you are well aware, and angular curvature is permanent. Now, there are two most important pieces of evidence to be adduced against the presence of such a condition of the vertebral column in these cases. The first is obtained from the fact, that pain, correctly so termed, is not a concomitant of the true disease; whereas, in the factitious form of the disease, pain is, in truth, the only one indication. If you examine the spine of an hysteric young woman, something more than moderate pressure will rarely fail to obtain an expression of suffering somewhere; and to a medical man who has cudgelled his brain for months, to discover the real cause of his patient's ailments, this small expression, whether of pain, or whether of mere discomfort, will suffice—a disease of the spine is proclaimed to the parents, and the result we all know too well.

Let me give you a practical hint on the mode of examination of the spine of a young person. Call to your recollection the last case you saw examined. The patient, probably a young female, was placed on a bed on her face—or rather, it should be said, she placed herself. Her arms are bent at her side, so that the weight of the chest is mainly sustained by the forearms. The consequence is, that the dorsal vetebræ are not in a continuous line with the lumbar, but rise upwards gradually towards the head. The consequence of this attitude is, that an angle is formed at the lumbar part of the column, which causes a strain upon the anterior vetebral ligament, and I defy any person, male or female, to suffer pressure on these lumbar vertebræ, in such an attitude, without more or less, if not of positive pain, at least of the apprehension, which causes an expression of suffering. When you examine the spinal column, for this or any other purpose, lay your patient perfectly flat on a hair mattress, with the arms extended from the body, or let her sit or stand upright.

The second form of evidence is this: there is an anachronism in the periods of the two diseases, the true and the false. We rarely find caries of the spine to appear for the first time long after puberty—rarely after the age of sixteen or seventeen; while the putative malady—the factitious disease—rarely appears till after the age of seventeen; and what is yet more remarkable is, that the spine always, eventually, wins the victory, and the patients recover without the slightest evidence of a curved spine!

Bearing on this subject of supposed diseases of the spine was a case I recently saw in private. A gentleman, aged about thirty, after a residence of many years in the West Indies, returned to England, and during last winter was for some days exposed to severe cold. Shortly afterwards, while making a spring, he was seized with severe pain in the neighbourhood of the third and fourth lumbar vertebræ, and was confined to his bed for a few days, when he resumed his out-door occupations, but with the recurrence of pain in the same locality towards the end of each day. In the summer, he had a return of the former pain, and two issues were placed in his back opposite the seat of it. When I saw him, he said he was extremely ill, and that he thought he could not recover. He had now pain in the shoulder-blades, and in the muscles of the arm which are attached to the trunk. He complained, also, of a pain almost allied to tic in the cheek and temple; and I also found that the pain in the spinal column had left the original site, and had attacked the dorsal vertebræ from about the fourth to the eighth. He was not so bad, however, but that he sat upright in bed, at his own instigation; and somewhat to the astonishment of himself and his attendant, I took the opportunity of giving his spine a good hearty rubbing, which shook him considerably; but observe, I had not the smallest complaint of pain to challenge my violent treatment. Now, I dare say you know the key to this case as well as I do.

But to return to hysteria. You will see abundance of hysteric hip affections, and in yet more abundance, hysteric affections of the knee. So general are these forms of disease, that I declare I think less error would result if every example of disease of these two joints, not caused by accident, which occurs in young women between sixteen and thirty years of age, were treated as hysteria. I state this opinion with the less hesitation, because we rarely have much difficulty in detecting the real disease. We can distinguish the real from the spurious much more readily than we can detect the spurious from the real. An hysteric affection of a joint is marked, first, by the sex and age of the person—generally a female, and young; second, we have more pain than in real disease; the pain is irregular—sometimes severe, sometimes entirely absent. On examination, the approach of the hand to the neighbourhood of the joint is attended by an exclamation anticipatory of pain, and a shrinking from pressure, often with tears. Pain is expressed on slight, but not increased by heavy, pressure, and rough manipulation. You may boldly shake the entire limb, while the patient's attention is engaged in some collateral thoughts. The head of the femur may be boldly revolved in its socket, and the limb carried round in all directions, at first slowly, and then with gradually increasing force.

I saw a lady, some months since, of about thirty-five years of age, who was brought up to London for surgical advice, on account of a severe affection of the hip-joint which she had suffered from, for some two or three years. She was on the point of leaving town when I paid her a single visit. When I entered the sitting-room of

her lodgings, she was lying on the sofa. I requested her removal to a bed in the room adjoining. While being carried to bed, her cries were most distressing, and some few minutes elapsed before I was admitted. When I again saw her, she said she really feared she could not submit to be examined; but I tranquillized her by speaking kindly and gently, and she at length submitted. I watched her face while I extended my hand towards her joint, and I never saw suffering more clearly depicted. I laid my hand with the utmost gentleness on her trochanter, and very gradually pressed it against the bone, and she did not shrink. Distracting her attention to the early history of her case, I quite raised her thigh, and turned it first gently, and then with force, though slightly, in all directions, and I then struck my hand repeatedly, and with force, on the trochanter. Everybody round the bed appeared astonished—and well they might be, for such force, applied to a joint that had been kept absolutely motionless for months—which appeared permanently inverted, and apparently abridged of its length, by reason of the curvature of the lumbar vertebræ—with a limb much wasted by inaction—might be deemed a curious *exposé* of its past history. This lady's health was wasted by long confinement and severe local and general depletive treatment. Of the result I know nothing, though I had one letter from the family, stating her improvement. I should mention, that the especial reason which led to my opinion being taken on this lady's case, was to ascertain whether or not her femur was dislocated, and the question was not, judging from the appearance of the limb, an inappropriate one. The thigh was drawn up, considerably inverted, and apparently fixed. All the tissues were soft and flabby, and the limb extremely pale. Any attempt to move it with the consent of the patient was followed by acute suffering. On examination, the anterior spine of the ilium was drawn up by long-continued curvature of the lumbar vertebræ, but the length of the entire limb was unchanged.

The difficulty of these cases is greatly increased by the local appearance of disease. In hip cases the position of the thigh is distorted—more or less fixed; and the whole limb is inverted. In the knee examples, the joint is swollen, and often hot. In rests, however, perfectly straight in bed. In the former we can detect the absence of that form of dislocation which its condition simulates, by the normal relation of the trochanter major to the anterior spinous process of the ilium. We are rendered suspicious of the existence of true disease by the more advanced age of the hysteric patient—by the severity, and, at the same time, the irregularity of the pain—and by the constitutional character of the patient. In the knee cases, although the joint is swollen, and the skin hot, yet the cavity of the joint is, for the most part, free from an undue quantity of fluid; and the swelling, as has been remarked by Sir Benjamin Brodie, is confined to the sides of the ligamentum patellæ. We more generally find irregular catamenia than otherwise, although this feature is not invariable; and we have an hysteric diathesis on which to base our inquiries.

In some cases, the harmony of the muscular action of the limb is entirely lost, and one set of muscles prevails in action over its antagonists. These insubordinate agents are mostly those of flexion: the arm may be bent up to an acute angle at the elbow joint, or the fingers may be closed, or the thigh drawn forcibly and closely up on the pelvis, one example of which I have mentioned. Amputation of such limbs formerly practised, left the diseased cause behind, but to be developed in other localities of the body. All these patients betray a weak condition of bodily health; the pulse is small and quick; the tongue pale-coloured; and severe and chronic cases are generally coupled with more or less of emaciation and debility of frame consequent upon it.

When local pain is a prominent symptom, it rarely fails to succumb to the external application of opium combined with a moderately stimulating liniment, such as the compound soap liniment. It is needless to apply for aid to any form of opiate tincture; nothing short of the extract will suffice: and I commonly employ the fluid extract of opium—such as is now a good deal used by the druggists in the neighbourhood in which I reside, and to several of whom I have myself introduced it.

The catamenia should, of course, be encouraged, but not forced; and, for general treatment, we must resort to iron, bark, good nutritious diet, with which a certain quantity of fermented liquor, whether beer or wine, in some form, should be combined. The profession is indebted to Sir Benjamin Brodie for an excellent practical work on this interesting subject.—*Lancet, Feb.* 24, 1855, *p.* 205.

18.—*Treatment of Sciatica.* By Dr. Peyton Blakiston, F.R.S. [Dr. Blakiston has pursued the following treatment for twenty years with considerable success. He first saw it adopted in Paris in 1833.]

A blister, about the size of a crown-piece, is placed over the chief seat of pain, which is usually the flattened part of the buttock. After it has risen well, and the cuticle has been thoroughly removed, the raw surface is sprinkled with a powder, consisting of one grain of acetate of morphia on an average, and a little white sugar. This dressing is repeated for six successive days, the surface of the blister being kept in a raw state, if requisite, by cantharides or savine cerate, or else by Albuspeyeres' plaster. This suffices for a very mild case; but, in severe cases of old standing, the pain will now be found to have left its original seat, and to have seized on the knee of the affected side. The same treatment is then applied to the ham; and, after six dressings, the pain will have generally disappeared, and the patient will rapidly recover. By this mode of treatment, eighty-three cases of uncomplicated sciatica have been cured, without a failure having come to the knowledge of the writer. This number might have been greatly augmented had it included the results arrived at by such of his friends and former pupils as had employed it at his suggestion, and which have been no less successful than those which occurred in his practice; but he is desirous of recording such only as have come under his own immediate notice, and for the accuracy of which he consequently can hold himself responsible.

In the great majority of these cases, no other drug was administered; but, in a few, some laxative medicine or injection was given to remove constipation.

In two or three cases, there was a tendency to double sciatica, and then the pain passed from the sciatic region first treated to that of the opposite side, and from thence down to the knee of this last side, but never attacked the knee of the side first affected.

It is right to mention, that in Hospital practice three cases were placed under the writer's care, which he considered more than doubtful, and they were therefore treated under protest, so to speak. They all turned out cases of hip disease, and therefore they are not included in those above enumerated.

The difference in the sensations felt by the patients on the first application of the morphia was remarkable; and, without any attempt to generalise, it may be stated that a close connection was observed between the sensations felt and the previous state of health. Thus the effect produced on three persons in robust health—a blacksmith, a gamekeeper, and a lady—was most intense; an extraordinary thrilling was felt over the whole body, particularly at the extremities, with great nausea and a tendency to faint. The lady vomited incessantly for twelve hours, so that it was found advisable to reduce the quantity of morphine and the powders to half a grain. On the other hand, a gentleman who had been much reduced by over-work and by long suffering, felt no effect whatever from the application of the powders, and yet he recovered in an equally short time with the others. A lady, also, who had been taking considerable doses of opium, hardly felt the application of morphia until it was increased to two grains; but this case has been excluded, because, although the sciatica was removed by the treatment, there remained an incurable disease, which eventually destroyed her.

One lady, 26 years of age, in whom the disease was not of long standing, obstinately refused to have a second powder applied; but, happily, the one application sufficed to effect a cure. In six cases the disease recurred after an interval of from five to eighteen months; and, in two of these, it recurred twice; but each attack was less severe than the one which preceded it, and yielded readily to the same treatment. It is possible, however, that relapses might have more frequently taken place without having come under the notice of the writer; but he thinks this cannot have happened very often.

Some other forms of neuralgia were also benefited by this mode of treatment. Thus, a very distressing case of neuralgia of the scalp yielded at once; and shooting pains, which frequently accompany cancer of the stomach, were sometimes much relieved by it.—*Med. Times and Gazette, Jan.* 27, 1855, *p.* 83.

19.—ON THE USE OF CHLOROFORM IN DELIRIUM TREMENS.

By JOLIFFE TUFNELL, Esq., Surgeon to the City of Dublin Hospital.

Regarding chloroform as I do, as a most valuable agent in the treatment of delirium tremens, as perfectly save if given with proper precaution, and highly efficacious if used with sufficient energy, I should be very sorry to see any check put to its employment in this affection; and I shall therefore beg to detail the particulars of a case lately under my care in the City of Dublin Hospital, which may tend to confirm confidence in its use, and prove that it can be given, and given freely too, in cases which are caused by suddenly leaving off the stimulant. The case is as follows—

J. N., aged 36, a sottish-looking individual, a tailor, was brought to the City of Dublin Hospital by his wife, at 10 A.M., on the 22nd of September last, presenting the ordinary symptoms of this disorder when fully established. The surface generally was bedewed. The eye wandering and restless; the tongue, when protruded, flabby, flocculent, and tremulous; the hands in perfect tremor when held off unsupported from the body. The pulse was full, but soft and very compressible; the conjunctivæ slightly congested. His wife stated that he had been on a drinking bout for some days, but gave it up on the 17th, apparently in consequence of sickness of the stomach having set in. He remained at home ill and unable to do anything on the 18th, and on the 19th the disease appeared, first manifested by his taking possession of a knife, which he said he must keep by him, ready to protect himself and her from a brother-in-law, who was plotting their destruction. From this time he had no sleep, and was perpetually on the watch until brought up to the hospital, a period of nearly eighty hours.

He was at once admitted, and ordered into a quiet ward, to be carefully watched, but not restrained, unless absolutely dangerous to those around him. To have a pint of porter, as he was complaining of thirst, and then a nicely broiled mutton chop, for he had not taken food for some time. Three leeches were next directed to be applied to the temple, and upon their coming off, a cold lotion to the head, after which a drachm of chloroform was to be given, and repeated at intervals at the discretion of the house surgeon, until sleep supervened.

Sept. 23rd. I visited him at 9 A.M., and received the following report:—He had taken the food and porter, and permitted the leeches to be applied, and afterwards remained quiet until four o'clock in the afternoon, when he began to be restless, and shortly afterwards violent, requiring restraint. For some time he would take no medicine, but at 6.30 was prevailed on to swallow a drachm of chloroform in a draught, and to repeat it at 7.30, again at 9.30—three drachms in three hours, but without producing any appreciable effect; and as the chloroform did not appear to be of any benefit the house-surgeon did not push it further, but at the same time (as I had expressed a desire that the treatment should be uncomplicated) refrained from giving any opiate. At the time of my visit the patient was tranquil, but had not slept, and there had been now no sleep for nearly 100 hours. The bowels had not been moved since admission; a turpentine injection was, therefore, directed to be given; the hair, which was very abundant, ordered to be reduced, and cold affusion to be employed for some time to the head, after which, as a brisk breeze was blowing, the bed was to be brought under the window, and the linen blind so directed as to keep a continual stream of cold air playing over the scalp. The following draught was to be taken at intervals, according to the effect produced:—

The yolk of an egg; white sugar, 1 drachm; whiskey, 4 drachms; a little nutmeg. All to be mixed well together; and to this a drachm of chloroform added at the time of being taken.

Strong beef-tea to be given in small quantities, as wished for, and chop and porter taken as on the preceding day.

Sept. 24th. At my visit at 9 A.M., I found the patient quietly asleep, and received from the house-surgeon the following statement: "Shortly after taking the chloroform he became violent, but only for a minute or two, he then tumbled off into a placid sleep, and remained so for half an hour. On awaking he was again very noisy and restive; a second dose was now given him; he became stupid for fifteen minutes, but did not sleep, and again became violent. As soon as possible the

draught was repeated, and on taking it he fell off asleep. In an hour he awoke, restless and noisy, but not so violent; after a short time, a fourth dose was administered, and under it he lay quiet, but did not sleep, and he soon began to be as furious as ever. A fifth dose was now given, plain, in cold water, which made him torpid, but from which he recovered and burst out again. A sixth drachm in water was then given, soon after which he dropped off into a slumber, and remained sleeping soundly through the night, and up to the time of my visit."

At 2 P.M. I called at the hospital to see another case, and looked into the ward to ascertain how he was going on. I found him awake and quite rational; he complained a good deal of soreness of the body generally, especially of the wrists, where he had been held when violent, but made no other remark. His bowels had been freely moved.

On the 26th he was convalescent, and on the 28th was discharged perfectly well.

Now here was a case, "brought on by repeated fits of intoxication, succeeded by a pause of perfect sobriety," one of deprivation of sleep for upwards of 100 hours, and with extreme nervous excitement, yet in which chloroform was given freely and with the happiest results; and I am satisfied it may be used generally, if administered with care. By the last expression I mean having a due regard to the support of the powers of life of the individual, by food (if he will take it) and by stimulants in the form of the egg flip with whiskey as prescribed. Chloroform is a powerful sedative, and therefore certainly not to be carelessly employed, but with due caution it may be safely and advantageously used.

As regards the objection "that chloroform often fails, and is not, therefore, to be relied upon for the treatment of delirium tremens," this I believe to have been the result solely of its not having been used in sufficiently large doses, or often enough repeated. Speaking generally, I think the intervals between its administration have been too long. Half a drachm of chloroform given every fourth hour may certainly be tried at first; but, if the second dose fail, it should be increased at once to a drachm, and the periods between the doses be entirely regulated by their effects. I do not regard chloroform as a cumulative medicine; I believe that it soon passes off by the lung, as the venous and charged blood is exposed to the air cells, and with it, its effects, leaving, however, in the case of delirium tremens, a sleep of a character that cannot be likened to the suspension of consciousness which it causes when employed as an anæsthetic. In the latter case the brain is overpowered, and insensibility from blood poisoning produced, which lasts so long as the blood is sufficiently charged with the chloroform, and no longer. In delirium tremens, on the contrary, it is not the continuous circulation of the chloroform that produces continuous sleep. The impression temporarily made upon the brain, if sufficient, gives to that organ the power of regaining a certain portion of its *vis insita*, and thence ability to discharge its proper functions.

Sleep being the suspended function, and the one nature and the system demands, it exercises or rather comes under the influence of it, and, if sufficient impression has been made by the chloroform, then the brain remains at rest until its normal tone is restored. Occasionally, it is true, we see genuine sleep follow upon inhalation of chloroform, after insensibility has been produced, and the individual, instead of returning to consciousness, passes into sleep, although repose is not needed by the brain.

Such an instance is the following:—Not long since I was sounding a young gentleman who had symptoms of calculus in the bladder. He had inhaled chloroform moderately from a handkerchief, and came sufficiently under its influence in about two minutes to have the bladder (without consciousness) carefully explored. The examination ended, the sound was withdrawn, and I accompanied his father to a corner of the room to speak upon his case. I was engaged in conversation for some time, and then prepared to leave the house, but was called back in haste by his father, who said that his son was senseless. On returning to the room, I found the boy was not as he supposed, but in a sound, comfortable sleep, from which he awake in a natural manner on hearing his father's voice.

Now until a calmative condition is produced, the chloroform already taken may, I believe, be considered as inert, and any good effect must be anticipated only from the succeeding dose. If this too fails, and delirium and excitement continue unchecked, then, after an interval of time sufficient to warrant belief that it also has

been unappreciated by the system, the dose must be repeated, and so continued, and I regard an hour as the full period that need be permitted to elapse before it is again given. Certain I am that the strength of the patient will be more rapidly expended by the continuance of the disorder—its clammy exudation, its muscular activity, and mental horrors, being left unchecked—than by any sedative influence of the chloroform.

I have no fear of its use internally. I have employed it to a degree that may startle those who have not had much experience of it. I have, in a case of traumatic tetanus in the City of Dublin Hospital, kept a young man, aged eighteen, under its influence for nine days and nine nights successively, during which time he took a quarter of a pint by the mouth, and a pint and a quarter were evaporated closely over the nostrils, carrying him in a state of unconsciousness through this fearful disease, and restoring him to perfect health.

I have lately in a case of tetanic convulsion in an infant, only eight weeks old, in consultation with my colleague, Dr. Beatty, employed it even more largely still, for no less than five ounces of chloroform were expended in vapour in twenty-four hours, completely controlling the convulsions, the child dying solely from inability to swallow sufficient support.

With these facts, then, before us, and the experience of the happy effects of chloroform in treating delirium tremens, as already recorded in this and other journals, I think we should give it the fullest and fairest trial; for the "horrors" are, in a tropical climate, by no means the manageable disorder we see them here, oftentimes unyielding to opium, and frequently terminating in death; and any remedy holding out the hope of recovery, which chloroform does, should be, I maintain, most severely tested, before it is reported as *inert*.—*Dublin Hospital Gazette, Feb.* 15, 1855, *p.* 19.

ORGANS OF RESPIRATION.

20.—ON A PREVAILING FORM OF CHRONIC PNEUMONIA.

By Dr. Richard Payne Cotton, Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

[Dr. Cotton thinks that acute diseases have been numerically declining whilst chronic diseases have been increasing. Chronic pneumonia has been generally considered to be a sequel to the acute form, but many cases would seem to be chronic from the first—original and independent diseases, running their course like any other chronic malady. The following account of this form of pneumonia is very interesting.]

Commencement of the disease.—The form of chronic pneumonia to which I would direct attention, may arise in persons under the most opposite circumstances as regards temperament, age, and predisposition. The strong and robust are as often its victims as are the feeble and the fragile. The old and the young are equally liable to its attacks. I have met with cases at the ages of four and of fourscore; but the middle period of life affords the greater number of patients.

Its exciting causes are those which might induce bronchitis, pleuritis, or a more active pneumonia,—such as exposure to cold and damp, sudden changes of climatic condition, and the like. Certain states of the atmosphere, however, seem to exercise a great influence upon its development, several cases often occurring simultaneously, and having many of the characters of a true epidemic. Sometimes it is the sequel to a tedious or neglected attack of bronchitis, more especially of the capillary form of this disease, in consequence of the extension of a low, inflammatory, or congestive action into the cellular structure of the lungs.

The initiatory symptoms are not always alike, but they are for the most part slight and singularly deficient in all external manifestation of local disease. There is seldom pain in the affected lung; generally, indeed, not the least uneasiness is complained of. Dyspnœa is rarely observed; and the breathing is, in most cases, scarcely if at all affected until the pulmonary disease has made considerable progress. There is generally, however, more or less cough, which, in some instances, has a spasmodic character, and is severely distressing to the patient; yet I have seen a few cases in which this symptom was entirely absent, even at a period when part of the pulmonary structure was unmistakably impervious to air. The expectoration is no less

variable; sometimes it is entirely absent. I have twice seen slight hæmoptysis, and, on three or four occasions, a few brown or rust-coloured sputa; but, in the greater number of cases, and throughout the early stage of each individual case, little else is expectorated than frothy or slightly muco-purulent matter.

The pulse is at first very little affected, seldom exceeding 80 or 90 in the minute; but in this symptom much variety may reasonably be expected. Its frequency and strength, however, are of great use in prognosis, the quickness of the circulation being in constant proportion to the extent and severity of the attack. There is seldom much, and oftentimes no symptomatic fever; but night perspirations are more or less observable.

The patients are, for the most part, singularly unwilling to regard their disease in a serious light, flattering themselves that they are affected merely with dyspepsia, or with such an attack of bronchitis as a little care and a very little physic will easily remove. The early symptoms, indeed, are generally of so slight and almost negative a character, as scarcely to excite the apprehension even of the medical attendant; and nothing but their long persistence, added to a feeling of debility, with a gradual loss of spirits and of health, leads to a suspicion that the malady is, after all, not quite so simple in its nature.

Progress of the disease.—The disease may thus go on for a considerable time without producing any very decided, and no alarming symptoms. Weeks, indeed, may pass away, the patient complaining perhaps of little else than general debility, together with a more or less troublesome cough or slight dyspnœa. Sooner or later, however, less flattering symptoms are too liable to present themselves. The cough becomes severe and spasmodic, breaking the patient's rest by night, and sadly harassing him by day. The dyspnœa is more marked, and unfits the sufferer for all active pursuits. Night perspirations grow more frequent and more copious. Emaciation steadily advances. Hectic fever makes its appearance. The spirits flag; and the hopeful fancies of the patient are exchanged for doubtfulness or despair.

Termination of the disease.—In by far the greater number of cases, the chronic pulmonary disease of which I have been speaking terminates, under timely and judicious treatment, favourably; the formidable symptoms just now enumerated do not occur; the patient gradually regains health and strength; and, although it may be some length of time before the disabled lung thoroughly resumes its proper functions, yet the local mischief is stayed, and finally disappears. Unhappily, however, even under the most careful treatment, the course of the disease is not always so encouraging. The cough, the night-sweats, the loss of flesh, with the consciousness of being and the necessity of acting as an invalid, tell sadly upon both mind and body, and it is only after a long, severe, and doubtful struggle that the malady is dissipated.

When the disease is destined to terminate fatally, it does so in one of three ways. 1st. By so wearing out the sufferer by its obstinacy, as to render him the ready victim to some other malady: or, 2nd. By inducing structural changes in the pulmonary tissues, which lead to a set of symptoms having all the general external characters of phthisis: or, 3rd, By laying the foundation of true pulmonary tuberculosis.

1st. Of the first of these three results I have seen but one example. This was in a patient at the Consumption Hospital, whose disease resisted, for several months, every kind of treatment, and whose health finally gave way, death taking place from an attack of diarrhœa, which, under other circumstances, would, in all probability, not have proved fatal. In this case, the lung was simply contracted and consolidated; there was no softening of its tissue, nor any tubercular complication.

2nd. The second result is, of the three, by far the most common. After a career of a few weeks, or sometimes even of a few months, during which time the general symptoms are progressively advancing, the tissue of the lung softens, and a fatal issue is too evidently threatened. Hectic fever, with profuse expectoration, night-sweats, or diarrhœa, rapidly supervene, and the patient ultimately sinks, exhausted and emaciated, as in ordinary phthisis. I have met with five examples of this kind; in all of which, had it not been for a knowledge of the previous history of the patient, and for the local disease being concentrated at the lower and middle lobes, rather than at the apices of the lungs, I might have mistaken the malady for true tubercular consumption.

Some patients, however, even at this stage of the disease, will struggle on for

months, or perhaps years, the disease passing into *chronic pulmonary abscess*. When this happens, there is usually, from time to time, an expectoration, or rather a discharge of a quantity of fetid muco-purulent matter, which, for a while, relieves the urgency of the symptoms, and flatters the sufferer with hopes of recovery. But the secretion is quickly renewed, to be discharged again at a subsequent period; and the system, at length worn out, succumbs to the disease. I have lately seen this in a lady, who, for upwards of four years, has been in the habit of expectorating, little less than a pint of offensive purulent matter, every six or eight weeks. During the intervals she rallies considerably; but each attack leaves her more exhausted than the previous, and she has, for a long time, been a confirmed invalid. Recovery, however, is not absolutely impossible, even at this advanced and apparently hopeless stage. In a few cases, the health may be restored; the lung gradually contracting, and the pulmonary symptoms disappearing. Such instances, however, are comparatively rare.

3rd. The third result does not exhibit itself, except where the disease is associated with a tuberculous predisposition. Phthisis originating in this way is invariably of a most intractable, and sometimes of a fearfully rapid kind, presenting, in many respects, the character of ACUTE CONSUMPTION. The consolidated portions of the lung seem to undergo tuberculous degeneration, and are found after death to be more or less in the condition known as *tubercular infiltration*. The patients often die before vomicæ have had time to form; but occasionally true tuberculous excavations are abundantly met with.

Pathology of the disease.—In the *first stage* the lung is, probably, in a state of engorgement, and presents the usual and well-known appearances of that condition; as a general rule its volume is unchanged; but in a few cases, it is, I believe, slightly augmented.

In only two instances have I been able to see the local consequences of the malady. In one of these—which may be regarded as representing the disease in its *second stage*—the entire lung was diminished in volume; it crepitated slightly at the apex, but below this the whole tissue was dense, firm, and resisting, appearing, indeed, as if it had been filled up with some albuminous or fibrinous exudation, which had afterwards contracted. The pleural covering was thickened, but no adhesion existed between the visceral and costal membranes.

In the other case—which may be looked upon as an illustration of the disease in its *third stage*—the lung was softened in its structure, of a reddish grey colour, and filled with a dark purulent matter; towards its base were several small cavities separated from each other by soft and easily broken tissue—the lung, in fine, presented the usual appearances of *grey hepatization*.

We might, perhaps, expect in the majority of cases to meet with some evidence of pleuritic inflammation, and, probably, in none should we find that the pleural membrane had not participated in the attack. But I am anxious to draw a broad distinction between the disease I have been describing, and that which is termed *pleuro-pneumonia*, wherein the pleura is prominently, and, in most cases, principally involved. In the chronic form of pneumonia inflammation to which I would direct attention, the pleura appears to be only secondarily affected, and even then to an inconsiderable extent; there being little if any disposition to pleuritic effusion. There may, of course, be an occasional exception; and the one form of disease may even pass into the other; but the great majority of cases which have fallen under my notice have exhibited the peculiarity in a very marked degree. It is probable, from this circumstance, that the disease has both its commencement and its intensity more frequently in the central and lower portions of the lung than towards the circumference or at its very base.

Physical Signs.—Inspection and Mensuration.—At the outset of the attack, and when the lung is simply engorged, there is no change in the form of the affected side: but, with the advance of the disease, and as the lung diminishes in volume, the thoracic parietes become more or less contracted. In cases of long standing, this contraction is usually very striking, manifesting itself even to the eye.

The respiratory movements are affected in proportion to the extent, the stage, and the duration of the pulmonary disease. When this is inconsiderable, or of recent date, it may not be easy to detect any difference in the two sides, except, perhaps, on deep inspiration; but it is impossible for the lung to be much, or for any length

of time, involved, without at once betraying its condition during the respiratory act, in the diminished movement of the costal structures.

Percussion, carefully employed, will generally, even at an early period, indicate both the existence and extent of the local disease. At first, there is simply diminished clearness; but, as the pulmonary consolidation increases, the sound becomes more and more dull, until, at last, it often acquires the quality which has been termed *wooden*. The sense of resistance to the percussion-stroke is also gradually developed, and is, I think, oftentimes more distinctly marked than in any other pathological condition. The *wooden* character of the sound, when fully developed, is very peculiar, and may help to distinguish the disease from pleuritic effusion.

Auscultation.—The respiratory murmur is at first either weak or harsh, or it is suppressed; it is seldom bronchial. The extent and situation of the pulmonary disease must, of course, greatly influence this particular; but weak and harsh breathing, extending for some distance beyond the dullest part of the lung, would seem to be the usual attendant of the early stage of this form of pneumonia. In some cases there is, even for weeks together, an entire absence of secretion sounds; but, more generally, the subcrepitant rhonchus is more or less abundant, and oftentimes so minute as closely to resemble *fine crepitation*. The latter sound, however,—erroneously regarded as the true characteristic of pulmonary inflammation,—is very rarely indeed to be detected. Sibilant and sonorous rhonchi are sometimes heard at different parts of the chest, and are indicative of attendant bronchitis.

There is more or less bronchophony; and the vocal and tussive fremitus are correspondingly developed.

In two fatal cases, I had an opportunity of watching the gradual changes in the auscultatory murmurs, induced by the softening process. The harsh breathing became by degrees bronchial, and subsequently cavernous; whilst the subcrepitant râles, increasing in abundance and size, gradually gave place to the cavernous or gurgling.

Differential diagnosis.—The disease under consideration is unlikely, if not impossible, to be taken for any other except bronchitis or phthisis; and, to distinguish it from either of these, it is only necessary to make a physical examination of the chest.

I was consulted a few weeks back by a healthy-looking farmer, who had been gradually getting thinner, and was troubled with a dry irritable cough. He had neither dyspnœa nor thoracic pain; but his chest had never been thoroughly examined, and all his symptoms were referred to an obstinate bronchitis. On making an examination of the lower lobes of the lungs, I found that on the right side completely dull and impervious to air.

Last year I saw a gentleman supposed to be phthisical. He had long had a severe, dry, spasmodic cough; he had been getting thinner, and was much harassed by profuse night perspirations. The base of the right lung was found, on percussion, to be completely dull and wooden; the respiration was in that part feeble, and marked by abundant fine subcrepitant rhonchi. Here, as in the preceding case, there was no difficulty about the diagnosis, immediately that the physical signs were taken into account. It would not be easy, perhaps, to name another disease wherein the value of a physical examination is more apparent.

Treatment.—If the attack be of recent date, either cupping, or the application of leeches, according to particular circumstances, and to an extent depending upon the patient's strength, will generally be productive of marked relief. In consequence, however, of the insidious nature of the disease, it rarely comes under treatment quite at its onset; and I have found the greater number of cases far more benefited by immediate recourse to counter-irritation.

The choice between the repeated application of blisters and the continued use of an irritating liniment, must be determined by special circumstances. I have generally applied in the first place one or two blisters, trusting afterwards to a strong liniment either of croton oil or iodine, or a combination of both. This treatment should be pursued for some weeks, indeed until some impression be made upon the pulmonary disease. More active counter-irritants, such as issues or setons, are, to say the least, undesirable; they are too exhausting in their effects, are less easily regulated in their action, and cannot so safely be discontinued when their use becomes no longer necessary.

The selection of internal remedies must, likewise, be determined by the particular stage of the disease, and by other special conditions. Where I have either discovered or suspected a tuberculous tendency in the patient, I have never given mercury; but, in the absence of this, and in proportion to the patient's strength, I have found it a valuable agent. I have generally at once administered small and repeated doses, until the gums have been slightly affected, taking care, however, not to carry it to extreme salivation. At the same time, I have prescribed some form of conium or hyoscyamus, combined generally with ipecacuanha or with squill, according to special symptoms. In a few instances, where the pulse has been rather quick, the respiration rather hurried, and the cough troublesome, I have found the addition of antimony of considerable use; but, more generally, depressing agents of this kind, even at the onset of the attack, are rather contra-indicated.

As the disease passes into a more chronic state, continued, but mild mercurial action, in conjunction with a general tonic treatment, should be perseveringly employed. The syrup of the iodide of iron,—the iodide of potassium, with some vegetable tonic,—or one of the mineral acids, with quinine or cinchona, will now become of essential service. To either of these, cod-liver oil may be advantageously united; but if I were required to say from which combination I have seen the most advantage, I should, without hesitation, name that of the iodide of iron with cod-liver oil. But I am far from being the advocate for an exclusive, or even a general use of any such remedy; it is to the class, rather than to the individual, to which I would direct attention. When the patient is anæmic, some of the salts of iron, in conjunction with cod-liver oil, are frequently useful. For children, I have often successfully prescribed a mixture of steel, wine, and cod-liver oil, twice daily; and, at night, small doses of ipecacuanha.

But it would be folly to attempt the treatment of this disease by physic only. In no other malady, perhaps, is the use of hygiene more called for, or more apparent. When the early symptoms have passed away, change of air and scene, and mental relaxation, should, as soon as possible, be resorted to. Pure country air, sea-breezes, healthful and happy associations, freedom from business and care, are now the allies of physic, without whose aid victory must always be uncertain and seldom permanent.

Even at the very earliest period of the disease, the diet should be nourishing. It is impossible to lay down very precise dietetic rules for the first stage of the attack, as everything must depend upon the activity, or otherwise, of some particular symptom, and the physical powers of the patient. Few persons, however, will bear any great reduction of their accustomed fare; and, I am convinced, that very few indeed will be found to require it.

When the disease is stationary, or passing into a more chronic stage, a liberal and nourishing diet is absolutely essential. The addition of wine or of beer is now not merely harmless, but really efficacious. To keep a patient upon a low diet because he may cough or be a little feverish would be, I am satisfied, only to treat the symptoms and to disregard the disease. The observant practitioner will, of course, occasionally see the necessity of departing temporarily from such a practice; but, as a part of the general system of treatment, he must speedily discover its vast importance.—*Med. Times and Gazette, March* 31, *p.* 308, *and April* 14, 1855, *p.* 358.

21.—PNEUMONIA TREATED BY WINE AND AMMONIA.

By Dr. E. A. Parkes, Physician to University College Hospital.

(The patient, a woman aged 24 years, was found to have "double pneumonia of a very bad kind at the base of the heart, both lungs, in fact, hepatised."]

Jan. 10. Her respirations are 78 in a minute! four times the natural amount. Her breath is fœtid; lips discoloured and cold; pulse intermittent and so quick it is impossible to count it. The woman was apparently dying. This was a case, as was well remarked by Dr. Parkes, where bleeding would have made matters worse; and yet where some practitioners would order it. On the contrary, he ordered her brandy, carbonate of ammonia, and wine. In the first twenty-four hours she took ten ounces of wine, thirty-six grains of carbonate of ammonia, and two drachms of

compound spirits of sulphuric ether. No calomel, or tartar emetic. Eighteen hours after admission it was found the pulse could not be counted, it was so quick. The respiration had fallen, however, to 64. She had become drowsy.

11th. Some improvement to-day; pulse is stronger and less intermittent; respiration down to 52. She begins to have expectoration. She spits up large muco-purulent masses, to which Dr. Parkes attaches great importance. She is to go on with her remedies as before. She is in such a state of distress and excessive illness that all sitting up in the bed, and examination with the stethoscope, would be obviously improper. Percussion note dull.

13th. She is still improving. Respiration 42; pulse 128.

14th. Since yesterday the inflammatory exudation in the lungs is softening down very satisfactorily. She is much the same to-day; but the expectoration is scanty. This is the fourth day under wine and stimulants. The pulse is still coming down, as it is only 104; the respiration 38. The physical signs, which we purposely leave out, as they are unsatisfactorily obtained, are improving. Some bronchitic râles, especially about the base of the right lung, are giving way; the left lung, however, is not improving as rapidly as the right. Percussion note still ominously dull; bronchial respiration also very general. With all this manifest improvement, Dr. Parkes directed the attention of his class this day to one point still wrong—there was no increase of the natural excretions. The effete inflammatory matter in the lungs had been obviously taken up into the circulation; but the excretions were still unchanged; the skin dry and without perspiration: urine below the average; bowels rather confined than otherwise; expectoration not so plentiful as it should be. Artificial diarrhœa, he thought, was not at all of use, but helped to throw the patient back. Yet she was precisely in the position in which the old observers—Sydenham, Cullen, &c.—laid such stress on the importance of crisis in pneumonia, and where a natural diarrhœa or sweating would in all probability carry off the entire disease.

18th. Since last report, as was feared, she had fallen back. She has had no "crisis" of any good augury; but, in place of it, an attack of pleuritis. She was at one time so feverish that the question even now arose as to bleeding, and laying aside the stimulants. As there is not much effusion, Dr. Parkes, however, continued the stimulants, which are manifestly agreeing with her.

20th. Pleurisy all gone off. She is quite another woman. Pulse 100, soft and full; respiration 40; appetite improved; exudation into the lungs nearly all absorbed. She now takes small doses of iodide of potassium, and is advancing towards convalescence.

Dr. Parkes draws attention to the fact, that in such cases we shall find the lungs attacked, so to speak, unsymmetrically—one lung attacked *before* the other; and, what is of practical interest, that one lung, perhaps, will get well also long before the other. We may have a state of things which, individually, we often meet in practice in *externe* patients, one lung in a condition over which one would place a blister, and for which we should exhibit stimulants, as ammonia, while the lung of the opposite side bespeaks an opposite treatment, leeches or antiphlogistics; one lung dull and solid, the other with recent crepitus and considerable inflammation, and what Dr. Addison calls "*calor mordax*." In the present case, and many such are met, but, we regret to say, not understood in routine practice, the constitutional disturbance first claims attention; you must disregard the local symptoms altogether for a time. It was a case, indeed, where the infinitesimal system of some quaint practitioners, joined with wine or brandy, would have effected a cure, the latter due solely to the stimulants; while all the usual depressing remedies, in six-ounce mixtures, would do harm; and the blisters and leeches of ordinary routine still further complicate the case. These, in their way, are no doubt invaluable—everything depending on the stage and character of the case in which they are used. We may have, and very often we do have, bronchitis in one lung, or in one portion of a lung, and very marked pneumonia in the other. This case will be still more unsatisfying in its treatment and ultimate result, if the practitioner is not able early with the stethoscope to detect the state of the patient.—*Lancet, Feb.* 3, 1855, *p.* 123.

22.—*Pneumonia treated by Bleeding.* By Dr. Addison, at Guy's Hospital.—[The following case shows the effect of the antiphlogistic treatment of pneumonia. This case, no doubt (although this symptom is not mentioned in the report), would be known to be one of an inflammatory character, not only by the symptoms described, but by the absence of the *brown tongue*, which so remarkably characterizes the typhoid pneumonia of Stokes and Todd. We do not mean a yellowish tongue, but one coloured brown, or brownish, by effused and decomposed blood, which we suspect so peculiarly colours the lips, teeth, and tongue, in cases of blood poisoning. This patient was admitted into the hospital with the following symptoms:]

His breathing is very much oppressed: his pulse quick, and quite of a pneumonic character; he is thirsty and feverish; his expectoration tawny or rust-coloured, viscid, and presenting that intimate mixture of mucus and blood so pathognomonic of pneumonia; he has pain in the chest, and lies on his right side. Dr. Addison ordered him to be bled to twelve ounces; the blood, as we saw it, was peculiarly cupped and buffed. It is curious, as observed at the time, that Louis believed venesection had little influence over pneumonia, other observers trusting to it almost alone; in the present case, the effect was most beneficial. Dr. Addison also ordered him calomel and tartar-emetic every six hours, arrow-root as diet. In fact, we had here the ordinary treatment of pneumonia.

Bleeding, to be of use in pneumonia, perhaps it cannot be too often said, must be tried early, in that first state of engorgement or splenization of the lung, where, however, we unfortunately are so seldom consulted.—*Lancet, Feb.* 3, 1855, *p.* 123.

23.—*On the Absence of Chlorides from the Urine as Diagnostic of the onward Progress of Pneumonia.* By Dr. John Hughes Bennett, F.R.S.E., &c.—[In the Edinburgh Monthly Journal for April, 1854, Dr. Bennett gave an interesting article on this subject. He now relates several good cases, which confirm his own opinions, as well as those of Simon, Beale, and others. After reciting three cases, he says,]

In all these cases the absence of chlorides marked precisely the onward march of the pneumonia, whilst their presence indicated its cessation, and was generally accompanied by the returning crepitation and commencing absorption of the exudation. It still remains to be determined whether the absence of these salts is a cause or a result of exudation into the lungs—whether the interference to the respiratory function, by diminishing the amount of oxygen absorbed, gives rise to those chemical changes in the blood which react on the urinary secretion. If so, what is the nature of these changes? Indeed, a crowd of questions will be suggested to the mind of the physiologist, from the establishment of the remarkable clinical fact of which we are now speaking. That such is an important diagnostic sign I have now no doubt, and it was singularly well tested in the following case, in which there were many signs and symptoms of pneumonia, complicated with heart disease. The question on admission was whether with heart disease and bronchitis pneumonia might not be conjoined, and I was assisted in answering in the negative by the abundance of chlorides which the urine contained.

Case 4.—*Bronchitis and Pulmonary Congestion, from Morbus Cordis, resembling Pneumonia, but no absence of Chlorides in the Urine.* John Dickson, aged 44, pensioner, admitted July 21st, says that on the evening of the 19th he was seized with chilliness, followed by sweating, heat of skin, thirst, impaired appetite, and expectoration of a frothy fluid, resembling liquorice juice. He has for some time felt an uneasy feeling in the epigastrium, which, since his recent illness, has amounted to pain. Yesterday he experienced great dyspnœa and anxiety, symptoms which have continued until now. On admission there is excessive dyspnœa, with expectoration of a tenacious sputum, of a reddish-brown colour. On percussion there is no comparative dulness, but posteriorly the resonance is impaired on both sides. On auscultation anteriorly the expectoration is everywhere much prolonged, and posteriorly there is considerable crepitation with bronchophony. Pulse 92, of good strength. The heart's sounds are entirely masked by the prolonged wheezing expiration and agitation of the chest. He cannot lie on his back or left side, is easily agitated, and fre-

quently experiences palpitations, and cannot sleep. Abundant chlorides in the urine. Other functions normal.

℞. Sp. æther. sulph. ʒ ss., aq. cassiæ ʒ iv.; one tablespoonful to be taken in water occasionally—to have ¼ grain of ant. tart. in solution every two hours.

July 25th. Since last report the dyspnœa has diminished, and the crepitation posteriorly continues, but the wheezing anteriorly is less. Still gelatinous sputum, specked with rusty-coloured blood. The apex of the heart cannot be felt, but a double murmur is now recognizable, accompanying both the first and second sounds—the systolic loudest at the apex, and the diastolic loudest at the base. *Omit the antimony.* July 31st. The pulmonary symptoms and signs have now greatly subsided, whilst the cardiac lesion has become more distinct. For this latter he remained in the house until the commencement of November, when he was dismissed greatly relieved.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 483.

24.—ON TUBERCLE.

By Dr. C. Radcliffe Hall, Physician to the Hospital for Consumption, Torquay.

[In a very interesting original article in the 'British and Foreign Medico-Chirurgical Review' for April, Dr. Hall enters into "the mode of development of tubercle in the lungs in chronic phthisis, its connexion with fatty degeneration of the epithelium, of the air vesicles, and its early manifestation by fatty epithelial cells in the sputa." He says,]

Tracing onwards from lung which has no abnormal appearance to the centre of a large crude tubercle, we find as follows:

1. The pavement epithelium of the air-vesicles is more nebulous.

2. Each epithelial cell becomes enlarged, more cloudy, more prominent when seen in profile, and is studded here and there with oil-dots.

3. The epithelial cells become still larger, and more fatty. In many of them no distinct nucleus can now be made out, but large dots of oil occupy its place. Some of the cells are detached, leaving the wall of the air-vesicle in one part bare, in another coated with compound tubercle cells. These are the preliminary stages of tubercle.

4. We now arrive at the completed tubercle, which consists of compound tubercle cells, small free nuclei in abundance, and granules; and occasionally, in addition, of a few fatty epithelial cells in various stages of disintegration; all being held together by a tough matrix.

5. So far, the deposit has been confined to the interior of the air-vesicles. It now invades the intercellular tissue of the lungs. For the first time, we find tubercle-corpuscles amongst the fine fibres which form the frame-work of the walls and septa of the air-vesicles. These fibres are here and there seen to be broken into lengths, and the entire tissue of the affected lung has become the seat of tubercle. In the first stages, tubercle is only intra-vesicular; at last, it is both intra-vesicular and interstitial. But it is to be remarked, that distinctly cemented in amongst the pulmonic fibres, we never find any of the compound tubercle cells, but only the free nuclei and granules.

The steps of the local morbid process, then, appear to be these:—Fatty degeneration of previously normal epithelium: shedding of this; its replacement by fresh epithelium, degenerate from the first, and rapidly becoming fatty; shedding of this; its replacement by large cells containing several nuclei; shedding of these; their replacement by free nuclei and granules, embedded in a structureless matrix. Up to this stage the tubercle is intra-vesicular only. The pulmonic fibres are next enclosed and separated by the morbid exudation, and free nuclei and granules are formed between and amongst them. The tubercle is now complete.

Tuberculization of the lungs thus commences as a degeneration of a normal tissue; proceeds as a production of this tissue in a depraved form; next, as an exudation capable of following only the lowliest process of organization up to maturity. Its ulterior changes from maturity, also, are those of the degeneration of a lowly-organized product; but concerning these I have dealt elsewhere.

The structural representatives of these several stages, starting from normal epithelium, are the following, and may be considered in their natural order. They

may not all be detectible at once in every mature tubercle, particularly if softening be commencing; but they are to be found in various proportions in the great majority of tubercles, provided we include along with the tubercle a certain portion of the pulmonary tissue around it.

Constant constituents of mature pulmonary tubercle at some period of its course:

1. Normal epithelial cells becoming fatty.
2. Fatty epithelial cells.
3. Many nucleated cells.
4. Free nuclei.
5. Granules.
6. Matrix.

Frequent constituents:

7. Small bloodvessels in a state of fatty degeneration.
8. Red blood-corpuscles, and orange-brown pigment.
9. Black pigment.
10. Granule cells, and glomeruli.
11. Induration matter.

[Dr. Hall enters briefly but well into each of these subjects and illustrates his paper by numerous interesting wood engravings. We would strongly recommend the reader to peruse the original article, if he wants a bird's-eye view of modern opinions on this subject, illustrated by the microscope.

The following remarks on the *Sputa*, &c., in impending, incipient, and established phthisis will interest the reader.]

Common to all.—Flat pavement epithelium, with sharp clear nucleolated nucleus, entire or partially dissolved, from mouth or pharynx. Bronchial columnar epithelium. Filmy cells, containing two or three dim granulous nuclei from tonsils. Pigment cells.

Impending Tuberculization of Lungs.—Expectoration grey, glary, adhesive; brought up chiefly when dressing in the morning, and scarcely noticed or thought of by the patient. If innocent (which of course it generally is), it consists merely of round nebulous mucous corpuscles, in transparent adhesive mucus. If threatening, contains, in addition, single plates, or small flakes of flat epithelium from the air-vesicles, fattily degenerating; and bronchial columnar epithelium, also presenting various degrees of fatty degeneration.

Incipient Tuberculization.—The same as the above; and, in addition, red blood-globules, more or less shrivelled and faded, enveloped in a filmy cell; a few large many-nucleated cells; granules; and frequently small casts of air-vesicles and the ultimate bronchi, in which are plainly visible epithelial cells in various sizes and various stages of fatty degeneration. So constantly have I met with enveloped blood-corpuscles in sputum which betrayed no appearance of blood to the naked eye, that I suspect that this microscopic hæmoptysis is seldom, perhaps never, altogether absent in those cases of commencing phthisis in which the more obvious expectoration of blood is wanting.

Established Phthisis.—The same as the last, largely mixed up with corpuscles of pus and mucus. Free tubercle-nuclei occasionally in small quantity.

Large Suppurating Cavities.—Absence of specific tubercle-cells, excepting portions of opaque tubercle itself happen to be expectorated. Habitually, little else than pus and mucus, and large granulous cells, unless confervoid vegetations be superadded, which is not unfrequent.

The microscopic appearances presented by the expectoration are most special and characteristic in the early stage, when other physical evidence is least marked; and may be re-enumerated as consisting of fatty epithelial cells, single or in patches, fatty bronchial cylinder epithelium, casts of air-vesicles, lined with fatty epithelium, and microscopic hæmoptysis—i.e., enveloped red blood-globules. These are enough to indicate that consumption is threatened. When the many-nucleated cells and special free nuclei of tubercle are superadded, no doubt can remain. All these I have repeatedly found in cases where the patient was not apprehensive of phthisis, but complained only of general malaise or dyspepsia, as well as in other cases in which consumption was feared, but was not supposed to be yet in active existence.

Theory of the Local Part of the Disease.—The natural defensive epithelial coat of the air-vesicles is unable to maintain its own nutrition, and slowly falls into a state

of fatty degeneration, It is then thrown off, and is succeeded by fresh epithelium, which is badly organized from the first, and quickly becomes degenerate and shed. This process of fatty degeneration and shedding may, perhaps, go on for some considerable time without further local disease; and we do not know but that it may happen in occasional instances, where the persons affected have their general nutrition by some means so much improved at this juncture, that good epithelium is once more laid down. This is quite conjectural; but if it ever do occur, such persons have been on the eve of becoming tuberculized, but have escaped. Ordinarily, after an uncertain number of desquamations of fatty epithelial cells, the surface of the air-vesicle forms cells which contain separate granule-bearing nuclei. In due course these also are shed, and probably some may grow larger after their shedding. A variable number of crops of these compound corpuscles may succeed each other, according as the wall of the air-vesicle maintains, for a longer or shorter period, sufficient organizing power to make them. The wall eventually becomes bare in places, and the albuminous part of the *liquor sanguinis* has no longer anything to hinder it from escaping into the air-vesicle. It does so escape; coagulates and corpusculates into such small, compact, and for the most part non-nucleated cells, as its feeble organic force permits; and these are the free nuclei, or tubercle-corpuscles proper. That portion of excluded plasma which is unable to perform even this feeble act of development, falls into amorphous granules. The adjoining air-vesicles, under a continuance of this process, are crammed with desquamated cells, and with tuberculous exudation. By the resultant pressure, the remnant of vitality which the enclosed intercellular pulmonary tissue (viz., the septa and walls of the air-vesicles) retained, is still more impaired; exudation infiltrates amongst the pulmonic fibres, and corpusculates in their interstices into the free nuclei, but not into the many-nucleated cells. The tubercle is now complete. Up to this time it may have been unattended by inflammation, or inflammation may have attended its course throughout.

The relation of inflammation to tubercle is far too important to be cursorily dismissed, and is discussed at length elsewhere. I will merely state dogmatically here, that tubercle *may* pass through all its stages *except the destructive ones*, without the coincidence of anything which we can justly term inflammation. But on the other hand, it seldom does. Tubercle may commence with inflammation, or be quickly followed by it. Inflammation, in short, may step in at any stage, and almost invariably does so at some stage, and is a necessary antecedent to the fatal issue of softened tubercle. Inflammation practically, therefore, exercises the greatest influence upon the course and termination of tubercle.

If we choose to say that when pulmonary tubercle commences with pneumonia it has an inflammatory origin, there is no refutation at hand. But as tubercle is not *always* of inflammatory origin, it cannot be essentially an inflammation. It is rather always a degeneration, which has a very habitual relationship to inflammation as a concomitant.

The exudation poured out by inflamed blood into inflamed tissue either fibrillates into induration-matter, or corpusculates into cells, for the most part round and nucleated, which becomes ultimately organized into tissue, or dissolved and absorbed, or changed into pus cells, according to circumstances; or else, the inflammatory blastema dies rapidly and at once. Tuberculous blastema does none of these after precisely the same type. And when tuberculous exudation is attended with inflammation, we find side by side the normal products of inflamed blood, and the specific products of tuberculous blood. The two are not mutually convertible. It follows that either that part of the blood plasma which forms tubercle *cannot* undergo the changes of inflammation; or else, that when some of the blood-plasma is inflamed another portion escapes the process, and is exuded in company with the first, to pursue afterwards a distinct course. In either view, the distinction between tubercle and all the known products of inflammation is obvious. But it may be said, that tuberculous inflammation may be of a specific kind, leading to the specific product of tubercle. Not so: for we find the inflammation which attends tubercle is just like common inflammation in its course, and in its products. If, again, it be urged that part of the process may be common inflammation, and specific inflammation, proceeding side by side in the same portion of tissue, this seems to be almost a *reductio ad absurdum*, since upon such terms every known disease might be called an inflammation.

Natural Alliances of Pulmonary Tubercle.—Viewed in its twofold aspect, as primarily a degeneration of existing structure, and secondarily an exudation which corpusculates into very lowly-organized cells, tuberculization of the lungs has a generic relation with similar morbid affections of many other organs. And such allied affection of any other viscus, when it happens to coincide with pulmonary tubercle, may be looked upon as equally con-generic, whether it stop short at the stage of fatty degeneration, or pass on to that of tubercular formation. Hence the frequent association of phthisis with fatty liver and fatty heart, and its occasional association with the fatty variety of Bright's disease. The occasional occurrence of this affection of the kidney; the more rare occurrence of apoplexy from fatty degeneration of the small bloodvessels of the brain in persons of phthisical habit, but who are not the subjects of developed phthisis; and also the antecedence of general fatness of integument, the fat being less firm than that which indicates mere excess of good nutrition in some persons who afterwards become phthisical, may point in the same direction. And several of the not unusual complications of phthisis, as well as some of its customary features, are possibly due to a similar kind of degenerate tendency. Thus, the thinning of the coats of the stomach which occasions the enlargement of that viscus in phthisis, though chiefly owing to simple atrophy of the muscular coats, may be in part also due to fatty degeneration. The hooked and clubbed nails, to fatty degeneration with enlargement of one portion of the bed of the nail. Scrofulous lymphatic glands, to enlargement, fatty degeneration, and accumulation of lymph-cells; or to positive tubercle: or to both, in different instances; inflammation being either coincident or absent. In like manner, the falling off of the hair, so common in females who are phthisical (as well as in many others whose mal-nutrition arises from more innocent causes), may greatly depend on fatty degeneration of the cells secreted by the hair-bulbs unfitting them for development after the existing hair is shed.

Upon these latter points (with the exception of the lymphatic glands), I have no certain knowledge: but reverting to the three instances in which the alliance is clear and sufficiently common—viz., fatty degeneration of heart, liver, and kidney, there is more to be said.

Muscle is not liable to tubercle. We have, therefore, no difficulty in admitting that fatty atrophy of the heart, when it occurs in phthisis, may be directly related to the tuberculous disease, although tubercle itself is not found in the organ. The degree in which the liver and the kidney are prone to become the seat of tubercle is not quite established. All that can be affirmed is, that whilst they are occasionally tuberculized, they are much more liable to be merely affected with fatty degeneration. When so affected in conjunction with tuberculization of the lungs, we need not hesitate to admit a direct relationship between the several organs diseased, in regard to the very essence of the disease. In the fatty form of Bright's disease, the similarity to the primary steps of chronic tuberculization of the lungs is particularly close, keeping in view the normal peculiarities of the two great excretory organs involved.

The lung is a chimney, which requires mere ventilation for carrying out its vaporous excretions. The kidney is a main sewer, always draining and periodically flushed. The internal surface of the lung keeps in all the blood except that portion which evaporates during its passage through the pulmonic capillaries. When, therefore, this surface has lost its natural defence, it does not instantly give exit to complete blood-plasma in excretable quantity. If it does so, there is often a large pouring forth, the blood escapes entire, red globules as well as plasma, and we have hæmoptysis. But in the kidney the internal surface is normally accustomed to let out the watery part of the blood, and so, when bared of epithelium in places, it readily gives passage to a portion of fluid plasma, with or without some red corpuscles, and we have albuminous urine, with or without hæmaturia. In the lung, tubercle follows; as a rule, it does not in the kidney, perhaps for these reasons:—So long as the plasma is periodically washed away with the urine, there is no material to corpusculate left behind; and when checked elimination of urine at length ensues, its consequences generally put an end to life too speedily to permit the question to be tested, not to mention the altered blood-crasis then induced. Moreover, when tubercle *is* laid down in the kidney, its seat of election is the cortical parenchyma, and not the interior of the uriniferous tubes. And although the exact relation of tuber-

culous deposit to the minute structure of the cortex is not clearly ascertained at present, it is quite possible that the free discharge of albumen from one portion of the gland may oppose the formation of tubercle in the other.

As in chronic phthisis the earliest local process consists in the enlargement and fatty degeneration of epithelial cells, and more or less accumulation of these in the air-vesicles; so also, in some cases in which, had the patient lived, he would probably have shown the customary signs of the fatty variety of morbus Brightii, do we find the uriniferous tubes so choked up with detached granulous and fatty epithelium, that the urine only filtrates through, and carries no albumen with it. Such a condition we shall generally find after death, when, in the last stage of phthisis, there is œdema of the lower extremities (irremovable by remedies), without any albumen in the urine, but with a copious sediment of phosphates.

In practice we notice, in respect to this subject, three kinds of case: the one just mentioned; phthisis with albuminous urine; and phthisis arrested, in which the patient dies at some future time of Bright's disease.

Is, then, in conclusion, tuberculization merely an advanced stage of fatty degeneration, when this affects certain textures? Certainly not, since fatty degeneration is related to many other morbid conditions besides tubercle, and having little in common with it. But it may be, that it is a constant attendant upon tubercle; in which case we have a right to consider tuberculization as fatty degeneration *plus*—something else. If so, all that is known respecting fatty degeneration has a certain reference to the etiology and treatment of phthisis.

Not to occupy space by compiling information easily obtained from the writings of Mr. Bowman, Dr. Quain, Mr. Paget, and Dr. Handfield Jones, I will merely state here that I have succeeded on a large scale in inducing fatty degeneration of various viscera by keeping animals under such conditions as entailed mere inactive discharge of the functions of these viscera.

Neither is tubercle in the lungs the necessary consequence of shedding the epithelial cells in the air-vesicles, and of subsequent exudation of plasma; for this occurs to some extent in simple pneumonia, and to a much larger extent in the intensely-congested lungs after section or ligature of both paria vaga at the same time. Here the deep, forcible, and very slow and infrequent inspirations, and the congestion of the pulmonic bloodvessels, acting as pressure alternating in opposite directions, cause the serum to transude and wash off the epithelium of the air-vesicles. The serum is churned into a spumous froth; hemorrhage follows in a degree which varies from mere reddening of the foam to pulmonary apoplexy. Inflammation and suppuration may afterwards ensue, but no tubercle. As the animal seldom lives long, how can we know this? If a few days be allowed to intervene between the injury of the two nerves, the animal will sometimes live. Birds will generally survive under any circumstances. Killed some months afterwards, we are able to trace in these animals the morbid actions which ensued in the lungs, and we find no trace of tubercle.

Neither fatty degeneration alone, nor exuviation of epithelium alone, nor mere exudation alone, is enough to constitute tubercle. The exudation must ever be at the first one of tuberculous blood-plasma.

But, in point of fact, as regards the lungs, are these three morbid processes ever conjoined in sequence, unless the blood be tuberculous? Does it ever happen that the epithelium of the air-vesicles degenerates fattily in considerable quantity, and is shed; and that this is succeeded by exudation into the air-vesicles, except in cases of tubercular phthisis? To this question, I am not at present prepared to furnish a positive reply.—*Brit. & For. Med.-Chir. Review, April*, 1855, *p.* 479.

25.—*Remedies in Phthisis.*—[In a visit to the Brompton Hospital the writer notices some of the remedies which are there used. He says:]

Among a few of the medical novelties we noticed, and which are found useful at Brompton, are divers forms of inhalations: one an inhalation of hydrocyanic acid vapours, of course in small quantities; inhalations of chlorine and inhalations of the chemical substance chloroformyl, inhalations also found very useful of conium and creasote.

A mixture of tannin, nitric acid, and lupulin is very valuable in the *night sweats* of the worst cases; the oxide of zinc also is found to answer wonderfully in some cases which will even resist acids and tannin; as practical facts not generally known, these valuable results cannot be too generally made public.

A mixture of decoct. hæmatoxyli and aqua calcis, not very chemical but very valuable, is used in *diarrhœa*. A solution of iodine as a caustic, now general in hospitals, is also found most useful for painting the chest; it is simply one part each of iodine and the iodide of potassium and two of rectified spirit. Dr. Quain also finds a solution of nitrate of silver, to act almost "like a charm," when applied in some cases with a probang to the top of the larynx and to the vocal chords.—*Med. Circular, July 5*, 1854, *p.* 5.

26.—*Treatment of Irritable Stomach in Phthisis.*—Several very pleasing cases, illustrative of the good effects of hydrocyanic acid and bismuth in the treatment of irritable stomach in phthisis, have recently been under observation among the out-patients at the City of London Hospital for Diseases of the Chest. The patients had been long subject to nausea and attacks of vomiting, and were quite unable to retain cod-liver oil on the stomach, very often rejecting also ordinary food. The mixture prescribed consisted of 3 minims of the hydrocyanic acid and 10 grains of the trisnitrate of bismuth made into a draught with mucilage and green mint-water, and taken thrice daily. By its aid several patients have got quite rid of their troublesome stomach symptoms, and are now taking the oil easily and with benefit. The class of cases best suited for this treatment are those in which the tongue is generally clean, and in which the disease is undue irritability rather than disordered function. The cases alluded to have been under the care of Dr. Peacock and Dr. Risdon Bennett.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 614.

27.—*Laryngitis treated with Nitrate of Silver.*—Dr. Ebert (*Annalen der Berlin Char. Krankheiten*, 1854), employs inhalations of nitrate of silver in substance, with great benefit in all inflammations of the laryngeal mucous membrane. He has employed the nitrate of silver also in solution after the manner of Green, but has never been able to satisfy himself that the larynx was really entered. The mode in which the solid caustic is introduced is as follows:—Three grains of the nitrate are mixed with one drachm of sugar; the powder is placed in a steel pen, which is itself firmly inserted in a quill open at both ends, the little apparatus is then put into the mouth, so that the end of the steel pen shall be on the root of the tongue, then the lips are closed round the quill, and the patient respires forcibly. The first attempt is almost always a failure, and the nitrate is only tasted on the root of the tongue; but the patient soon learns to manage it very well; a little cough and irritation follow, but no uneasiness. For young children this method does not answer, and a spinal apparatus must be used.—*Med. Circular.*—*Dub. Hospital Gazette, Nov.* 15, 1854, *p.* 314.

28.—*On the Use of Iodine in Phthisis.*—The following remarks occur in a paper by Dr. Barnston upon the Parisian hospitals, contained in the 'Montreal Monthly Journal of Medicine and Surgery;' it refers to the practice of M. Piorry in phthisis:—"A patient arrives in the ward; he is introduced to M. Piorry, who immediately subjects him to a lengthened examination, in order to ascertain the physical qualities of every organ in his body. Having satisfied himself that there is a circumscribed induration in the upper part of both lungs, this is carefully marked out with a pencil on a piece of canvas, with various shades, indicating the amount of condensation. A plan of treatment, sanctioned by long experience as most trustworthy, is now adopted, and in such cases as the present, where the consolidation is assumed to be tubercular, iodine is the all-powerful remedy. It is accordingly administered in every possible way: for example, it is given—1st. Internally in the form of solution, and if the stomach is not well able to bear it, after some time, iodine enemata are substituted. 2nd. In the form of vapour, by inhalation, which is resorted to

once to three times in the day. 3rd. In form of ointment, by friction upon the cutaneous surface adjacent to the diseased parts. 4th. By means of iodine baths; and lastly, the whole atmosphere is impregnated with the atoms of iodine, by which a constant supply is furnished to act directly upon the tubercular disposition. The patient is thus subjected to treatment, and every successive examination, made with the same minuteness and precision, at intervals of two or three days, shows a slight, but yet distinct and satisfactory diminution in the circumference of the induration, till at length the lapse of time intimates a complete disappearance of the dulness, and return of the lung to its normal condition.—*Dub. Hosp. Gazette, Nov.* 15, 1854, *p.* 314.

29.—EMPYEMA TREATED BY INJECTION OF IODINE.

By JOHN WINDSOR, Esq., Senior Surgeon to the Manchester Eye Hospital.

[Mr. Windsor being satisfied in this case that fluid existed in the chest, passed a grooved exploring needle along the upper surface of the seventh rib. About 52 ounces of puriform fluid were thus withdrawn, and plaster and bandage were then applied. In about six weeks, however, the man relapsed for the third time.]

On this day (May 30th) I operated for the third time, and drew off from near the same part of the chest two pints and a half, or forty ounces, of sero-purulent fluid, but certainly less puriform than on the two former occasions. Immediately afterwards I injected five ounces of the following solution:

℞. Iodinii 3i.; potassii iodidii 3ij.; spiritus rectificati ℥j.; aquæ puræ ℥v. M.

After retaining it in the chest a short time, and moving him a little about to bring the injection into contact with the pleural membrane, I withdrew my thumb from the canula, when about an ounce of the injection returned through it, the rest remained in the cavity. On this occasion I was assisted by my son and two pupils; and another gentleman was also present.

He bore the operation again very well, but a troublesome dry shaking cough almost immediately succeeded, as after the first thoracentesis; it was, however, again soon relieved by taking a little warm tea, and he remained pretty easy the rest of the day.

May 31st. The pulse was 96; the tongue was whitish, and he complained of an unpleasant taste or sensation about his mouth and throat; his appetite was indifferent; the cough was troublesome. He had not much sleep last night. The bowels had not been moved since the previous morning. He complained of more uneasiness in the chest, and of a sort of general soreness over the left side, probably referrible to the action of the iodine injection. He lay at present most easily on his back, or slightly towards the left side. A large quantity of urine, about four or five pints, was passed in the afternoon after the operation, as happened somewhat similarly after the two former tappings. He was directed to continue the mixture, and occasionally the pills.

June 1st. He felt better this morning. The pulse was 84, the soreness of the chest was going off, and he could lie on either side, but neither his sleep nor his appetite were yet good.

June 7th. He now had begun to go out of doors, and walked a good deal daily, of which exercise he was very fond. The pulse was 92; the bowels were regular; the unpleasant taste of the mouth was gone; the appetite and sleep were better; the cough still continued, with occasionally a little mucous expectoration.

As stated above, he had a disagreeable taste in his mouth for some days after the operation, probably from the presence of iodine in the secretions. It may be added, also, that on testing the urine on the third day after the operation with liquid starch and sulphuric acid, it still afforded a distinct violet tinge.

Auscultation and percussion afforded nothing new, except that there was a very resonant space between the left nipple and the sternum.

June 15th. Since last report he had continued going on well. He now lay quite as easily on the right side as on the left side; he walked out a good deal, almost daily. He had still some cough, for which he was prescribed a linctus, with morphiæ murias; and was ordered to continue his former medicine.

June 20th. His cough was now less frequent and easier, with a little mucous ex-

pectoration, and his breathing was pretty easy, under exercise out of doors; he had no pain, and scarcely any feeling of soreness of the left side, and he could lie in any position. The feeling of "swilking," as he called it, or gravitation of fluid from one side to the other, which he used to experience in turning in bed, had now gone off.

July 1st. He said that he now felt free from complaint, having no cough, pain, or soreness of the chest. He purposed going into Cheshire for about a fortnight.

On examining the chest, I found the heart beating now most distinctly nearly in its natural situation, or just to the right of the left nipple. On desiring him to breathe rather deeply, the left lung seemed to admit air nearly throughout, although the respiratory sound was not so loud as on the other side. The resonance on percussion also approximated to that on the right, but the percussion was dullish in the situation of the heart, where it had lately been resonant. The intercostal spaces had become about as distinct as on the other side.

July 20th. He called upon me after his return from the country, and said that, feeling well, he was about resuming his occupation.

Sept. 9th. I saw him again, and found him pretty well, and pursuing his occupation. There was yet little, if any, depression or contraction of the affected side; and he indeed complained of nothing, except a little weakness or aching about his shoulders occasionally, probably ascribable, at least in part, to the laborious nature of his employment.

Remarks.—In the preceding case, which seems to have terminated so satisfactorily, I was induced on the third time of tapping him to add the use of an iodine injection, in consequence of reading a paper (pointed out to me by my son), published by Dr. F. A. Aran, physician to the La Pitié at Paris, in the 'Union Médicale' (Nos. 97, 98, and 99), 1853, and also inserted in the 'Encyclographie des Sciences Médicales for October, 1853. Dr. Aran recapitulates in part what has been done, especially of late times, in regard to injection of the pleural cavity after thoracentesis in cases of effusion in consequence of acute or chronic pleurisy. In such cases, as Dr. Aran remarks, patients sink under exhausting hectic fever, as in phthisis pulmonalis; or, from the formation of a pleuro-bronchial fistula, suffocation may occur by too great an accumulation of blood in the bronchial passages: or sometimes, especially perhaps if air be admitted, from the fluid becoming vitiated, and thus poisoning, as it were, the blood. In some of these cases of fluid, generally sero-purulent accumulation in the pleural cavity, it finds an exit by a sort of abscess forming in the parietes of the chest, as occurred in the case of Morris, published in my former paper in the fifth volume of the 'Transactions' of this Association. Thus the pleural effusion may, when not re-absorbed spontaneously, be sometimes discharged by communication with the corresponding lung and bronchial tubes; at others, by a communication with the external parietes of the thorax; or, indeed, by a combination of both these modes. Of this last form, apparently, I attended a case this year in consultation with Mr. Robb. It occurred in a Mrs. Kitchen, Chester Road, Hulme, aged about 60, and the mother of a large family. Mr. Robb had attended her a considerable time before I saw her. Besides expectorating a good deal of puriform fluid, occasionally tinged with blood, subsequent to an attack of pleuropneumonia, probably of the right side, two abscesses formed soon after I saw her, on the lower part of the right side, one a few weeks after the other. They were both opened, and, especially the first one, gave exit to a considerable quantity of pus, increased during expiration and coughing. She became gradually exhausted, and did not long survive the formation of a second abscess. Unfortunately a *post mortem* examination could not be obtained. In this case there was great dulness on percussion over a considerable portion of the right side, and only slight respiratory murmur could be heard anteriorly at the upper part of the chest, in the subclavicular space, and posteriorly near the spine.

Dr. Aran cites two cases of thoracentesis for *acute* (purulent) pleurisy by Dr. Trousseau, but both terminated fatally at last, which was not remarkable, as they were complicated with other serious affections. Dr. Trousseau had also operated four times for *chronic* purulent pleurisies, but all terminated fatally at no distant period.

From the pyogenic disposition of the pleural membrane, and therefore the great disposition to relapse after thoracentesis, it becomes a question, and a very momentous one in preventing a fatal termination, so common in this disease, how far we may be able to counteract a recurrence of the effusion by the use of injections,

and most effectively perhaps by the use of iodine ones. M. Boinet has referred to three cases where they have been employed, once by himself, once by M. Boudant, and once by M. Massiani. In the last case a cure was effected in three months, but with considerable deformation of the chest ensuing, along with depression of the corresponding shoulder, and lateral inclination of the vertebral column. It is well known since the researches of Laennec, that this change may follow a natural cure, and was indeed referred to in my former paper, as occurring in Mr. Burton's patient, Mr. Liversedge the artist. In M. Boudant's case, a cure was effected in six months, but with some sinking of the ribs, and slight deviation of the sternum. In M. Boinet's case, a cure was effected in four months, but with flattening also of the ribs. In all these three cases air was admitted into the chest, which probably modified injuriously the character of the fluid. If the admission of air be prevented, either totally, or almost so, by making a valvular opening, as I did in each of the three operations in the case related above, a speedier cure may be expected, with less subsequent contraction of the chest, probably from the readier adhesion of the opposing membranes, and consequent obliteration of the pleural cavity.

In two cases operated on by Dr. Aran also in this way, one of acute, and the other of chronic pleurisy, the former terminated fatally by the supervention of subacute peritonitis; but the latter, occurring in a locksmith, aged 24, and of fifteen to eighteen months' duration, previous to treatment, terminated successfully after a single iodine injection.

When we consider the result of the treatment of these six cases of pleuritic effusion by iodine injection, five out of the six being successful, and that the other would not improbably have been so also, had it not become complicated with another affection,—it must, I think, be conceded that it is one offering considerable advantages.

I am not aware of any other case besides the one related in this paper having been yet treated similarly in this country; but from the satisfactory result in this one instance, conjoined to those of a few similarly treated abroad, I think we are encouraged to give it a further trial in cases apparently adapted for it. Such cases seem to be those of chronic uncomplicated pleuritic effusion. Similar success could scarcely be expected when this empyema is the result of a vomica or vomicæ bursting into the pleural cavity, or where the effused fluid has been suffered to remain so long as to form external communication through the parietes of the chest, or where both these states are coexistent. Of all these forms and complications, I doubt not, most have seen examples; and instances of them are referred to both in this and in the former paper. However, even in such circumstances, where a permanent cure is unattainable, great temporary relief to the oppression and dyspnœa is often afforded by the operation of thoracentesis. The process itself of withdrawing the fluid by the use of the trocar and canula alone, without any previous incision or dissection, is one of extreme simplicity; and, though I have now performed it many times, no unpleasant accident has ever occurred.—*Association Med. Journal, Oct.* 20, 1854, *p.* 944.

30.—*Iodine Injection in Pleuritic Effusion.* By Dr. Atlee.—Dr. Atlee relates the case of a gentleman (age not given) from whose side nine pints of pus were discharged on the 16th December, through a trocar passed into the left side of the chest between the seventh and eighth ribs, midway between the spine and the sternum. No further discharge took place until the 2nd of January, but the closed wound then reopened, and from that time about a pint of pus continued to be discharged daily, with the effect, in spite of restoratives, of rapidly wasting the patient's strength. On February 2nd, 3 ½ of liq. iodin. c. (U. S. Disp.) diluted with ℥i. of tepid water, was thrown in without inducing any ill effect, no precaution against the admission of air being taken; and next day the amount of discharge was diminished by one-half. Each day the strength of injection was increased, and on the 7th February the iodine was used undiluted. The amount of matter discharged rapidly decreased, and by the end of February the flow had ceased, the patient daily recovering strength, and in a few weeks later being able to go to business. When seen three years later he was quite well, and had insured his life. On examination the left side of the chest was found measuring 2½ inches less round

than the right. The respiration was normal, and the heart's action was audible in the natural locality.

During the discussion upon this paper, Dr. Bowditch related an interesting case which occurred in the person of a young woman, who, during a three months' treatment, had her chest tapped seven times. A steady improvement and a diminution in the size of the collection ensuing each time, and the intervals between the operations being gradually increased. During all this time pregnancy steadily advanced, and the fœtus was vigorous in its movements. At first a large trocar was employed, so that by leaving the canula *in situ* a fistulous opening might be produced; but it became displaced, and caused much more irritation than the small one employed on subsequent occasions. Dr. Bowditch observed that during the past three years he has operated with the small trocar seventy-seven times, and in no instance with ill effect. In all the cases when fluid was obtained relief followed; and in some the operation was the chief or sole cause of cure.—*Amer. Journal Med. Science.—Med. Times and Gazette, April* 14, 1855, *p.* 371.

31.—*Treatment of Catarrh.*—[Dr. Lombard, of Geneva, suggests that catarrh should be treated as follows:]—A piece of metal is heated in a spirit lamp, and a few grains of powdered opium having been sprinkled upon it, the patient is directed to hold his head in the fumes, and to make a few forced inhalations. It is said to afford most marvellous and speedy relief to the distressing pain and sense of weight so commonly felt in the frontal sinuses. As addressed to this particular symptom, the expedient might be advantageously combined with "orange-juice treatment," so useful in allaying the fever, restlessness, and general disturbance which often attend this common and most disagreeable complaint.—*Med. Times and Gazette, March* 31, 1855, *p.* 310.

32.—ON COD-LIVER OIL.

By Dr. E. Headlam Greenhow, Physician to the Blenheim Street Dispensary.

Of the several kinds of cod-liver oil in use, the pale is that which is best borne by the stomach, and, on account of its comparative freeness from odour and taste, least objected to by the patients. In my experience, the darker oils much more frequently nauseate, and almost invariably, after a time, induce gastric disturbance of such a character as obliges the discontinuance of the remedy. A few years ago, I very often found the oil to disagree, and was in consequence induced carefully to examine the several kinds used by my patients. Of these, many were either not cod-liver oil at all, or this was mixed with seal or some other oil of a cheap kind. Sulphuric acid is a satisfactory test of the genuineness and purity of cod-liver oil. A small quantity of the oil to be examined being placed upon a white tile, or a piece of clean writing paper, two or three drops of sulphuric acid on being dropped into the centre instantaneously occasion a number of radiating currents of a delicate and beautiful lilac hue; the genuineness and purity of the oil is in exact relation to the richness and persistency of this colour, and the rapidity with which it is produced. In very rich oils the lilac colour, after a few seconds, occupies nearly the whole disk of oil, excepting the small portion directly in contact with the acid, which, although it displays the same tinge for an instant, very speedily turns of a deep purple-brown from the charring of the oil. A careful trial of seal oil, train oil, sperm oil, and the several vegetable oils in common use with this test, has in no instance given a reaction which could be mistaken for that of cod-liver oil, the colour in these cases being entirely free from purple or violet tinge, and varying in the several kinds from ash to very dark brown. Skate-liver oil, for a specimen of which I am indebted to my friend Dr. Hassall, cannot readily be mistaken for cod-liver oil, its colour being more yellow, approaching to a very bright amber; its reaction with sulphuric acid very nearly resembles that of some inferior kinds of cod-liver oil, but is rather of a reddish-purple than lilac colour, and the effect is more confined to the portion of the disc immediately touched by the acid. Ray-liver oil, which is analogous to skate-

liver oil, is used in Germany indiscriminately with cod-liver oil; and although the specimen of skate-liver oil I have examined is less pure, and, from being thicker, more nauseous than cod-liver oil, it is possible that if manufactured with the same care, it would prove equally efficacious.

Several years having elapsed since these experiments, I have carefully repeated them for the purpose of this paper. I have also tested the value of sulphuric acid as a test for cod-liver oil, by endeavouring to manufacture a spurious oil from pure seal oil, combined in various ways with iodine, iodide of potassium, and ox-gall, both separately and in various degrees of combination. Specimens of cod-liver oil, manufactured in London, in Newfoundland, and imported from abroad, have likewise been carefully tested, with a view of ascertaining their comparative value. These experiments have been repeated many times with great care, and as the quantity of oil and acid in each was as nearly the same as possible, the result may be considered as satisfactory. As the subject is possessed of considerable interest, I shall briefly state the nature of the trial and the result in several of these experiments, premising that each was tried repeatedly, both on the same and different days, and that notes of the result were taken on the instant.

1. The first trial was with cod-liver oil itself, two specimens of which, roughly disintegrated, on being submitted to the test, afforded the peculiar lilac reaction of cod-liver oil, but, as might be expected, in a less intense manner. One of these specimens had been more than twenty-four hours out of the fish prior to its examination; and as, on a microscopic investigation, there was no trace of granular or secreting cells, the entire substance appearing to consist of fibrous tissue containing innumerable globules and droplets of oil of various sizes profusely distributed throughout it, and possessing the brilliancy and regularity of milk globules, which they closely resembled; a second liver was procured from a fish immediately after death, and submitted to examination within three hours. This gave the same reaction with sulphuric acid as the former one, but required a more perfect disintegration. On examination with the microscope, the secreting cells were found to be so distended with oil as to cause a slight irregularity in their outline; the cell walls were extremely delicate, and yielded their contents freely on the earliest approach of putrefaction, to which the liver is peculiarly prone. The fresh liver is extremely pale-coloured and tolerably firm, and the oil, which it contains in great abundance at this season of the year, appears perfectly pellucid and pale-coloured. After the fish has been a short time dead, the liver loses its firmness, and such changes take place in its delicate structure as materially to alter its appearance under the microscope.

2. Seal oil, sperm oil, whale oil, and cod-liver oil, were each carefully tested with sulphuric acid, the result being that the other oils gave a brown reaction varying in the different kinds, and not to be mistaken for the peculiar lilac colour afforded by cod-liver oil.

3. Eight specimens of cod-liver oil procured from different druggists, were tested simultaneously. All reacted characteristically, but there was a material difference in the intensity and richness of the colour. In some the colour appeared instantaneously, in others more slowly. The duration of the lilac hue, before passing into brown, varied materially, and for the most part bore a direct ratio to the richness of the colour which was most persistent in the best oils.

4. Two samples of cod-liver oil prepared in Newfoundland, having previously been tested and found to be genuine, were exposed to the acid at the same instant with four samples manufactured in London, with the result that, with one exception, the reaction of the Newfoundland oil was richer than with the English. The excepted oil was as perfectly as possible freed from the stearine, of which the Newfoundland oil is said to contain a smaller proportion.

5. A specimen of brown cod-liver oil was submitted to a comparative examination with two London and one Newfoundland made samples, the result being that the richness of colouring in each was nearly alike; but that, whilst the Newfoundland and English pale oils gave the characteristic lilac reaction, that of the brown oil was much redder and less characteristic. This experiment, varied in several ways, and with several specimens, both of brown and pale oil, was repeated at least for[illegible] times, and on several days, with the like result. The brown oil is less fluid t[illegible] pale oil, in this respect more nearly resembling seal oil. The same is the case

the skate-liver oil in my possession, although it is much purer looking, and more fluid than several specimens of the brown oil that I have examined.

6. Small quantities of the best Newfoundland and London oil were carefully heated for from ten to fifteen minutes in a test-tube held over a spirit-lamp. Copious bubbles of air were at first given off, and the lamp removed as soon as the oil began to give off vapour. The result was slight thickening of the oil, without any diminution of the transparency; the oil also became darker in colour, so as closely to resemble the best kinds of brown oil. On being tested comparatively with the same oil unheated, and with the brown oil, the reaction was found to be much less characteristic than the former, and to have much of the redder hue of the latter.

7. Skate-liver oil, of a rich amber hue, was exposed to the same comparative test with Newfoundland, London, and brown oil. The colour yielded by the skate oil more nearly resembled that afforded by the brown oil than the others, partaking more of a red than lilac tint, but was intermediate between the two kinds.

8. Two specimens of oil, the one divested of stearine, the other in its natural condition, were tested with this result—that the former reacted more rapidly with the acid, but both were equally rich in the characteristic hue. That which retained the stearine lost the lilac colour earlier than the other.

9. Specimens of stearine from cod-liver oil, procured from two different manufacturers, were tested, and found to possess the characteristic reaction of the oil itself, which speedily turned to a dirty-brownish hue.

10. Equal quantities of pure seal oil and of pale English cod-liver oil were carefully mixed, and then tested; the reaction was less intense, and the lilac very soon passed into a dirty-looking colour.

11. The same experiment repeated with Newfoundland cod-liver oil gave nearly the like result, the reaction at the first more nearly resembling that of pure cod-liver oil, but the lilac hue speedily passed into a dirty slate colour.

12. Seal oil, with which iodine in the proportion of half a grain to an ounce of oil had been carefully triturated, was exposed to the test. There was but slight action, the iodine appearing to protect the oil from the action of the acid.

13. Seal oil, with which iodide of potassium had been carefully mixed in the proportion of three grains of the salt to an ounce of the oil, submitted to the same test, gave very much the same reaction as unmixed seal oil.

14. Seal oil, mixed with fresh ox-bile in the proportion of three drops of the latter to an ounce of the oil, was converted into a sort of emulsion, and gave only the reaction peculiar to seal oil. The same result followed when the mixture had been kept a month, and shaken up at frequent intervals.

15. A portion of seal oil, in which iodine and ox-bile had been carefully incorporated in the proportions already named, gave the same result as that mixed with iodine only.

16. Ox-bile, submitted to the sulphuric-acid test, either pure or mixed with two parts of water, was turned to a dirty green colour.

17. Diluted ox-bile, mixed with a little weak syrup, and exposed to the sulphuric-acid test, gave a lilac reaction, very nearly resembling that afforded by cod-liver oil.

18. Diluted ox-bile, containing a few drops of acetic acid, on the addition of sulphuric acid in excess, yielded a reddish-purple, much less rich and characteristic than that afforded by syrup.

The result of these experiments appears to prove that the pale cod-liver oil is the purest, being that which is least changed in the course of manufacture. This, at least, applies to the oil extracted from the livers of the species of cod met with in British waters and on the banks and shores of Newfoundland. The darker varieties of oil would seem to owe their colour and the greater thickness which they usually possess to the mode of manufacture; probably to the degree of heat employed in their extraction from the liver. It is also probable that the quality and colour of the oil vary according to the length of time the livers are kept previous to the extraction of the oil, for they very soon begin to undergo decomposition.

Sulphuric acid appears to be a perfectly reliable test of the excellency of cod-liver oil, a very little practice enabling a tolerably careful manipulator both to distinguish whether the article is genuine, and likewise whether it has been carefully prepared

from perfectly fresh livers and at a moderate temperature. It would appear, as one result of my investigations, that no artificial compound of fish oil with iodine or bile can be readily manufactured so as to simulate the reaction of genuine cod-liver oil with sulphuric acid. The experiment with the liver seems to be decisive as to the lilac hue being the distinctive reaction that pure and well-made cod-liver oil ought to yield with sulphuric acid; and that with the bile seems to prove that this peculiar characteristic is chiefly, if not altogether, due to the biliary elements contained in the oil.

To several pharmaceutical chemists I am indebted for the particulars of the plan upon which the oil is extracted. All agree in the opinion that the freshness of the liver is a most important element towards the manufacture of a pure, sweet, and almost tasteless oil—an opinion fully confirmed by the proof of the rapidity with which it undergoes decomposition afforded by my microscopical investigations. At whatever hour the livers are received from the purveyors, the oil is at once manufactured. The most usual mode of preparation is to put the sliced livers, after being carefully cleansed, into a suitable vessel, to which the heat of hot water or steam is applied. The oil, which is yielded freely, is then strained, and ought to require no kind of purification. In cool weather cod-liver oil deposits its stearine in a flaky form, which, diffused through the oil, gives it a milky appearance. Moderate warmth re-dissolves it, and renders the oil perfectly transparent and as pure-looking as before. During this state, patients find the oil more nauseous and liable to cleave to the palate in an unpleasant manner, and probably on this account the stearine is occasionally extracted. The removal of the stearine does not impair the efficacy of the remedy, neither is there any reason to believe that it increases it, for I have witnessed equally good results from both kinds. As, however, the stearine contains a large proportion of the ingredients which occasion the lilac reaction with sulphuric acid, I am disposed to believe that its extraction, excepting for the use of very fastidious persons, is, commercially speaking, a mistake, as tending to lessen the quantity of oil procured from a given weight of liver, and thus to increase the cost of an article of so much importance in a remedial point of view, and the price of which is a serious hindrance to its proper administration in the diseases of the poor. It would seem that season exercises a great influence over the quantity of oil yielded by a given weight of livers, the largest produce being obtained in winter. The best time for commencing operations is said to be at the end of October, at which time the yield is very moderate. The quantity obtained gradually increases through November, and attains its maximum in December; after January the quantity again declines, and in the spring becomes so small as to render the manufacture unremunerative. During the height of the season, the amount of oil extracted very nearly equals half the weight of the liver employed. One manufacturer informed me, about the middle of December, that from 2301 pounds of liver received during the present season, he had extracted 900 pounds of oil. At that time the yield was just attaining its maximum.—*Lancet, Feb.* 17, 1855, *p.* 182.

33.—*Cod-liver Oil with Quina.*—[Mr. Bastick makes this preparation as follows:] The oleum morrhuæ cum quinâ is simply a perfect solution of quinine in cod-liver-oil. The quantity of quinine may be varied according to the wish of the prescriber, although it is generally employed in the proportion of two grains to each ounce of the oil. The preparation is best made in the following manner:—The requisite quantity of disulphate of quinine is dissolved in distilled water, with the aid of a little dilute sulphuric acid. The quinine is precipitated from this solution by means of an alkaline carbonate; the precipitate is treated with boiling alcohol; the resulting alcoholic solution, after being filtered, is evaporated to dryness. The residue, which is pure quinine, is then added to the cod-liver oil, and the mixture is heated in a water-bath until solution is completely effected, which is known by the oil becoming perfectly transparent.—*Lancet, Feb.* 3, 1855, *p.* 126.

DISEASES OF THE ORGANS OF DIGESTION.

34.—ON SARCINÆ VENTRICULI.

By Dr. Arthur Leared, Physician to the Royal Infirmary for Diseases of the Chest, &c.

[In a case of this kind, treated by Dr. Leared, the vomit consisted of a small portion of watery fluid followed by a gush of thick brown matter, varying from a pint to a quart, which, on standing some hours, indicated active fermentation, the surface being raised and covered with bubbles. The microscope showed numerous sarcinæ.]

Ordered, ℞. Liquoris calcii chloridii 3j. aquæ 3vij. Sumat haustum talem ter quotidie.

May 26. Six weeks have elapsed since the above was prescribed; she has not taken it regularly, but is, nevertheless, much better. After the first few days I could discover no sarcinæ in the vomited fluid. The vomiting had been reduced to an average of not more than once daily, and this improvement has continued. Since then she has scarcely had any pain or appearance of the tumour.

She was to-day admitted under my care to the Royal Infirmary for Diseases of the Chest. A daily register of the case was now kept, of which the following is an abstract:—

On the first five days she took the liq. cal. chlor. as before directed; vomiting occurred daily, with one exception, after dinner (at one p.m.), at intervals of from one to four hours—average three hours and a-half. It occurred on two nights also.

On the eleven succeeding days, she took a drachm of hyposulphite of soda in a little barley-water thrice daily, there being few omissions, with these results:—First and third days, no vomiting. On the nine remaining days it occurred from one to five hours after dinner—average, two hours, fifty minutes. On three of the latter days, however, it occurred at four a.m. also; and on two of these at nine p.m. and ten p.m. respectively. The medicine was omitted, as she said it made her weak.

On the three succeeding days she had no medicine. On the first she vomited at four p.m.; the second, at four a.m., and at nine p.m.; the third at four a.m., and again at half-past four p.m., although she had no dinner.

For the next fourteen days she took the following:—

℞. Infus. rhei, infus. gent. aa. ℥ss., fiat haust. unam talem ter indies ante cibum sumend.

First day, vomited at half-past four a.m. (medicine not commenced with), again at five p.m. Vomiting after dinner occurred on six days; longest interval six hours and a-half; shortest four: average, five hours ten minutes. It also occurred on two other days, times not noted; but on five of the fourteen days there was no vomiting.

The following pills were now substituted:—

℞. Creosoti ♏iss., ext. opii gr. ¼, ext. gent. q. s. M. ft. pil. sumat. unam talem ter quotidie ante cibum.

These were also taken for fourteen days. They appeared at first more beneficial than the draughts, especially in diminishing the quantity vomited. Vomiting occurred but six times during the above period. The entire cessation of vomiting on the last three days of it appearing to be caused by her now evidently sinking condition, they were omitted.

From the date (June 12) to her death (June 16), although she continued to take food, there was no vomiting. Death ensued slowly from exhaustion.

[On a post-mortem examination the stomach was found as follows:]

Stomach.—This organ was completely dislocated. It extended perpendicularly from the diaphragm, against which its cardiac extremity pressed to a line that would connect the anterior superior iliac spines. Lying entirely to the left of the spinal column, it occupied the position of the descending colon, which at first sight it closely resembled. It contained about a pint of "coffee grounds" fluid. Its greatest length when empty and laid flat, was 11¼ inches. Central width 2¼ inches. Capacity, measured by distension with water by means of a funnel in the œsophagus, the pylorus being tied, exactly a quart. The coats of the organ, for about three inches from the pylorus, were transformed into a thick unyielding mass. On section,

this was found to be cancer of the colloid variety, with some fibrous intermixture. It presented under the microscope the usually received evidence of malignancy; and Dr. Bristowe, Pathologist to St. Thomas's, who was kind enough to examine the morbid growth, agreed with me as to its nature. The cancerous mass terminated abruptly in a ring of nodules, some as large as marbles, projecting into the stomach; beyond this the mucous membrane appeared quite healthy; but the muscular coat, near the diseased part, was hypertrophied. A test tube 5-12ths inch diameter was with difficulty passed through the tortuous passage representing the pylorus portion of the stomach. The tract was almost completely denuded of mucous membrane.

With regard to remedies, it would appear from the foregoing that chloride of calcium was more efficacious in restraining the vomiting than hyposulphite of soda. Full doses of the latter were given, in order that it should have a fair trial. Its beneficial action was at first more apparent than subsequently; but I observed this to be the case with all the remedies tried. It is to be remarked, however, that from the first exhibition of chloride of calcium, the frequency and severity of the vomiting became permanently diminished; and the entire cessation of pain is fairly ascribable to the same agent. I constantly questioned the patient while in hospital in reference to pain; and it is an interesting fact, that, during that period, she suffered none whatever. The relation between the non-appearance of the tumour, cessation of pain, and diminution of vomiting, was, I think, however, clearly established by the position in which the stomach was found. Improvement of these coincided with diminution in the quantity of distending gas.

Although, so far as I am aware, chloride of calcium is now, for the first time, recommended for affections of the stomach, I had previously used it extensively in dispensary practice. In cases of atonic dyspepsia, with acidity and foul eructations, its use is attended with excellent results. Many old sufferers have assured me that no remedy had proved so beneficial to them. I explain this, from its decomposition by many acids likely to be present in the stomach, either abnormally or in excess, their being precipitated thereby, and also to the evolution of free chlorine. To the latter, the destruction of sarcinæ is, perhaps, to be attributed. From close microscopic observation during the exhibition of both, I believe this power to be possessed at least as much by chloride of calcium as by sulphite of soda. The point on which I lay stress at present is the permanent improvement of symptoms after liq. cal. chlor. had been a short time taken. Was some catalytic body, distinct from that concerned in the fermentation which subsequently occurred in the stomach, eradicated by this agent?

On omitting the above remedies sarcinæ soon re-appeared, and were invariably afterwards found in the vomited fluid. I also noticed, both associated with sarcinæ, and as constantly when they were not present, a profusion of irregularly-rounded, semi-transparent bodies, of a greenish brown colour, smaller than the sarcinæ, but of variable sizes, more or less aggregated in masses. The constancy, similarity, and general appearance of these bodies made it impossible to confound them with debris of food. I cannot but regard them as embryonic sarcinæ, and that influences destructive to the maturer organisms only arrested their development. This would explain the rapidity with which sarcinæ are re-produced after apparent total destruction. According to Frerichs, sarcinæ are developed from primitive round cells.

The effect of remedies on the vomiting was marked by its aggravation on their omission. But that sarcinæ were not its essential cause was plain, because the greatest improvement was effected by remedies, with the use of which sarcinæ coexisted.

Different kinds of diet were tried, but with less difference of result than might be expected, and great difficulty was experienced in restraining the craving of the patient for certain articles. Animal food appeared most suitable. Vomiting was always increased by a bulky diet, as bread with broth or gruel. Constipation became at length very unmanageable; castor oil was latterly fancied by the patient as a purgative, but it required the addition of croton oil to insure effect. The suspension of the renal secretion was remarkable. On two occasions the quantity of urine passed in twenty-four hours is noted as only two and a-half ounces; yet the skin was constantly parched. Diuretics were tried, and seemed ineffective. Compared with the thirst and dryness of skin in diabetes, the absence of thirst in this

patient was interesting. It points to an intimate relation between perception of thirst and increased renal action. In the present case, there was ample abstraction of pabulum from the secreting organs. From a comparison of the quantities of urine passed with the quantities vomited in twenty-hours, on eighteen occasions, accurately noted, I find a very close inverse relation between them. The urine was generally dark coloured, of high specific gravity, and of variable re-action, occasionally depositing whitish lithates. It was often so charged with urea that a very copious crystallization resulted on addition of nitric acid. I have frequently observed this urea in cases of chronic copious vomiting.—*Med. Times and Gazette, Nov.* 18, 1854, *p.* 517.

35.—*Gallic Acid in Pyrosis.*—Dr. BAYES says that in pyrosis, where this disease is unaccompanied by extensive ulceration, or organic malignant diseases of the stomach, or by disease of the liver, the most marked benefit will follow the use of the remedy. Gallic acid, here, not only checks the secretion with a certainty and rapidity he has never seen follow the administration of any other remedy, but it gives general tone to the stomach, increases the appetite, and (what I very little expected when I first used it) in many cases removes constipation. This I can only account for on the supposition that the relaxed atonic state of the stomach which favours pyrosis is continued throughout the alimentary canal, the constipation in these cases arising from want of power in the muscular coats of the intestines to expel the fæces. This want of tonicity is remedied by gallic acid.—*Monthly Journal of Med., April*, 1855, *p.* 344.

36.—*Expulsion of the Tænia Solium by the Ætherial Extract of the Male Shield Fern.* By Dr. J. H. BENNETT, Edinburgh.—[Professor Bennett has found this to be the best remedy for Tapeworm. The following remarks on a case of this kind illustrate his views:]

She was ordered ℈ij of the ætherial extract of the male shield fern. to be followed in the morning by ℥j of castor oil. This caused the evacuation of seven joints of the worm, each of which were longer than they were broad. Another ℈j dose of the extract was ordered at night, also to be followed by ℥j castor oil in the morning. Nov. 22. Only three joints of the worm passed. To have this evening 3ss. of the extract. Nov. 23. This morning, after taking the oz. dose of castor oil, she passed many separate joints, and several long portions of tænia. The whole together, when measured, was calculated to be about fifteen yards long. One portion was evidently formed of the joints of the worm near the head, as they were broader than they were long, and not above the tenth of an inch in breadth. Some joints were square, and others longer than they were broad, measuring from half an inch to three quarters of an inch in length. No head could be discovered, though carefully searched after. She remained in the house till the 6th of December; but although she took 3ss. of the extract three times, and one dose of ℈ij, no more joints of the worm came away. This woman has returned to the hospital twice since her dismissal, to say that she is quite well.—*Monthly Journal of Med., Feb.* 1855, *p.* 107.

37.—*On the East and North-East Winds, and their Connection with some Derangements of the Gastro-pulmonary Mucous Membrane.*—[DR. C. B. GARRETT, in a work which he has just published on this subject, recapitulates his opinions as follow:]

I believe that the east and north-east winds dry up the skin and respiratory mucous membrane; that they arrest the removal of lactic acid by the skin; that the mucous membrane becomes loaded with this acid; that they set up the lactic diathesis. That lactic acid is generated to excess in the capillaries; the alkalinity of the blood is overpowered; the sugar and farina undergo acidulous conversion, and that the mucous membranes elaborate lactic acid in excess. That the pneumogastric nerves sympathetically connect the respiratory organs with the stomach, and that other organs also are powerfully affected by the lactic acid diathesis. Under these circumstances, fogs, and an atmosphere loaded with moisture or floating extraneous

matters, may also distress the sensitive larynx. The principles of treatment are to restore the healthy action of the skin, to neutralise free acid in the stomach as well as in the blood, and to procure the proper alkaline ascendancy in the circulation. Lastly, to prevent a recurrence, and insure a permanent cure, by the *strictest attention* to diet. I may conclude by saying, that if I was forced by any strange coincidence to make my choice in the treatment of diseases (not immediately threatening life) with the use of medicine or the regulation of diet, I would infinitely prefer the means placed at my disposal by the latter, and should rely with the greatest confidence on the powers afforded me by the vast resources of dietation.—*Med. Times and Gazette, Jan.* 20, 1855, *p.* 63.

DISEASES OF THE URINARY ORGANS.

38.—ON DIABETES.

By Dr. Thomas Hodgkin.

[Dr. Hodgkin seems to have paid great attention to cases of this description. What he says is quite true, that in this, as in most other intractable diseases, the patient rarely continues under the same medical man throughout the affection. Hence Dr. Hodgkin has been careful in many cases to watch the disease throughout. He says,]

The exciting causes have appeared to be very various; sometimes the disease seemed traceable to mental fatigue or anxiety; sometimes to profuse perspiration checked; sometimes to the decline of life; and I have little doubt that in young persons it may have been induced by errors in diet, though instances of this kind are not so easily detected.

With little, if any, exception, I observed the decline and death of diabetic patients to be characterized by what seems to indicate a remarkable loss of tone or vital conservation of the tissues. They were defective in the power of repairing injury, or gave way to destructive inflammation and sphacelus inducing the death of the patient. It is therefore difficult to say what would be the extreme of the diabetic tabes, if it were not curtailed by the intervention of these local causes.

In illustration of this remark, I may mention, first, the case of sphacelus after venesection, to which I have already alluded, and the fact that the young man whom I have likewise noticed as the subject of another case, was carried off after a very few days of acute pneumonia induced by comparatively slight exposure. On examination of the body, a considerable portion of pulmonary texture surrounding a solitary and solid tubercle was found in a state more resembling gangrenous softening than any of the ordinary stages of pneumonia.

Another gentleman beyond the middle period of life, in whom the disease had probably existed for some years, its progress having been checked by assiduous care, was, by an accidental circumstance, exposed to the cool air of the evening at the close of a warm autumnal day. He was almost immediately seized with pneumonia, which was so very rapidly fatal, that I can scarcely doubt that its characters resembled those of the preceding case, although no examination of the body was made.

On referring to inspections made by other persons, as well as to my own, I find additional examples of similar acute lung disorganization.

Another gentleman, of similar age, whom I saw occasionally during the course of his malady, but who ceased to be under my care towards its close, was, I was informed, cut off by sores in one or both feet, having a gangrenous character, and proving speedily fatal.

In another case, which I had the opportunity of observing for several years, and which was the most protracted case of well-marked diabetes which has come to my knowledge, the feet were long in a very troublesome state—often threatened with the formation of sores which were only warded off by the most assiduous care, which was paid not only to every indication of commencing lesion in the lower extremities, but also to every symptom in his variable and progressively declining health. At length, symptoms of decided gangrene occurred in one toe, progressively extending to the whole of the anterior half of the foot, and having very much the character of

dry senile gangrene. Its progress, as well as the annoyance attending it, appeared to be mainly counteracted by the careful exclusion of the air by means of genuine Barbadoes petroleum. I think it could not have been less than six or seven months that the patient survived after gangrene had decidedly commenced. Another indication of the impaired conservative vitality of the textures is seen in the tendency to the production of sores on the glans and prepuce from the irritation of the urine.

Reflecting upon the evident tendency of diabetes to impair the vitality of the textures, I could not but attribute it, not to accident, but to an essential connexion between the disease and the function of nutrition; and it occurred to me that there was a great analogy between this state of the system produced by diabetes and that which is met with as an occasional endemic, in some parts of the continent, from the use of damaged rye; both these diseased states being characterised by favouring the production of gangrene. There is, however, this necessary difference in the tractability of the two, that one is produced and maintained by faulty vegetable aliment supplied to the system, and ceases or yields when a wholesome diet is substituted: whereas the other depends on defects of the material of nutrition, produced by imperfection in the process to which materials, good in themselves, are subjected.

Not having had the opportunity of myself observing the symptoms and progress of ergotism, I am unable to say how closely the analogy may be traced; but I should like to commend it to the attention of those who may have the opportunity of making the investigation. It is now eight or nine years since I suggested this view of the subject in my lectures delivered at St. Thomas's Hospital.

Although far from undervaluing the interesting researches of animal chemists for the elucidation of the disease, I was so forcibly struck with the fact that the therapeutical suggestions derived from them, whether in medicine or in hygeia, had signally failed in producing the desired result, that I determined to proceed on a different tack. Instead of restricting patients to animal food, I was content to allow it decidedly to predominate, being at the same time associated for the most part with such vegetable matter as could not yield a large proportion of sugar. The succulent vegetables and well-fermented beers were therefore preferred; though the restriction was by no means strictly enforced, except when an extraordinary increase in the specific gravity of the urine rendered temporary rigid restriction desirable; in these circumstances, it appeared to me to be decidedly useful. In one case in particular, the greatest advantage resulted from a careful member of the family being instructed frequently to take the specific gravity of the urine, and thereby to be able to decide when to indulge and when to restrain. The stomach was therefore not needlessly offended; and the preservation of health was promoted by a somewhat extended range of diet.

The example of the Edinburgh patient, so remarkably benefited by returning to bodily labour, induced me to insist strongly upon the use of daily exercise. But, in conjunction with the promotion of perspiration so effected, I judged it necessary to guard against its suppression by vicissitudes of temperature, by means of flannel clothing, and to maintain the healthy condition of the skin by sponging and friction. I was particularly impressed with the desirableness of this part of the treatment, by the notorious fact that the kidneys and skin are to a considerable degree vicarious to each other, and also by the observation that diabetes appeared in some cases to be traceable to the suppression of a very active cutaneous perspiration. Moreover, in diabetic patients not only is there a sweet odour of the breath and urine, but the perspiration also partakes of the same character. A diabetic patient has himself been struck with the return of the normal odour of this secretion amongst his symptoms of improvement. Another point to which I thought it necessary to pay strict attention was the character of the alvine secretions, which, as I have already observed, are apt to be not less perverted from their normal character than the urine. A greatly perverted and offensive odour, and a more or less complete absence of bile, were the most evident morbid characters. Whilst they appeared to me strongly to forbid the employment of opium (much as this drug has been recommended), they pointed to the use of occasional alterative doses of mild mercurials, and induced me to give ox-bile in such a manner that it might be blended with the food on leaving or when it had just left the stomach. The benefit derived from this last suggestion was so marked, in a very protracted case which came partially under my care, that the medical man who was in close attendance constantly had recourse to it to relieve

the *malaise* which was experienced whenever the absence of bile became most complete.

The medical agent which I have been accustomed to regard on the whole as the most important, has been creasote, given in very small doses (half a minim) at the time of taking food. It appeared to me that the greatest error in the process of assimilation is in that part which should take place at its commencement, viz.:—in the stomach and duodenum; and it is in stimulating these that creasote in such doses seems mainly to act. In large doses it would probably be too powerful, and would disturb rather than assist; whereas, in very small quantities, it probably produces that diffused slight excitation which, as Billard has shown, is characteristic of the digesting state of the healthy stomach.

In conjunction with these means, I have thought it desirable to use such other vegetable or mineral tonics as seemed best suited to the state of the individual patient. Strychnine, quinine, and cascarilla, are those which I have preferred. About eight years ago, I attended a patient in the north of England, who presented the symptoms of well-marked and far-advanced diabetes. He was emaciated, but his loss of power was still more remarkable. I laid down for him a course of management as to medicine, diet, and exercise, in accordance with the principles which I have just explained. I never saw him afterwards; but I had the satisfaction of learning that his improvement was great and unequivocal. He recovered the power of walking several miles, but I had no opportunity of learning whether there was any restoration of the sexual power, which he had entirely lost. After the lapse of a considerable time, the patient's health again declined. He came to town to consult me, when I was unfortunately absent; and I was not informed of his state and symptoms. He necessarily came under other hands, and ultimately sank; but I heard no particulars regarding his death.

I had the opportunity of watching for many years a much valued friend and patient, who was long the subject of this malady. It had been recognised and treated for a considerable time before the case came under my observation; in fact, it might be said to have then arrived at a fully confirmed stage. The mode of treatment which I have described was carried out in every particular. The urine was frequently and carefully examined; and, though it fluctuated in its specific gravity, it for the most part but little exceeded the lightest diabetic point. The urine was also frequently submitted to careful chemical analysis by an experienced manipulator who was purposely kept in ignorance of every particular respecting the patient, except that he was diabetic; and I regard it as no small confirmation of the beneficial influence of the means employed, that he volunteered the opinion that the specimen of urine was voided by a young person in whom the disease was slight and incipient, and that he also suggested that the diet employed was too exclusively animal, although, as I have before observed, it had by no means been my practice to subject diabetic patients to severe restriction in this respect; and the gentleman in question, who was advanced beyond the middle of life, had formed no exception to this rule. I may further add, that this patient continued to be corpulent, and, for a considerable time, varied but little in weight. Even towards the close of life, the diabetic symptoms did not increase, unless some tendency to tenderness and soreness in the lower extremities might be regarded as such. Great hepatic derangement and gastric symptoms apparently connected with the gouty diathesis of this patient, may be said to have nearly or altogether superseded the original malady.

In another case, occurring in a person connected with one of the government offices, diabetes was fully established. The urine was copious, saccharine, and heavy; and thirst was proportionately great. Under treatment of the character which I have already described, all the symptoms yielded, and the patient improved in health, strength, and appearance; and, as he assured me, he still retained his virile power. Notwithstanding this encouraging change, the patient died suddenly, and, if I am not mistaken, in a fit; but, as I neither attended him myself, nor received any information from the medical man who did so, I am unable to give any particulars of this event, except that it was quite unexpected.

I might mention some other cases which have come under my care; but they offered little if anything to add to the points which I have already noticed. I have not sought for diabetic patients, and have never taken up the subject as a speciality. Had I done so, I should have been prepared to present a very different communi-

cation from that which I am now making. Contemplating the disease with interest, not merely in consequence of the remarkable character of the symptoms which it presents, but also in relation to other diseases, it suggested considerations which, in my opinion, possess interest and importance in relation to pathology and therapeutics in general. Although I have not the vanity to suppose that I have done much towards the cure of a disease generally regarded as incurable, I may at least hope that I have taken a short step in a right direction towards the legitimate object, viz., that of mitigating its symptoms and deferring its fatal result. It is also some satisfaction to believe that this may best be done by discarding some hitherto applauded means which are deleterious or distressing.

In the pathology of the disease, I believe that the first perceptible indication of derangement of the healthy process occurs in the stomach. I have long entertained this belief; and, on turning to the authorities who have made a study of the subject, I find that I am borne out by Mr. Bouchardat, and also by Dr. West, an American physician. But are we hence to suppose that there is any organic derangement in the stomach itself? The morbid anatomy of the stomach has thrown no light on the subject. Daily experience teaches us how much the functions of the stomach depend on conditions not primarily connected with the organ itself. Mental influence can instantly destroy the best appetite; a blow on the head may give rise to nausea and sickness; and loaded intestines may produce intense acidity. As we may palliate and mask this acidity by an alkali, so we may temporarily suppress or modify the condition of the stomach as it exists in the diabetic at the commencement of their digestion. The effect, however, is only palliative, not curative; and to make any progress towards obtaining this result, we must employ all those means which indirectly yet effectually improve the stomach of the dyspeptic, viz., relief from care, cheerful but not engrossing mental occupation, bodily exercise, fresh air, attention to the skin, and a due amount of rest. A trainer would perhaps be the best hygeist for a diabetic patient.

The curious experiment of Bernard, who has shown the powerful influence of the irritation of a particular part of the brain in the production of sugar in the liver, seems to have a bearing on this subject, and to favour the view which I have taken; since it can hardly be doubted that the influence on the liver in the one case, and on the stomach in the other, was exerted through the *par vagum*. It is not improbable that, even in Bernard's experiments, the sugar was really produced in the stomach, and thence conveyed into the circulation.

The presence of sugar, in some form or other, in the animal system, is not inconsistent with a healthy or physiological state. It is perhaps always more or less present in the liver, especially in that of the dog; hence probably picromel in the bile. It is always present in milk; and all the farinaceous articles of diet appear to be partially dulcified in the process of digestion. It should not be so much our object wholly to banish sugar, and those vegetable principles which are convertible into it, as to maintain the function by which its production and conversion are regulated. It is probably from an imperfect view of the whole subject that we may be led by accurate chemical observation to an erroneous chemical theory in treatment; hence, inconsistent as it may appear, I should have no hesitation, under certain circumstances, to employ acids in the treatment of a dyspeptic patient, though troubled with various symptoms of acidity; or to use castor oil, or cod-liver oil, or oily emulsions, or to allow a moderate quantity of some fatty aliments where they might seem to be called for, although I might have reason to suspect fatty degeneration in some particular organs.

Although in diabetes I attach the primary importance to the digestive organs, and more especially to the stomach, liver, and pancreas, and to some undetermined condition of the nervous system by which these organs are influenced, I am very far from limiting the interest to these organs. The kidneys also claim attention; and great importance is doubtless to be attached to the peculiar cachectic condition superinduced, very probably through the kidneys, by the disease.

I am induced to differ from Dr. West, and others who may regard the kidneys as only passively concerned in the disease, by the following reasons:—In three or four instances I have met with diabetic patients, in which the disease occurred in families in which Dr. Bright's disease of the kidneys also occurred; and in two of these examples, the family predisposition to this latter affection of the kidneys was very

strongly marked. Again, the disturbance of the function of the kidneys does not seem to be limited to the mere passing off of the sugar brought to the kidneys by the blood; since, though urea is found in diabetic urine, it appears to be far below the healthy proportion. There is another symptom which, though not immediately connected with the kidneys, will strengthen the presumption that they are materially concerned. The great diminution or total abolition of the sexual power, so intimately associated with this disease and so characteristic of it that a patient has told me that it was the first symptom which called his attention to this malady, cannot be ascribed to mere general debility; seeing that the symptom occurs when the patient, as respects the general powers of the system, is very much stronger than many advanced phthisical patients, and other extremely debilitated subjects, whose virility remains perfect. I conceive, therefore, that there must be some sort of reflex action upon the organs of generation, of which the starting point is in the kidneys. The idea does not appear to be an overstrained one, seeing that the renal and spermatic plexuses are so intimately connected with the same portion of the spinal cord.

Again, it seems scarcely possible that the kidneys could be subjected with perfect impunity to the constant passage of their secretion, inordinate in its quantity, and vitiated and morbid in its properties. Finally, the resulting cachexia is precisely such as is to be expected from the imperfect performance of the depuration of the blood by the kidneys, and should direct our attention to them, just as a livid countenance prompts us to look for derangement in the organs of respiration.

I would in the next place invite attention to the state of the tissues of the body in general, which constitutes the cachectic condition to which I have alluded. It seems to result from the combined action of the diffusion of the sugared blood over the system, and the imperfect elimination of those excrementitious matters which the kidneys are destined to throw off. I have already compared it to the effect produced by the employment of diseased rye. It ought also to be compared and contrasted with the cachexia resulting from Bright's disease of the kidneys, to which it must be considered as allied; and, to do the subject ample justice, it should be considered in conjunction with some other forms of cachexia which merit careful attention, and which require to be studied as the complement to the morbid anatomy of all the organs considered individually. To attempt to do this, even in the most cursory manner, would carry me beyond the limits which can be allowed to this paper.—*Assoc. Med. Journal, Oct.* 13, 1854, *p.* 916.

39.—*On the Curative Treatment of Chronic Morbus Brightii.*—[In a paper read before the Harveian Society on this subject, by Dr. Handfield Jones,]

Four cases were related, in which patients, who suffered under dropsy, with more or less of general cachexia, and an albuminous condition of the urine, which contained casts, &c., were restored to greatly improved health by treatment steadily continued. At the same time the quality of the urine improved in a corresponding measure, and this very gradually. The treatment consisted in the administration of iron, chiefly the muriated tincture, either alone or combined with the bichloride of mercury. Cod-liver oil was given in one case, and quinine and the sulphate of iron in another. The position taken was, that Bright's disease was essentially a degeneration, widely remote from inflammation, and that its cure was to be attempted by the employment of all means likely to raise and maintain the general power. In a fifth case, after the removal of considerable ascites and anasarca, at first by blue bill, squill, and digitalis, and afterwards by the tonic treatment above described, the latter was continued with great improvement to the general health, but the urine still contained albumen. As, however, there was found no deficiency in the amount of urea or uric acid in the secretion, according to a rough estimate, the opinion was expressed that the draining off of albumen probably depended on an altered state of the Malpighian capillaries, similar to that which exists in cases of (so-called) chylous urine, the renal functions still performing their functions fairly well. In conclusion, it was strongly urged that practitioners should always carefully look out for indications of commencing renal degeneration, and strive vigorously and perseveringly to arrest the morbid process by such measures as in

the cases above detailed had proved successful. The disease was compared to pulmonary phthisis, and the hope expressed that, as medicine was more adequate in many instances to the arrest of the one, so it might become also to that of the others.—*Lancet, March* 31, 1855, *p.* 343.

40.—ON DIABETES.

By Dr. R. H. Goolden.

[An anonymous writer in the 'Med. Times and Gazette' recommends that in this disease we should give the salts containing oxygen in feeble combination, such as the chlorate of potassa, or, still better, the permanganate of potassa. Dr. Goolden has, however, failed in seeing any advantage from these medicines. In his interesting paper read before the Harveian Society, he gives a series of cases, in which cerebral disease and injury were the direct causes of diabetes, and with the relief to the cerebral mischief by treatment directed to the brain alone the diabetes vanished.]

The first case, which suggested all my subsequent experiments and observations, was that of a railway guard, stunned by a blow from the handle of a break, when revolving with force. Concussion and diabetes ensued. Since then, I have been able to trace the commencement of the disease to injuries. One patient had fallen down stairs, struck the back of his head, and was stunned, nine months before admission; the same date he gave as the commencement of his diuresis. Another had been stunned by the fall of a piece of timber from Victoria Tower, which injured his arm, and he had been diabetic ever since. This man was improving, but left the hospital, because he would not have a seton; but is, I understand, now in St. Bartholomew's, with the seton. Another man, now in King's Ward, traced his disease to a kick on the head from a horse, from which he suffered severely at the time. I might relate several others, were it not that it would occupy too much of your valuable space.

Another group of cases has presented itself, whose history does not record any accident, but which have been cured by treatment directed to the brain. I have only three of these, a small proportion to the whole, but invaluable as to their importance for induction.

Another important group consists of cases where the cure has been spontaneous or accidental. I have seen diabetes in its most aggravated form subside within a few hours; but pulmonary deposit, ending in a vomica, supervened.

Mr. Clapton, our very able resident assistant apothecary, who has watched these cases with great care and interest, informs me of a case of a medical man in the country, who was obliged to part with a large practice on account of being the victim of diabetes, and unable, from his consequent debility, to pursue his profession. After suffering great emaciation and debility, a large anthrax on the nape of his neck occurred, which extended towards the occiput, and down the back, and was nearly fatal to him; but, thanks to port-wine, brandy, and quinine, he recovered not only from the carbuncle, but also from the diabetes, and is now quite well.

Mr. Amphlett, of Birmingham, informed me of a case of diabetes in the person of a wealthy manufacturer, whom he treated upon the old plan of animal diet for some months, without much benefit, when he was suddenly seized with an attack of apoplexy; for this he was purged and cupped, after which there was no more diabetes. He subsequently died, as I learn, of a second attack of apoplexy.

In contrast with these cases, I have met with many that have not been cured by anything I could do for them; but, in the larger number of such cases, the urine has diminished at least one-half in quantity, and the specific gravity sunk eight or ten 1000ths, notwithstanding the patients have been supplied with abundant farinaceous food, cane-sugar, and as much fluid as they liked to drink.

I must also mention, that pulmonary disease, or congestion, is not an essential part of diabetes, though it does frequently coexist. In two aggravated cases now in King's Ward, the lungs are quite clear. Dr. Bristowe and myself took some pains in examining them, to ascertain the fact, in consequence of your correspondent's assertion, which, if universally true, would be an important point.

With regard to remedies, our attempt at specific treatment must prove a failure.

The supply of oxygen to the food in the stomach can and does do no good. The stomach cannot be made to perform the function of the lung; but, whatever the suspended function may be, I have found it partially restored by the application of the continuous galvanic current from the nape of the neck to the epigastrium. In one chronic case, which I had not been able to relieve to any satisfactory amount, I found the quantity diminish one-half after the battery had been applied for half an hour for three days; and I am now about to try what may be effected by the use of Strongfellow's or Pulvermacher's portable electro-voltaic batteries.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 598.

41.—ON DIGITALINE.

By Dr. R. Christison, Professor of Materia Medica in the University of Edinburgh.

[No observations seem to have been made in English works on digitaline, the active principle of digitalis, although this is one of the best diuretics which we possess, its greatest fault, however, being its uncertainty. On the action of digitaline Dr. Christison says:]

My experience of its effects has been highly satisfactory. I have used it expressly as a diuretic only; but its sedative virtues have also come incidentally under observation; and it is quite evident that digitaline is a most energetic remedy in both ways.

In the first two trials made with it, which were both of them in cases of extensive, protracted, obstinate œdema in connection with disease of the kidneys, diuresis commenced, in one towards the close of the second day, and in the other a day later; in both the flow of urine was profuse; and in both the œdema entirely disappeared, but with the slowness not uncommonly observed in this form of dropsy.

Here I cannot help observing in passing, that a striking illustration was presented, on both these occasions, of the unsoundness of the objection which has been brought by many practitioners, since it was first propounded by Dr. Osborne, against the use of diuretics in the renal forms of dropsy. I have taken every opportunity, in my clinical lectures, and in occasional publications, to protest equally against the unsoundness of the theory and the looseness of the facts, which have led to the prohibition of diuretics in renal dropsy, and which have deprived many persons of the most immediate and most efficacious means of relief from the principal secondary affection occurring in Bright's disease of the kidneys. And I may here repeat, in the strongest terms, that I have never, except in a single instance, seen the albuminosity of the urine, or any other indications,—which either the other conditions of the urine or any local symptoms can furnish,—increased under the employment of such diuretics as I have given in this disease. These are digitalis, squill, and bitartrate of potash, sometimes singly, more commonly combined. What may be the case with other diuretics, I shall not pretend to decide. I strongly suspect that they are all in the same category, and that they have been shunned on grounds purely theoretical and baseless. But at any rate I can answer for the theory not holding in the case of the three diuretics I have mentioned. So, too, in the case of digitaline. It is not a little remarkable that in the first two patients to whom I administered it, the albumen, instead of being increased, was quickly and greatly diminished. In one it disappeared entirely in a few days, and did not return so long as the patient remained under observation in convalescence. In the other it also disappeared; but after some days the albumen reappeared, though in a greatly diminished proportion. Digitaline, therefore, while it stimulates the kidneys to increased secretion, has not, in so doing, any effect, as some have thought of digitalis and diuretics at large, in increasing the peculiar renal irritation which constitutes or occasions Bright's disease.

Digitaline has proved equally, or even more serviceable in dropsy connected with disease of the heart. It has, in my hands, accomplished complete discharge of the dropsical effusions, and thus effected such relief as to enable the patient to return to his occupations, though given in circumstances apparently very desperate. In this respect digitaline has done no more than is often accomplished by foxglove itself. But it has appeared to me to act with more speed, and with greater force after the action did begin. In one instance great depression of the heart's action was brought on instead of a flow of urine.

I have also given it in some local dropsies, more especially ascites, either simple or combined with anasarca of the lower part of the body only. It has not proved more useful, however, as a diuretic in these cases than digitalis and other diuretics; which in general fail to influence the urine or remove the dropsical effusion, when given internally. I have not yet tried it externally, according to the method recommended by me with infusion of foxglove.

There can be no doubt that digitaline possesses the action of foxglove itself upon the heart and circulation. I have not made express trial of this action. But while using it to excite diuresis in a case of dropsy with diseased heart, the pulse began to flag, soon fell to 44, and became very soft and somewhat irregular. At the same time languor, nausea in a slight degree, and faintness ensued. All these symptoms disappeared in a few days after the remedy was discontinued. On this occasion I observed that the two actions of digitaline, its diuretic and sedative actions, do not concur. It is very likely that they are even incompatible. This I have formerly stated to be a probable fact in regard to the actions of foxglove. The disregard of it is, I apprehend, the reason why some underrate the efficacy of foxglove as a diuretic. Attention should therefore be paid to the fact in using digitaline.

The dose to be administered is stated by MM. Homolle and Quevenne at a seventy-fifth part of a grain of digitaline, three times a day. I have never given any other dose. It is a very small one certainly. But a tenth of a grain will kill a little dog; so that the dose is only in proportion to its exceeding energy. The form I have used is that of a pill about a grain in weight. It is to be given with the conditions and precautions observed in using foxglove.—*Monthly Journal of Medicine, Jan.* 1855, *p.* 2.

42.—*Treatment of Enuresis.*—As it is often desirable to have at hand various formulæ for the treatment of this troublesome affection in children, we may mention that Dr. Blaschko, of Freyenwalde, recommends a mixture of equal parts of *tinct. nucis vomicæ* and *tinct. ferri acet.*, of which from 10 to 15 drops are given twice during each evening. Dr. Hüber, of Zürich, recommends *ext. nucis vom.*, 1 p., *oxyd. ferri nigr.* 48 p., giving 2 grains night and morning. Nägele gives 1 grain of *tannin*, night and morning.—*Gaz. des Hôp.* 1855, *No.* 22.—*Med. Times and Gazette, March* 24, 1855, *p.* 289.

43.—*Use of Cubebs in Infantile Enuresis.* By Dr. Deiters.—This author has found cubebs more effectual than any other remedy in curing the incontinence of urine so common among children. This complaint may depend on atony of the bladder, or on the presence of intestinal worms. In the former case, the cubebs acts as a tonic, in the latter as a valuable anthelmintic. The medicine requires to be given in considerable doses; two pinches (*i. e.* a few grains or *Zwei Messerspitzvoll*) for infants, and half a teaspoonful twice or thrice daily for children of a somewhat more advanced age. Its effect is speedy and permanent; and although occasionally it happens that during its administration the incontinence returns at periodical or irregular intervals, these recurrences gradually become less frequent, and eventually disappear altogether. To effect a radical cure, the author has often found it necessary to continue its use for a period of from three to eight weeks, and he has never observed any injurious effects from its administration.

Deiters observes that he has found the same remedy most efficacious in checking nocturnal emissions in cases of spermatorrhœa.—*Preuss. Vereins-Zeitsch.*—*Dub. Hosp. Gazette, Feb.* 15, 1855, *p.* 32.

SURGERY.

AFFECTIONS OF BONES, JOINTS, &C.

44.—ON CONSERVATIVE SURGERY.

By Henry Smith, Esq.

[Mr. Henry Smith, in a journey partly for health and recreation, visited the Jersey Hospital, the scene of Mr. Jones' labours. He gives us some interesting remarks on what he saw.]

Both in the Jersey Hospital, and in various parts of the Island, are many living instances of the remarkable success which has attended Mr. Jones in his Conservative efforts.

Of the cases of excision of the knee-joint I had an opportunity of seeing three, one of which only is under treatment, the patient being a girl about 15, who had suffered for several years with incurable disease of the joint, which had made serious inroads on her constitution. The operation was performed about three months since, and although recovery had been much retarded by a large abscess which had formed on the back, the case is doing admirably; the line of incision had healed; the patient can move her limb, which was in capital position; and she was anxious to get up, as she thought she would be able to move about. This is the only operation of the kind since my visit last year.

The other two cases are those where operations were done about two years ago. The one is that of the little boy who was brought over to London by Mr. Jones last winter, and whose case excited much interest, in consequence of some morbid condition existing in the hip on the other side. Much difference of opinion was expressed by various eminent surgeons, as to the precise state of that joint—some saying that the head of the thigh-bone was dislocated, others that it was not. Now, however, there is no doubt, for the disease is in an advanced stage; and with all the signs of dislocation, the head of the thigh-bone can be distinctly felt to be thrown upwards and forwards; the parts about the knee, on the other side, are perfectly healthy, there being a very firm anchylosis, and the limb itself has increased considerably in length, and is perfectly straight.

The third case is that of a boy also, aged about 15, whom I saw last year, then perfectly well. He is now a fine active youth, and has a firmly anchylosed knee; by means of an apparatus which Mr. Jones has contrived for him, he can move about with the greatest agility without any stick. In fact, his activity is so great that he is obliged to be restrained—as illustrations of which I may mention, that on one occasion the young fellow was caught in the topmast of a vessel in the harbour, and another time he was espied in the branches of a mulberry tree in the garden of the hospital. In Park's celebrated case of excision of the knee-joint, the patient could run up aloft, he being a sailor. Mr. Jones's little patient has, it will be seen, accomplished the same feet with his stiff knee.

I had not time to see the other three cases in which Mr. Jones had successfully excised the knee-joint, but I believe they are all well.

More remarkable and perhaps more strikingly illustrative of the benefits of conservative surgery than any other case, is one in which excision of the entire ankle-joint, together with about three inches of the fibula, had been performed by Mr. Jones. It is impossible to describe accurately the extraordinary result which has

taken place here. The patient was an adult, who had some six months since received a compound Pott's fracture. Attempts were made to save the entire limb, but so much mischief was produced, both locally and generally, that Mr. Jones determined, against the opinion of all, to excise the ankle-joint. This operation was done, and the man made an excellent recovery.

On looking at the limb, if it were not for the cicatrix across the front of the joint marking the line of incision, it would not be easy to know that any such operation had been done, for the parts look almost natural, and there is a movable joint, and so little shortening that the patient can walk about, with or without his boot, briskly. The wound is perfectly healed; and what is extraordinary is, that in so short a time the portion of the fibula which was removed has become regenerated. The operator is very proud of this case, and well may he be. I have possibly had the opportunity of seeing and being engaged in as many operations upon joints as any other young surgeon, but I must candidly confess that this ankle-joint case is beyond anything I have ever seen in that way.

From the ankle we went to the *wrist;* and here Mr. Jones has had splendid success in two cases, where he performed excision of the entire joint. In the one, the patient is a healthy-looking young woman, who had suffered long from strumous diseases of the carpus. Excision was performed some twelve months since, and the consequence has been a perfect cure. There is a completely stiff and well-looking joint; the girl has very fair use of the fingers, and she exhibited it to me with a great air of pride and satisfaction, the more especially perhaps, as she recognised in me the same individual who, twelve months ago, saw her completely disabled, she at that time having just undergone the operation.

In the second case, the patient is a woman, about 40. The operation has been done for disease about six months; a complete recovery has taken place, and there is a stiff wrist, but, as yet, there is very little ability to move the fingers; for this, more time is required.

Of the Elbow.—Mr. Jones showed me one case where he performed re-section with the most satisfactory results, the patient being a woman, about 50 years of age, and the operation having been executed about two years since for disease. Notwithstanding the patient's age, a capital result has followed, for she has a good movable joint, and can exercise with facility the not very easy duties of cook at a large house. I may here mention, that it is somewhat rare to see a movable joint after excision of the elbow; and, moreover, that the operation in question does not generally turn out very well in persons of advancing or advanced life. Therefore, I look upon this case as the more interesting and remarkable.

Among other cases in the Hospital, I saw still under treatment a patient from whose foot Mr. Jones had removed a greater portion of the os calcis and astragalus for disease of the tarsus. This case is bidding fair to do well.

A case was shown to me presenting an admirable illustration of the result of amputation at the knee-joint, the patient being a little girl, who, some months since, had received a terrible injury of the leg, extending to the knee, and wounding the popliteal artery. Amputation was done through the knee. The result has been a perfect recovery, and the little girl has as good and as long a stump as can be seen in any Hospital.

Among other instances I saw in this little Hospital, was one which may be truly ranked with the best efforts of conservative surgery. It was that of a poor little boy, about 8 years of age, whose left arm, from the shoulder as far as the lowest point of the elbow, had been completely bound down to the side of the chest by firm adhesions, the result of a burn. By a series of surgical manœuvres, the arm has been completely liberated, and the boy in my presence clasped both hands over his head. The wounds made by the surgeon are perfectly cicatrised.

I saw also an illustration of the result of resection of the hip-joint; but it was in the form of a pathological preparation in the little Museum attached to the Hospital. It consisted of the parts about the hip, removed after death from a female adult, who had died from disease of some internal organ a year or two after excision of the head of the femur had been effected by Mr. Jones.

This preparation shows the upper end of the thigh-bone resting against the ilium, just above the site of the acetabulum, which has become partially obliterated, and bound down to it by a dense and tough fibrous tissue, forming almost a complete

capsular ligament. The patient could, after the operation, make use of this new joint with considerable facility.

The cases above alluded to indicate pretty tolerably the advance which Mr. Jones has made in conservative surgery during the last two or three years. The success which attends his operations is remarkable. There has been, I believe, only one fatal result after these operations, the case being one where he performed excision of the knee-joint; and here death was due to a violent dysentery, with which the patient was attacked during an epidemic raging in the island. It is evident that the very great success met with here is in a measure due to the salubrious air of the place.

It will be seen, by consulting the records of surgery during the last one or two years, that the opposition to excision of the knee-joint is not maintained with that violence which formerly held; for the operation in question has been executed by several London and provincial surgeons, and with such an amount of success, that the mortality arising from it has not been so great as that attending amputations of the thigh; and there are doubtless many who will not allow their minds to be prejudiced by incautious critics against this proceeding, but will test this operation in well selected cases of disease of the joint, when such opportunities occur.

It is not, however, to be expected, that in London such success should be met with as has attended Mr. Jones in Jersey, where everything so favourable to the recovery of patients obtains—cheap and wholesome diet, and, above all, pure air; nevertheless, the very excellent results which have followed these efforts in conservative surgery in the hitherto (professionally speaking) obscure island of Jersey, have doubtless induced many to cultivate that branch of their art with more assiduity; and possibly this brief sketch of what I have lately witnessed there, may stimulate others to inquire into fairly, and investigate in all its aspects, a subject which is worthy of the best attention of every surgeon.—*Med. Times and Gazette, Nov.* 4, 1854, p. 475.

45.—*Condition of a Joint after Resection.*—Mr. Henry Smith exhibited to the Medical Society the bones which had been removed from the lower extremity of a boy who had two years previously undergone excision of the knee-joint by Mr. Jones, of Jersey, had entirely recovered from the operation, and had died from disease of the liver a few weeks ago. A firm, bony anchylosis had taken place at the site of operation, between the extremity of the femur and the tibia, in nearly a straight position. The patella also, which had not been removed in the operation, was united to the anterior part of the femur, and its ligament was still attached to it, and to the tibia, so that altogether a very firm, compact mass of bony tissue occupied the situation of the knee-joint. This preparation demonstrated that bony union does take place between the ends of the bones after resection of the knee, and that a most useful limb may be saved. In the instance of this boy, as in that of others he had seen and heard of, there was great facility of walking. The patient from whom this most instructive specimen was removed was the younger of the two boys who had a year ago been exhibited to the Medical Society, and the fellows had seen for themselves how well he was enabled to use the limb. There was at that time some disease of the hip on the other side, and, since his death, it was discovered that dislocation upwards had occurred. This case was a sufficient answer to the objection that had been raised, of making the limb, by the operation, "like a flail."

Mr. Jones said he had no remarks to make, except to strongly urge upon surgeons in cases of operations for diseases of the knee joint, to save the patella, and not divide the ligament. When these parts were saved, there was less difficulty in walking than when they were removed.

In answer to a question, he said the boy from whom the preparation had been taken had not suffered from any tubercular disease, but had died from extensive disease of the liver, spleen, and kidneys. Two months before, he was quite well, and walked about with ease.—*Lancet, Dec.* 9, 1854, *p.* 484.

46.—ON EXCISION OF THE ELBOW-JOINT, &c.

By James Syme, Esq., F.R.S.E.

[Mr. Syme says, that twenty-five years ago, in all cases of incurable disease of the elbow, the established practice was amputation of the arm. This might have been so in Edinburgh, and even in London, and most other places, but from the testimony of Mr. Smith, senior surgeon of the Leeds Infirmary, we believe that excision of the elbow-joint was an established practice in that hospital for many years before that period, although the practice was not published in any book or journal. Mr. Syme, however, certainly has the credit of popularizing this important operation, and could not have known what was going on in Leeds when he himself perhaps was only a student. Mr. Syme attributes the ill-success which attended the early operations of this kind, by Sir Philip Crampton and others, to an error in regard to the extent of bone requiring removal. He says,]

In caries, little more than the articular surface is engaged in the disease, and the part thus affected is of very limited thickness; but beyond this, the bone is roughened, being covered by an effusion of osseous substance, which is not morbid any more than that thrown out to unite a fracture, or supply the loss of substance in necrosis. Moreau, father and son, were the surgeons who had performed the operation in France. Moreau says, in speaking of one of his operations, that finding the bone roughened, he cut off another inch of the shaft. Sir P. Crampton, for a similar reason, removed the bone "three inches above the tuberosity of the inner condyle." The effect of such proceedings is, to produce so great relaxation of the parts, as to allow the muscles little command over the limb, while at the same time the recovery is much protracted. It occurred to me that the results might be better, if the part really diseased were alone removed. In 1827, I had under my care a young gentleman of tender years, with disease of the elbow-joint, which had resisted all treatment. I wished to excise the articulation, but not finding any one in Edinburgh to sanction so unprecedented a proceeding, I reluctantly performed amputation. I have not seen him again till last year, when a tall, fine-looking man entered my room, and introduced himself as the subject of this case. I confess, I felt some remorse of conscience on looking at his empty sleeve; but I had the satisfaction of reflecting that this was the last case in which I was guilty of any such unnecessary mutilation. In 1828, I performed the operation on my own responsibility, and in 1831, published a treatise on the subject, having in the interval operated on fourteen cases. This mass of evidence attracted attention, and notwithstanding very violent opposition, the operation has since become more and more generally adopted, and at last even in London, which held out longest, it has during the last few years been occasionally practised. But while thus adopted, it has shared the same fate as another operation to which I lately alluded, the principles of its performance having been overlooked. There is no operation in which the mode of making the incision is of more consequence. Unless they be sufficiently free, the portions of bone cannot be removed without risk of injury to the remaining parts, and unless they be properly directed, the resulting cicatrices will interfere with the free motions of the limb; and we are not satisfied here with the result unless there be perfect flexion, extension, and rotation, with the full natural amount of strength. I believe the only incisions by which this perfect result can be obtained are those originally practised by Moreau, which are, a transverse one, carried across the back of the articulation immediately above the olecranon, from the ulnar nerve to the external condyle, and two longitudinal through the extremities of the transverse, so that the incision altogether has the form of the letter H. When the flaps have been raised, free access is gained to the articulation, and when the operation is completed, the edges of the transverse incision, if brought accurately together, generally adhere by first intention, instead of healing by granulation and adhering to the bone, so as to prevent free motion. Having dissected up to the flaps, do not attempt to remove all the articulation at once, as otherwise there will be a risk of cutting the ulnar nerve, with consequent deficiency of sensation and atrophy of the limb; but having exposed the olecranon, cut it off in the first instance, so as to get free access to the joint, then divide the external lateral ligament, and having pushed the ulnar nerve over the inner condyle, free the end of the humerus and saw it off on a line with the tube-

rosities; and, lastly, remove in succession the ends of the radius and ulna on a line with the base of the coronoid process. More than the extent thus defined would be unnecessary and injurious, while less would hardly remove the disease, and, even if it did, would incur the risk of anchylosis. In the cases you have happened to see there has been anchylosis, resulting in one from disease, in the other from injury, and therefore there was some deviation from the ordinary plan of proceeding, but its general principles were kept in view.

The operation of excision of the elbow was not employed for anchylosis till long after it had been adopted for disease, since I had operated more than a hundred times for the latter, before I did so for the former. Though often requested to excise the joint under these circumstances, I had not been prepared to interfere with the healthy integuments merely for the convenience of the patient. But at last a young man, from the Highlands of Perthshire, came to consult me with both arms permanently extended, one immovably so, and the other admitting of but a very slight degree of motion at the elbow; resulting in the one case, from fracture with dislocation, in the other from dislocation only. It is impossible to imagine a greater degree of helplessness than this young man's, who was obliged to seek assistance in the performance of the most ordinary offices. The case called loudly for interference, and I performed excision of one elbow. The result was an arm so perfect that it could not be distinguished from a sound one till his sleeve was removed, exciting some surprise at the Medico-Chirurgical Society of this city, where I exhibited him several years ago.—*Lancet, May* 3, 1855, *p.* 231.

[Mr. SMITH, senior surgeon to the Leeds Infirmary, denies that Mr. Syme was the first to introduce this operation into this country. The following extract from our 10th Retrospect, and the additional note from Mr. Smith to the Editor of the 'Lancet,' will explain this matter.]

"In '*Retrospect*,' vol. x., Mr. Syme says—'Previous to 1829, the operation of cutting out the elbow-joint had not been performed in Great Britain.' Since this paper was published, we have received a note from Mr. Smith, senior surgeon of the Leeds General Infirmary, from which we take the following extract, to show that the operation was performed in Leeds, by Mr. Stansfield, as early as 1817 or 1818. Mr. Smith says—

"'In 1818, I was town surgeon here, and had charge of the workhouse. During that time, a man about twenty-four years of age, named Jeremiah Butterworth, a chimney-sweep, died under my care in the workhouse of consumption. Knowing that excision of the elbow-joint had been performed with success upon him at the infirmary a year or two before, by Mr. Josiah Stansfield, then junior surgeon to the infirmary, I was anxious not only to examine the joint, but to preserve it as a pathological specimen. I asked the Board, and (in consequence of my explanation of the importance of the case) obtained its permission to remove the limb, which I took to my own house. After satisfying my own curiosity by dissection of the limb, I presented the bones of the new joint as a trophy to Mr. Stansfield. This gentleman died in the following January. After his death, Mr. J. P. Garlick had possession of them. I succeeded Mr. Stansfield at the infirmary in February, 1819. At that time excision of the elbow-joint was a recognized operation there, and had been performed by both my seniors,—Mr. Chorley and Mr. Hey. For a time it fell into disuse; but has been performed by both my present colleagues, Mr. Hey and Mr. Teale, as well as myself. The last operation I performed, the case was so unfavourable, that I was recommended, when the operation was half done, to finish by amputation; but I determined to risk the case, and had the satisfaction of succeeding, the cure being complete, with a very good power of flexion, extension, pronation, and supination.'"

I have this week called on Mr. J. P. Garlick, who was house-surgeon to the infirmary, in 1818; and he empowers me to say he has a perfect recollection of the case of Jeremiah Butterworth, and remembers its success.

There is another operation which Mr. Syme claims, and which goes by his name, —the Division of Stricture by Incision in the Perinæum for the cure of Stricture and Fistula in Perineo. That operation also has been performed in all proper cases by the surgeons of the Leeds General Infirmary for forty years past. I have, however, never seen it performed in cases where a grooved staff could be previously passed into the bladder, as we have generally been able to cure such cases by

milder measures, without the necessity of so severe an operation.—*Lancet, March* 17, 1855, *p.* 300.

[Mr. D. R. RANKIN says that Mr. Syme's historical account of excision of the elbow-joint is imperfect. He states as follows:—]

Previous to Mr. Park's cases of excision of the knee-joint, and his suggestions as to excision at the elbow-joint, (1782, translated and published in France, 1784,) the latter operation (excision of the lower end of the humerus) had been performed by Mr. Wainman, of Shripton, 1760; and again, twenty years subsequently, Mr. Justamond, of London, removed the head of the ulna. Moreau came on the stage in 1786, and here Mr. Syme commences his history.—*Lancet, March* 31, *p.* 349.

47.—*Excision of the Elbow-joint.* By W. FERGUSSON, ESQ., F.R.S.—[Mr. Fergusson operated more than two years ago on the patient whose case is here commented upon. The man now enjoys a fair amount of motion in the elbow-joint, although the elbow had been previously anchylosed in a very awkward position, and was useless.]

In that state the patient had applied to various hospitals, as also to a bone-setter, (who twice disturbed the anchylosed surfaces.) When the man came before Mr. Fergusson, the latter thought the only way of dealing with this stiff joint was to follow the method of Mr. Barton of America, who had cut out a wedge-shaped piece from the front of the tibia, and created, in some degree, a new joint below the anchylosed knee. Mr. Mackenzie had applied the same method to the elbow, and he (Mr. Fergusson) had thought that this operation was the surest means of giving some mobility to the stiff elbow-joint of this patient.

He accordingly had cut out a large portion of the end of the ulna, radius, and humerus, and the amount of bone taken away was rather considerable, as there had been much hypertrophy of bone, or rather formation of new osseous tissue. The operation was, in fact, a resection of an anchylosed joint. The subsequent treatment was simple, and the patient had left the hospital with the wounds cicatrized, and some motion in the joint. Now, two years after the operation, the mobility of the arm was considerably increased, and it was satisfactory to have a living proof of the great amount of benefit which may be obtained by the adoption of decisive measures in cases of this kind. The patient had been reported to be dead, but he stood here to prove the contrary.—*Lancet, Nov.* 25, 1854, *p.* 436.

48.—EXCISION OF THE KNEE-JOINT.

By DR. WM. KEITH, Senior Surgeon to the Aberdeen Royal Infirmary, &c.

[This patient was aged 14, and of a scrofulous constitution.]

Physical Signs.—The right extremity is wasted, the leg drawn up and fixedly bent at a right angle to the thigh. The knee is greatly enlarged, tender to the touch, and incapable of being moved in any degree, the attempt causing much suffering; the skin over the whole limb, leg, knee, and thigh, is so dry and scurfy, as to present very much the appearance of ichthyosis; there are two openings in the ham, one on each side of the popliteal space, from which a sero-purulent fluid is constantly discharged, and both openings communicate with the knee-joint.

History.—He states that seven years ago, when jumping from a dyke, he sprained his right ankle-joint; this was followed by a succession of abscesses, the last of which only healed up at the end of three years of constant suffering. Continuing well and at school for two years and a-half, he met with an accident eighteen months ago, which, bruising the knee, was followed by much pain and swelling of the joint. With little done deserving the name of treatment, six months of confinement to bed elapsed, when abscess from within the joint found exit by one of the openings in the ham, after which he was so much relieved as to be able to go out on crutches, his knee bent up as it is at present. The opening being very imperfect, a fresh collection of matter formed soon after, and burst at the opposite side of the ham, and these for twelve months have continued to discharge daily, the joint all the time much en-

larged, tender to the touch, and more or less painful on movement up to the present time. Pulse never under 120.

On consultation it was agreed to excise the joint.

Wednesday, 17th May, 10.15 a.m.—Under chloroform; the joint was opened by the semilunar incision of the late lamented Dr. Richard Mackenzie. In lifting up the flap so formed, osseous union, partly accomplished between the femur and patella, had to be broken up. 1 inch and $\frac{4}{8}$ths was sawn off the femur, and $\frac{4}{8}$ths of an inch from the tibia, making in whole 2 inches of bone removed. The surface of the patella was pared and smoothed, and the gelatinous, degenerated, synovial membrane peeled and dissected from the face of the flap. Three small vessels were tied, the limb straightened, and the wound, closed by six stitches, was dressed with ointment, compresses, and a many-tailed bandage, and laid into a well-fitting Macintyre's metal fracture-frame. After being put to bed, 20 drops of laudanum were administered. To have arrowroot and brandy for dinner. 9 p.m.—Stomach irritable, vomited the arrowroot, retains port wine and water. Limb lies comfortably, feels the frame press hard on the outside of the knee, which was at once remedied by a soft pad of lint. It added also to his comfort to have the heel set entirely free from pressure, accomplished by passing a long thickish pad under the tendo-Achillis. To repeat the anodyne at bedtime.

Oct. 30th, 166th day.—He walked to the hospital, a distance of nearly a mile. The limb is all but straight, the joint stiffly anchylosed, he lifts the limb about with freedom, and that without the slightest uneasiness. While walking he rests on the toes with confidence. The heel wants $2\frac{1}{4}$ inches to touch the ground when he stands erect with the pelvis squared, showing the height of heel which his boot will require to enable him to walk comfortably. The whole limb is plump, and he himself florid and healthy to look at. He has grown sensibly in height and stoutness since the operation, and he congratulates himself much on having saved his limb, whether it keep pace with his growth or not—a point which can be settled satisfactorily only after some years' observation.

Remarks.—No worse subject than the above John Reith could well have been selected for any major operation. A constitution saturated with struma, weakened by long suffering, and his body wasted by copious purulent discharges. The knee-joint disorganised, and the soft parts in the ham permeated in various directions by sinuses constantly distilling pus, yet on the complete removal of the one great source of irritation, viz., the diseased knee-joint, the constitution was enabled, by the aid of a generous diet, to rally, to re-establish healthy action, and repair every breach.

To the remarks appended to my former case, published in the 'Monthly Journal of Medical Science,' for April, 1854, p. 329, I will only add, that while performing the operation, it were well to clear away from the face of the flap the whole of the synovial membrane; healthy it cannot be, but whether healthy or not, bony union is desiderated, and any secreting surface of a serous character will retard, and cannot facilitate either adhesive union of soft parts, or *ossification of bones.*

Secondly. Much attention should be given to the direction of the saw when cutting through the femur and tibia, that these bones be cut on planes parallel to each other, else they cannot meet face to face when the limb is fairly extended. All important as it is for early union that cohesion should commence from the hour they come into contact, the line of section in the femur should be close to the edge of the articulating cartilage of the inner condyle, which will remove all the articular surface likely to be implicated in disease, and still leave a breadth of bony surface about equal to the cut surface of the tibia. The more nearly of one and equal extent the better for present union and for after use. I must again repeat, the patella is indispensable, if after strength and comfort is desired, and that no portion of flap should ever be pared off or lost in any way. Skin is always useful, and when even superfluous, is wonderfully retractile and accommodating.

In conclusion, it may be safely affirmed that the nature of John Reith's constitution, and the state of the soft parts around the knee, were alike unpromising prior to the operation; yet on the seat of disease and the source of constitutional irritation being removed, nature's powers were speedily called forth, and have accomplished healthy and solid union of bone to bone, so that the patient could walk with little assistance on the 99th day after excision of the joint. The present opportunity may be taken to state, that John Hay, the subject of the former operation, is at this date

skin-whole and sound, walking nimbly on a stiff straight limb, miles in a day, without the aid of staff or cane.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 524.

49.—ON CHRONIC INFLAMMATION OF THE HIP-JOINT.

By Wm. Coulson, Esq., Surgeon to St. Mary's Hospital.

[Hip-joint disease, of a chronic nature, is, perhaps, the most important of all articular affections. Mr. Coulson prefers the term chronic inflammation of the hip-joint, which may commence in and for some time be confined to any of the tissues of the joint. The early symptoms may easily be mistaken at first.]

At this stage, then, the only symptoms essentially connected with the disease are, pain and impeded motion, and these become modified or of greater intensity as the complaint progresses towards the second stage. The general health is frequently unaffected, though this, of course, will depend upon the state of the patient at the moment of attack, while the presence or absence of constitutional disturbance will mainly depend upon the low or active nature of the inflammation at the onset. At this period we do not discover any deviation from the natural condition of the affected limb; it is of the same length as the other; the nates are not altered in form, except under circumstances which I shall presently notice, and the spine is perfectly straight. Still, after some time, it will be observed that the patient, while walking, has a tendency to carry his limb straight, as if there were no joint in the knee, and to incline the foot either inwards or outwards. These are slight symptoms, but they should always awaken attention, and if a careful examination of the joint be now made, we shall rarely fail in discovering that it is the seat of disease.

I have already explained to you the best mode of conducting your examination in cases of suspected hip disease. Abduction of the limb generally excites more or less pain in the affected joint; but to avoid all source of error you should examine the patient in an horizontal position, on a flat, hard surface, as a table, &c. Grasping the foot of the affected limb in one hand, and placing the other hand on the knee, you rotate the head of the femur against the acetabulum, and the increased pain which the patient experiences will apprise you of the mischief going on within the joint. Pressure should always be made with the hand on and behind the trochanter major, and in the front, where the psoas and iliacus muscles pass over the articulation.

From the structure of the hip-joint, and the difficulty of avoiding undue pressure between the articular surfaces of the bones, you can readily understand how, as the disease progresses, the patient employs every effort to diminish the pain and distress arising from the necessity of throwing the weight of his body on an inflamed, and, perhaps, ulcerated surface. The greater part of the symptoms attending the second stage of hip disease are connected with this constant effort to keep the limb as free from motion as possible, and various physical signs are now added to the functional derangements just described. The whole limb becomes flabby and diminished in size, from the constrained state of repose of its muscles; but the diminution is best seen in the glutæi muscles, which are so intimately connected with the joint. On looking at the patient from behind, you will at once be struck by the appearance of the nates. The affected buttock always becomes flatter and more wasted than the one on the sound side; the marked division between the buttock and thigh is lost; the buttock on the affected side is lower by an inch or more than is natural, and the whole limb appears to be longer than the one which is free from disease. The wasting and altered form of the nates are easily explained by the forced inactivity of the glutæi muscles; but the real or apparent lengthening of the limb does not admit of such ready explanation, and has given rise to considerable discussion; some surgeons contend that the limb is actually longer than the other, while some affirm that the lengthening is merely apparent. The truth appears to be, that, in the greater number of cases, the affected limb presents an appearance of elongation; that in a few cases it is really elongated to a slight degree; and, finally, that the second stage may pass over without any change whatever taking place, either in the real or apparent length of the limb.

An actual lengthening of the limb may be produced by effusion of fluid into the

cavity of the joint. I have no doubt in my own mind of this sometimes taking place, but it is not of frequent occurrence, and the elongation thus produced seldom exceeds one third of an inch; the head of the bone cannot be depressed more than an inch without destruction of the round ligament. To understand the nature and causes of the apparent elongation of the limb, which so often characterizes the second stage of hip disease, we must examine the patient in the erect and recumbent posture, and take into account the influence exercised on the limb by the positions which he voluntarily assumes. I have already discussed the different points of this interesting question, but a brief notice of them here may be useful. If we examine the patient in the erect posture, we shall perceive at once that he supports the whole weight of the body on the sound limb; the thigh is flexed on the pelvis; the leg on the thigh; the limb is commonly rotated outwards; the pelvis is inclined, that is to say, depressed, towards the affected side, and at the same time carried forwards, the anterior spinous process being more or less in advance of that on the opposite side. Now, all these circumstances tend to give an appearance of elongation to the limb, which can be produced at will in the healthy body, and which, during hip disease, often makes the affected limb appear from three to three inches and a half longer than the sound one. Indeed, in some cases of disease of the knee-joint, where the hip is unaffected, but where the patient assumes the position just described, the limb has been found apparently elongated to the extent of two inches—a fact which clearly proves, that apparent lengthening may take place independently of any change in the articulation of the hip. The lateral inclination of the pelvis brings with it a corresponding depression of the fold of the buttock; and if you are curious enough to examine female patients more closely than is the custom, you will discover another effect of the same cause. One of the labia—that of course corresponding to the affected side—is much lower than the other. This is very remarkable in young subjects from the close juxtaposition of the parts. The other symptoms which accompany this second stage of hip disease are little more than aggravated states of the pain and impaired functions already noticed.

In the third stage the pain gradually increases in intensity, becomes very severe, and impedes every motion of the limb, particularly that of extension. The effect of a constrained and uneasy position may probably contribute to increase the original pain in the knee-joint, and from some recent dissections it would appear that the articulation itself is more frequently involved in the disease than we had formerly been led to think. The pain in the hip is often violent during the night, preventing rest; the slightest movement of the joint causes agony, and the patient is either confined to bed or hobbles along with difficulty upon crutches. In many cases, however, even at this advanced period, the pain in the hip is often masked until the precursory symptoms of suppuration make their appearance. Constitutional disturbance is now observed, perhaps for the first time, and ushers in this stage of the disease. The pain in the joint becomes aggravated, and the febrile symptoms continued; there are frequent shiverings, with painful startings of the limb; the rest is greatly disturbed, and the patient experiences great distress from starting and catching during sleep. Suppuration now takes place, sometimes in the cellular tissue external to the joint, sometimes within the joint itself, in which latter case the capsular ligament soon gives way, and the effused matter gradually works its way to the surface. It is not always easy to distinguish whether the suppuration has commenced in the cellular tissue or in the joint; but, however this may be, it sooner or later comes to the surface, and is either discharged by a natural opening or with the aid of art. In many cases inflammation attacks the cellular tissue interposed between the glutæi muscles; and the nates, which were formerly wasted, become tumefied and painful; at a particular point the skin now inflames, fluctuation can be distinguished, and if an opening be not made, the abscess discharges itself by ulceration. In other cases it descends along the outer and back part of the thigh, or an intra-articular abscess, having perforated the capsule of the joint, may take one of the various directions which I have pointed out in a former lecture. From the abundance of cellular tissue in the neighbourhood of the hip-joint, the quantity of matter discharged is always considerable at first, and it may soon give rise to hectic symptoms; but it is astonishing to what an extent patients are sometimes reduced, and yet recover. Arrived at this stage, the disease may terminate in one of two ways: the symptoms may gradually decline; the discharge of matter becomes less and less, until it ceases

altogether, the fistulæ heal up, the patient's health is slowly restored, and a cure is at length effected by anchylosis of the diseased joint. Or the symptoms may become aggravated, and signs of destruction of the joint then set in. During this period the various lesions which I have described in a former lecture succeed each other with greater or less rapidity, and by reflecting on their nature and extent you can understand without any difficulty many of the symptoms by which they are accompanied. In order to avoid repetition I may briefly state the different changes of structure now alluded to. The ligamentum teres is ulcerated and gradually destroyed by absorption. The capsular ligament also is thickened and softened, relaxed in some places, perforated in others, and at an advanced stage of the disease often almost entirely destroyed. Ulceration of the cartilages sometimes precedes effusion of pus into the joint, sometimes appears to be the effect of purulent inflammation of the other tissues. However this may be, the articular surfaces of the acetabulum and head of the femur are in all cases more or less damaged by ulcerative absorption, the extent of the destruction varying in nearly every case. Thus only small portions of the cartilaginous lining of the joint may have been removed, or the acetabulum is rendered deeper and wider by ulceration, while the transverse and cotyloid ligaments have likewise given way. In the head and neck of the femur every degree of destruction may be observed. The cartilage, for example, is either partially or wholly absorbed, loose portions of cartilage may be found in the cavity of the joint; or the head, and even the neck, of the femur may be more or less destroyed by ulceration and caries, with or without scrofulous deposit in the cancellous tissue of the bone.

Chronic inflammation of so large and important a joint as the hip is always a serious disease, and it must be admitted that in the majority of cases the prognosis is unfavourable. Still, many circumstances will modify this opinion. Thus the disease is much more manageable when it attacks young children and persons below fifteen or twenty years of age, than when it occurs in adults. This may depend on the circumstance that the disease in children is usually of a scrofulous nature; for scrofulous inflammation of the joints in young subjects, even when accompanied by abscess and extensive destruction of the joint, has a much greater tendency to heal than when the disease occurs at a later period of life. I have seen many children recover who laboured under unequivocal symptoms of hip disease in its various stages; and the experience of the surgeons attached to the Children's Hospital in Paris confirms this opinion of its curability, even under the most discouraging circumstances. That form of the disease which commences in the cancellous structure of the bone has, I believe, a greater tendency to terminate in anchylosis than the other two forms. The prognosis I should consider peculiarly unfavourable when the disease is attended at an early stage with extensive gelatinous degeneration of the synovial membrane. Any serious constitutional disease co-existing with the local malady must affect the prognosis, and if the patient exhibit symptoms of pulmonary tubercle, the case may be considered as altogether hopeless. The existence of numerous abscesses about the joint, particularly if they communicate with its cavity, is likewise an unfavourable circumstance, for here we have not the resource of amputation, as when other joints are affected. In the latter stages of the disease a favourable termination may be expected if the abscesses begin to heal and signs of commencing anchylosis present themselves.

The diagnosis of hip disease in its various stages is surrounded by many difficulties, for surgeons engaged in extensive practice are frequently called upon to examine patients who have been treated for hip disease, when the results of the cases prove that no such disease ever existed. I have seen many examples of this error myself, and you will find various others recorded in works on diseases of the joints; I shall therefore endeavour to explain to you at some length the complaints with which chronic disease of the hip may be confounded, and the characteristic signs by which it may be distinguished from them. The affections which bear more or less resemblance to hip disease are, psoas abscess, caries of the pelvis, inflammation of the sacro-iliac articulations, chronic rheumatism of the hip-joint, sciatica, and hysterical affections of the joint.

You are already aware how, in the early stage, hip disease seldom presents any other symptoms than pain and some slight impediment to motion, and hence you can see at once that nervous disorders are the most likely to stimulate this first stage of the disease, because their characteristic sign is pain, and the pain, if severe, entails

more or less difficulty of motion. What, then, are these painful nervous disorders? They are sciatica and hysterical affections of the joint. Sciatica is chiefly characterized by pain, so is hip disease at the commencement, but there are several circumstances by which the two complaints can be distinguished without much difficulty. The pain in sciatica becomes very severe soon after the onset of the disease, and though it may be first felt about the hip it soon extends down the thigh to the leg, following the course of the sciatic nerve and its branches. In hip disease, as I have told you, the pain as often commences in the knee as in the hip, perhaps oftener; it is seldom very acute at first, and artificial motion during this early stage never produces the violent aggravation of pain which is excited by moving the limb during a severe attack of sciatica. Again, patients affected with hip disease can go about tolerably well during the first stage, showing slight lameness only, whereas a person attacked by sciatica is soon laid up and quite unable to place the foot on the ground, much less bear the weight of the body. As the diseases progress their distinguishing symptoms become much more clear and decided. After the first smart attack, sciatica has a tendency to diminish, even though no treatment be employed; mere rest alleviates the pain, and in a week or two a manifest remission takes place, whereas the progress of hip disease is steady and onwards; and during the second or third stages no error of diagnosis can be committed.

Hysterical affections of the joint simulate incipient hip disease much more closely. Like sciatica, these affections are characterized by intense pain in the neighbourhood of the joint, together with a real or imaginary inability to move the affected limb; but on carefully examining all the circumstances of the case, you will seldom experience any difficulty in arriving at an accurate diagnosis. In the first place, the patients are almost invariably females; and whenever one of the fair sex complains of intolerable pain in a joint, with little appearance of organic lesions beyond a mere puffiness of the integuments, you may be pretty certain that her complaint is hysterical, for it has been calculated that at least four-fifths of the females amongst the higher classes of society, supposed to be suffering from diseases of the joints, are, in reality, affected with hysteria, and nothing else. You will not, however, rest content with this general inference, but direct your attention to the concomitant signs of hysteria, and compare any existing symptoms with those that are known to attend true hip disease at a corresponding stage. It is an established fact, that hysterical females are more subject than any other individuals to neuralgia, and it seems to me highly probable that the complaint now under consideration is hysterical neuralgia of the joint. Many of the characters of the pain lead to this conclusion. It is excessively violent from the commencement, and in this respect differs from the pain of true morbus coxarius. The patient also refers it to the hip-joint, and not to the knee, as is so often the case when the disease depends on incipient organic changes. The pain, again, is not limited to the affected articulation, but extends to the buttock and down the thigh, a circumstance in which it resembles sciatica. This last affection, indeed, is often nothing but rheumatic neuralgia. This general diffusion of the pain constitutes an useful distinction between the hysterical complaint and true disease of the joint. The irregularity of the pain, and the peculiar modifications of sensibility which accompany it, are highly characteristic, and at once point to an hysterical origin. The pain, though apparently intense, is not constant; it often suddenly disappears for a time, without any evident cause, particularly if the patient's mind be diverted from her condition; and in like manner, the more the sufferer's attention is directed to the part, the more she complains and appears to suffer. Besides this, the pain is absent during sleep, whereas the rest is always more or less disturbed by the pain of organic disease. The sensitiveness of the skin is truly hysterical; the patient shrinks from the approach of the finger, yet sometimes will allow you to press the joint in all directions, without complaining of much pain. The nates and the limb in general are not wasted, and there is no elongation of the limb. Sometimes, however, from the spasmodic muscular contractions to which these hysterical patients are very liable, the limb is drawn up and inverted, the trochanter major is prominent, the knee bent, and the thigh rests on the sound one. Here the case might, by a superficial observer, be easily mistaken for dislocation; but its history, and the possibility of bringing the affected limb into a natural position, will distinguish the case from one of true displacement of the bone.

The foregoing are examples in which functional disorders simulate organic dis-

ease in its early stages; in other cases, the difficulty of diagnosis may depend on the existence of some organic lesion in or near the joint, yet differing in its nature from the changes which constitute true hip disease.

It is evident that the more nearly these lesions resemble in their nature those of morbus coxarius, and the closer they are to the joint, the more numerous will be the points of resemblance between the two affections. Experience proves this to be the case, and shows that caries of the pelvis, inflammation of the sacro-iliac symphysis, and psoas abscess, are the affections most likely to be confounded with chronic hip disease. When caries affects any portion of the pelvis close to the hip joint, the functions of the articulation are impeded; pain is felt in the region of the joint, and abscesses form in its neighbourhood; but the general symptoms are always less severe than when the articular cavity is the seat of the disease; the pain, also, is less severe, and not so evidently located in the joint, the movements of which are not very materially impeded. In hip disease, the immobility of the limb, after a certain period, is a constant and characteristic symptom; whereas here, although the hip-joint may be stiff, and a disinclination to movement exist, certain motions can be effected, without inducing any great pain. The suppuration from caries of the pelvis is less abundant in most cases than the discharge from hip disease, and after some time the probe will detect the existence of caries in the bone, when all doubt as to the nature of the disease will be removed.

Chronic inflammation of the sacro-iliac symphysis often bears a close resemblance to morbus coxarius. In children, a true joint, furnished with its synovial membrane, exists at the union between the sacral and iliac bones, and the inflammation is of the same scrofulous character as that which attacks other bones at an early period of life. The symptoms follow the same slow and progressive course as in hip disease. At first the patient complains of pain in the lumbar region; in some cases the pain is seated quite close to the hip, and is referred to that joint; or it may show itself from the commencement in the knee—a circumstance very likely to deceive the practitioner, and induce him to refer the symptom to disease of the hip-joint. Lameness soon follows, and the symptoms, which, as you perceive, are the same as those observed at the commencement of hip disease, gradually increase in intensity. But this is not all; they soon give rise to other symptoms also, analogous to the effects produced by disease of the hip. Thus it will be observed that the patient avoids lying on the affected side: the nates are flattened and altered in shape; the muscles of the limb become wasted; the patient throws his weight on the sound limb; the affected one is apparently elongated, from depression of the pelvis on the affected side; the knee is slightly flexed, and the point of the foot directed either forwards or slightly outwards. As the disease progresses, the symptoms increase, and the elongation of the limb is followed by shortening, which adds another remarkable point of resemblance between this disease and true coxalgia. If not arrested at this stage, abscesses form in the neighbourhood of the posterior spinous process of the ilium, and open either on the back of the thigh or in the groin; the suppuration becomes abundant, and the patient is worn out with hectic. This unfavourable termination is, however, more rare than in hip disease, for the progress of the malady is arrested, or imperfect anchylosis takes place between the opposite surfaces of the iliac and sacral bones.

From this account, you will perceive many points of resemblance between the two affections: we have in both pain and lameness at the commencement; we have the same, or nearly the same, appearances of the nates and limb, the same position, the same lengthening followed by shortening, and the same formation of fistulous abscesses about the upper part of the thigh. Great attention will therefore be required to distinguish inflammation of the sacro-iliac symphysis in a young subject from hip disease, not only at its commencement, but in subsequent stages. The first point to which attention should be directed is the precise seat of the pain. In hip disease, the pain of the joint, when it exists, is most severe in the groin, or just behind the trochanter major; in the other affection, the pain is chiefly developed by making pressure transversely near the posterior superior spinous process of the ilium. When tumefaction exists, its seat is also different. In hip disease, the fulness is at first perceptible either in front of the joint or immediately behind the trochanter major; in chronic inflammation of the sacro-iliac symphysis, we never discover any tumefaction in the groin, but the puffiness or swelling is always seated over the pos-

terior spine of the ilium. In hip disease, the joint soon becomes fixed, and any attempted motion excites intense pain, whereas the thigh can be freely moved while the patient is lying down, in cases of sacro-iliac affections. The limb, in cases of this latter disease, can be extended without much difficulty; and although the patient may walk very lame, he puts the whole of the foot on the ground; while, in hip disease, the toes only touch the ground, and the heel is elevated. Finally, the shortening of the limb in sacro-iliac affections is always apparent, not real; but in hip disease, the shortening depends on destruction of the acetabulum and head of the femur, or dislocation on the dorsum ilii; it is real instead of being apparent.

Formation of matter about the region of the groin or hip, accompanied by pain and difficulty of motion, might lead to the inference of disease of the hip-joint in its third stage, and cases are recorded where psoas abscess has been mistaken for it; but attention to the following points will always enable you to distinguish the one complaint from the other. In psoas abscess the pain commences in the lumbar region, and is not felt on the groin until the disease has made considerable progress. Rotation outwards of the limb greatly increases the pain, whereas the reverse is noticed in cases of diseased hip-joint; the various positions of the limb, its elongation, shortening, &c., which characterise morbus coxarius, are observed in psoas abscess, and the peculiar effect of position, of coughing, &c., in increasing the swelling or discharge of matter depending on psoas abscess, is altogether absent in cases of hip disease.

There are still to be noticed two series of cases, one of which may simulate generally disease of the hip, while the other only resembles the third stage of that affection. The complaints to which I now allude are a peculiar shortening of the limb, produced by injury to the hip-joint, and chronic rheumatism, or, as it has been otherwise called, morbus coxæ senilis. It was formerly supposed that all cases of real shortening of the limb depended either on the hip disease which I have thus described, on fracture within the capsule, or on chronic rheumatism. But Mr. Gulliver has described, in the 'Edinburgh Medical and Surgical Journal,' (No. 128, vol. xlvi. p. 97,) some cases of shortening of the neck of the thigh-bone, depending upon other causes, and which are particularly worthy of attention. This peculiar lesion has been hitherto attributed either to fracture within the joint, or to the natural effects of old age; but Mr. Gulliver has clearly shown that it may occur in young subjects, as the result of injury to the joint, and under circumstances which give considerable resemblance to the later stages of hip disease. The history of these cases, as related by Mr. Gulliver, may be comprised in a few words. A young man receives an injury to the hip, from a fall probably, and immediately afterwards complains of pain in the joint. The pain is not very severe, but it continues, and lameness supervenes; these symptoms may go on for many months, the thigh wasting, and the limb slowly shortening; until it becomes perhaps two or three inches shorter than the sound limb.

Cases of the kind described by Mr. Gulliver bear a close resemblance to hip disease in its earlier stage, and at the onset cannot well be distinguished from it; but as the malady progresses the differential signs soon become evident, and a little attention will enable us to discover them. Although the limb is a couple of inches shorter than the sound one, and although the shortening be found to depend on organic lesion, the motions of the joints are perfectly free. Now this could never occur in the ordinary hip disease, and the reason why it happens in the cases now alluded to is that the synovial membrane and cartilages are quite free from disease. For the same reason also there is little or no pain produced when the opposite surfaces of the joint are brought into contact by pressing the head of the femur against the acetabulum; there is no tumefaction about the joint, abscesses never form, and the patient remains free from constitutional disturbance of any kind, although the limb is continually getting shorter and the lameness increasing until it becomes incurable. All these circumstances are incompatible with the notion of chronic hip disease, such as I have described it to you; and even at the earliest stage, while the pain and lameness are considerable, we cannot detect on examination any signs of inflammation going on within the joint. I should also mention that the patient, though he limps, puts the foot flat on the ground—another point of distinction between this disease and morbus coxarius.

A short account of the pathology of these interesting cases will enable you to understand better the symptoms by which they are accompanied. The synovial capsule of the joint, and the articular cartilages, are quite healthy; but the neck of the thigh-bone is much shorter than is natural, and forms nearly a right angle with the shaft; or the neck may be removed altogether by absorption, while the head is flattened and expanded. On dividing the parts, the shell of the neck, if any remains, is found condensed, and, in one case, the cancelli contained caseous matter. Sometimes the acetabulum is widened and remarkably shallow, corresponding to the alteration of shape in the head of the bone. These cases have been assimilated to the examples of interstitial absorption of the neck of the femur which takes place as a natural consequence of old age; but, in my opinion, they have a different origin, and should be referred to absorption—the result of osteitis, without any tendency to suppuration.

Analogous, in outward appearance at least, to the foregoing affection, is the chronic rheumatism of the hip-joint, which has been well described by Mr. Robert Smith and Mr. Adams of Dublin. This disease generally affects patients above fifty years of age, though it may occur at an earlier period of life. Its pathological characters are peculiar. The cartilage of the head of the femur is more or less destroyed, not by ulceration, but by a process which seems analogous to interstitial absorption; it looks, in fact, as if it had been worn away by the effects of long continued friction; and the points at which the cartilage is most extensively removed correspond to this notion of wearing down or *usure*, as the French surgeons term it. The neck of the femur is also shortened to a greater or lesser degree, and sometimes so much so that the change produced bears a strong resemblance to fracture of the neck of the bone within the capsule. The os femoris is expanded and flattened, while the acetabulum, which is likewise deprived of its cartilage, is rendered wider and deeper than natural. When the disease has continued for any length of time, the round ligament disappears, and the osseous surface, from which the cartilages have been removed, are covered by the porcelain-like incrustation, which I have described in a former lecture; the synovial membrane is not diseased, nor has suppuration ever existed, I believe, within or outside the joint, but the capsular ligament is generally thickened. Many of the symptoms of this disease are common to it, and to morbus coxarius, and hence the necessity of discriminating between them.

The patient first complains of pain about the joint, increased whenever he throws his weight on the affected limb; the limb itself is apparently shortened to the extent of two or three inches; and this, of course, produces considerable lameness, the limb and foot being rotated outwards. The trochanter major is more prominent than in the natural state, the muscles of the thigh are wasted, but not so those of the calf of the leg, according to Mr. Adams at least. The nates of the affected side are quite flat, and no trace exists of the lower fold of the glutæus. Here we have several of the symptoms which attend morbus coxarius; but the points of difference are sufficiently numerous to render our diagnosis easy. In the first place, if you make accurate inquiries touching the history of the case, you will generally discover that the patient experiences some rheumatic pains in other parts of the body, or has previously laboured under an attack of rheumatism—in a word, that he is of the rheumatic diathesis. In the next place you will learn, that the pain and stiffness of the joint are relieved by gentle exercise; when the patient has walked about for a short time, the movements of the joint become somewhat more free—a circumstance quite the reverse of what takes place in hip disease, accompanied by shortening, when indeed the patient is totally incapable of walking at all. The pain is not increased by pressing the head of the bone against the acetabulum, and this is a distinctive sign of much value. Finally, on careful examination, you will find that a great deal of the shortening of the limb is apparent, not real; the affected limb may appear to be three inches shorter than the sound one, yet on making accurate measurements of the limb in the manner I have already described, you will discover that two-thirds of the shortening, at least, depend on altered positions of the limb and pelvis. In addition to all this, the constitutional disturbance of true hip disease is never present, nor has the rheumatic affection any tendency to excite the suppurative inflammation which so frequently accompanies the later stages of morbus coxarius. I must, however, observe, that the shortening of the limb in rheumatic

cases, which I have just described to you, does not always depend on shortening of the neck of the thigh bone, and deepening of the acetabulum; it may be produced by actual dislocation on the dorsum ilii, and thus is added another remarkable point of resemblance between rheumatic affections of the joint and the ordinary disease.

I have still to consider the treatment of chronic hip disease, but a great deal appertaining to this part of the subject has been already noticed; I shall therefore conclude with a few additional remarks on certain points of treatment which were not sufficiently developed in my preceding lectures. In them I confined my account of treatment to scrofulous affections; what I shall now add is intended to apply to cases of hip disease occurring in persons who are not scrofulous. When speaking of scrofulous affections of joints, I expressed an opinion unfavourable to the use of active antiphlogistic measures in such cases, but this does not apply to hip disease in strong subjects, who present no signs of a scrofulous habit. Here, bloodletting must be employed, or at least blood must be taken by leeches or cupping-glasses from the neighbourhood of the joint. For the quantity of blood to be extracted, no fixed rule can be given; you must have regard to the strength and constitutional powers of the patient, his age, the amount of pain, &c., and you must regulate your conduct accordingly. At an early stage of the disease, and in the cases now spoken of, the best general treatment consists in administering calomel, combined with opium, until the mouth is affected. The disease is often arrested as soon as the peculiar effects of mercury manifest themselves; but some patients, as is well known, are unable to bear this powerful remedy, which irritates their bowels, and if continued, may excite dysenteric inflammation of the intestinal mucous membrane. Hence, if pain with tenesmus and scanty mucous stools be observed, an attempt should be made to allay the irritation by opiate injections, and, if these fail, the use of mercury must be immediately abandoned. Counter-irritation may be employed more freely, and at an earlier period in these cases than in scrofulous affections of the hip-joint. When the inflammation has been somewhat subdued by local bloodletting, I generally employ, with advantage, a succession of blisters over the region of the hip. In the second stage, also, counter-irritation must be continued. Some surgeons prefer blisters, while others have recourse to caustic issues behind the trochanter major. When the disease is evidently confined to the osseous tissues, much benefit has been derived from cauterization by means of moxas or the actual cautery. Should any severe inflammatory symptoms appear, we must have recourse to antiphlogistic remedies, and administer purgatives, combined with saline and antimonial medicines. In the cases now alluded to, absolute rest may be enjoyed without any great danger of injury to the general health; and hence the patient should be confined to his couch, while the whole limb is kept immovable by a proper apparatus. With respect to other points of treatment, I do not know that anything of importance remains to be noticed. During the third stage of the disease, the same method is applicable to all classes of patients.—*Lancet, Dec.* 16, 1854, *p.* 499, *and Feb.* 10, 1855, *p.* 148.

50.—ON SINUSES OF THE HIP DEPENDING UPON EXFOLIATIONS FROM THE PELVIS.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[The patient who gave rise to the following remarks had come from Canada to be under the care of Mr. Syme, who commences his clinical lecture by a few introductory words which are important. He says,]

It is necessary that you should recollect the distinction between necrosis and caries. In the former disease a portion of bone dies, and separates from the living substance, so that no obstacle to recovery exists after the exfoliation or detached piece escapes from the position where it is situated; but in the latter the bone retains its vitality, and obstinately remains in a diseased condition, without any natural limit of duration, except the life of the patient or conversion of the caries into necrosis. It must further be recollected that the dense osseous substance which composes the shafts of bones is chiefly liable to necrosis, and that the spongy or cancellated texture is almost exclusively the seat of caries. Now, sinuses about the pelvis are unhappily met with very frequently as the attendants or consequences of disease in

the hip-joint, vertebræ, or sacrum, where the disease, being of an incurable kind, and the part concerned not admitting of removal, any sort of treatment can produce no better effect than a very imperfect degree of palliation, and it has hence been usual to regard such cases as of a very hopeless character. But nearly thirty years ago there happened to fall under my notice a case which showed that such a judgment should not be passed as a matter of course, or without more caution and discrimination than had been supposed requisite. The patient was a young man, aged 28, who for the preceding seven years had suffered from sinuses about the hip and upper part of the thigh, which being regarded as proceeding from fistula in ano, had been treated by the late Mr. George Bell and other surgeons, without obtaining the relief desired. He had then applied to quacks with no better success, and finally abandoning all hope of recovery, had allowed the disease to pursue its course. It was a considerable time after this resolution that my assistance was asked. I found him extremely emaciated, and so weak that he could hardly leave his bed, with a large abscess of the thigh, and several sinuses about the hip, discharging matter profusely. Having opened the abscess, I examined the sinuses, and found that one, which opened in the fold between the buttock and thigh, led to the tuberosity of the ischium, in which there was a cavity containing an exfoliation of bone. I therefore dilated this sinus by incision, introduced my finger, and having found an opening between the origins of the extensor muscles, enlarged it by the bistoury, so as to obtain access to the interior, whence I removed a small bit of dead bone. The patient then quickly recovered, and ever afterwards enjoyed good health.

As there could be no doubt that the exfoliation had caused all the distressing symptoms of this severe and long-protracted case, it became very desirable to ascertain the origin of the evil, which could hardly be attributed to the ordinary influences of cold or external injury. Upon inquiry, it appeared that the patient had first experienced uneasiness after a day's employment in curing herrings, and that the discharge of this duty had required him to stand with his feet apart, alternately stooping and stretching his arms upwards to the full extent, subsequently to which he had felt a painful sense of fatigue in the back part of his thighs. It then occurred to me that the disease had originated from over-exertion of the muscles which arise from the tuberosity of the ischium, and that the bone with which they are there connected had suffered in consequence, so as to exfoliate, instead of being excited to preternatural growth, as Sir Astley Cooper, with apparently good reason, thought likely to happen, from inordinate muscular contraction. Several cases of a similar kind, which afterwards occurred, tended to confirm this suspicion; and in a paper on the subject ('Edinburgh Medical and Surgical Journal' for 1828) I thus expressed my sentiments in regard to the pathological explanation:—"The history of these cases will, I hope, effect the great object of this paper, which is to excite a more discriminating diagnosis and active treatment of sinuses of the pelvis. As to the origin of the exfoliations, I will not at present say much. It seems very evident that they cannot result from the direct effects of violence, since, in all the cases detailed, the bone concerned was securely protected by its situation from any such injury. In all of them (if we except the first, where no information could be obtained as to the origin of the complaint), there was violent muscular contraction, and I am inclined to think that this may have been the exciting cause of inflammation and death of the bone. The subject is curious, and worthy of investigation, but of little importance when compared with the practical benefit which may result from a knowledge of the fact that sinuses of the pelvis sometimes depend upon loose exfoliations, which will not find their way out unassisted, but which may be readily removed artificially, with the effect of a speedy and permanent recovery."

We may now proceed to the case which has led to these remarks. The patient is T. C., aged twenty-four, a blacksmith in Toronto, Upper Canada. About a year and a half ago he was trying how far he could leap, when his right leg slipped as he came to the ground, and went out sideways from him. He felt shaken at the time, but did not experience actual pain until next morning, when his right thigh was so painful at its upper part that he could scarcely move, the pain was deeply seated, apparently in the very middle of the substance of the thigh, while the upper part of the adductor muscles was very tender to the touch. The pain continued for about two months, at first preventing him from working, and greatly crippling him, even to the end of this time. A lump then began to form at the extreme upper and inner

part of thigh, a little anterior to the tuberosity of the ischium, and the pain gradually subsided. This swelling remained in nearly the same state for twelve months, when, having attained the size of a hen's heg, it was opened by a surgeon, and discharged about a tea-cupful of matter. Three weeks afterwards another abscess of larger size formed nearer to the groin, and was followed by several others of smaller extent, which were poulticed, and allowed to open spontaneously, while he lay in the hospital of Toronto, with apparently little prospect of recovery. Two medical gentlemen who had been educated at Edinburgh then became acquainted with the case, and having recognized its similarity to those pointed out by me as dependent upon exfoliation of the pelvic bones, advised the patient to seek my assistance. He accordingly travelled to New York, crossed the Atlantic, and was admitted to this hospital a week ago, (on the 30th Oct.) The patient is now before you. You observe that he is a well-formed, healthy-looking man, and that there is no curvature of the spine, or any other sign of disease, either there or in the hip-joint. There are several sinuses, one in the groin above Poupart's ligament, and others at the junction of the thigh with the perineum. Through one of the latter the probe passes towards the ascending ramus of the ischium, and there detects a loose piece of bone, which we shall now endeavour to remove.

[Chloroform having been administered, Mr. Syme dilated the sinus by incision, introduced his finger up to the origin of the adductor muscles, and having there felt a small aperture, enlarged it by means of a probe-pointed bistoury. He then extracted the exfoliation in three portions, which, when put together, fitted accurately, and constituted the inner margin of the pubic arch, the whole of which is concerned in affording attachment to the adductor muscles.]—*Lancet, Jan.* 6, 1855, *p.* 1.

51.—*On Amputation of the Thigh.* By JAMES SYME, Esq., F.R.S.E., &c.—Mr. Syme gives us the following hints on this subject. He says, "I have been long satisfied, that though in the upper part, or middle of the thigh, the flap operation answers well, yet, at the third, the circular, or a modification of it, is preferable; for, of course, the greater the distance from the origin of the muscles, the greater will be the amount of their contraction, and, therefore, the greater the necessity to guard as much as possible against its effects. Mr. Liston did not recommend operating lower down than the middle of the thigh, on the ground that the stump obtained by operating in that situation was long enough to preserve the use of the hip-joint, and any more was superfluous. I cannot quite agree with this; and I suspect that he, while giving this advice, did so, though unconsciously, under the impression that better stumps could be formed in the upper than in the lower third of the thigh. In this boy's case, I shall operate immediately above the knee, by a modification of the circular method, which I have employed for some time past—viz., by forming two short flaps of skin by semilunar incisions on the anterior and posterior aspects, then retracting the skin so as to expose two inches of the muscles above the angle of union of the flaps, and dividing the muscles in front as high as they are exposed, and those behind as low as they are exposed to compensate for their greater contraction. The next point is one of great consequence—viz., to retract the muscles an inch and a half or two inches before sawing the bone. The retraction of the muscles, previous to dividing the bone, is of great importance even in the flap operation, since it is not the length of the flap that prevents the risk of protrusion of the bone, but the height at which it is divided above the angle of union of the flaps. There is one advantage in operating low down in the thigh, which is perhaps of little importance here, where we have plenty of good assistants, but which is often of great moment in private practice—viz., that a tourniquet can be applied without inconvenience, which is impossible when the operation is performed in the upper part of the limb."—*Lancet, March* 31, 1855, *p.* 333.

52.—ON AMPUTATION AT THE ANKLE.

By JAMES SYME, Esq.

[The case which gave rise to the following clinical remarks of Mr. Syme was that of a man, 24 years old, in a most pitiable state of suffering from disease of the tarsus. Mr. Syme decided on amputating at the ankle.]

The reasons why there is so little danger connected with it are, in the first place, that a very small portion of the body is removed; even part of the integument of the foot is left—viz., the skin of the heel, which forms the posterior flap, and the anterior flap, which extends a short distance over the dorsum of the foot; in fact, little more is taken away than in Chopart's operation; and the shock to the system, so far as it depends upon the extent of the part removed, must consequently be less than from amputation of the leg. Another consideration is, that there are no large bloodvessels divided in the operation—only the anterior tibial, where it is much diminished in size, and the plantar branches being concerned; there is therefore little blood lost at the operation, or risk of serious secondary hemorrhage. In the third place, the tibia and fibula are divided through the cancellated texture, and you thus avoid the danger connected with opening the medullary hollow, especially where there is a disposition to phlebitis, while at the same time, the spongy texture not being liable to exfoliation, like the dense part of the shaft, that inconvenience is prevented. On these grounds I anticipated that this operation would prove a very safe one; and this expectation has been now confirmed by long and very ample experience. Accordingly, I performed the operation, in the present case, on the 13th of last month (February); and though I cannot undertake to say as yet that the patient is safe, yet he certainly is somewhat improved in general condition, and the stump is looking well. And, therefore, even if the patient should die, so far from this case leading you to think less favourably of amputation at the ankle, the improvement which has hitherto taken place will prove to you how very little disturbance of the constitution is produced by it. Patients, no doubt, may die after this amputation; but I believe they will hardly do so from the effects of the operation, if it be properly performed. In the first place, the incisions must be correctly made. A transverse one should be carried across the sole of the foot, from the tip of the external malleolus, or a little posterior to it (rather nearer the posterior than the anterior margin of the bone), to the opposite point on the inner side, which will be rather below the tip of the internal malleolus, but can be readily determined by placing the thumb and finger at opposite sides of the heel. If the incision be carried further forward, considerable inconvenience is experienced from the greater length of the flap; and I believe a great deal of the difficulty that has been attributed to the operation has arisen from this source—the operator getting into the hollow of the os calcis, cuts and haggles in striving to clear the prominence of the bone, with the desperate energy of an unfortunate mariner embayed on a lee shore in a gale of wind. Another incision is then to be carried across the instep, joining the ends of the former. The next point to be attended to is, that in separating the flap of skin from the os calcis, you must cut parallel to the bone. This is of the greatest importance, since, when the flap is detached from the bone, its only supply of nourishment must be the branches which run through its parallel to the surface; and if, instead of keeping parallel to the surface you cut on the flap,—as a butcher does when he skins a sheep,—you will, by scoring it in this way, necessarily cut across these branches. I have reason to believe—nay, to know—that the sloughing which has occurred in some cases, has been due to these defects in the performance of the operation; the flap having been cut too long, difficulty has been experienced in separating it from the calcaneum, and this has led to the scoring of the flap, which has been inevitably followed by death of a portion, or the whole of it.

The dressing is the next point to which I wish to allude. It is of great moment, particularly in a case like this, where the tissues of the flap are very soft and weak, to avoid as much as possible the application of pressure,—tight bandaging would of itself be sufficient to kill the flap. I accordingly use a very light dressing for the first few days; and, in the present case, the flap was covered only with a piece of wet linen.

This is all I wish to say at present regarding the operation; but, before leaving this subject, I will make a few remarks on certain alleged improvements which have been proposed. Some of these consist in differences with regard to the incisions, which have been made either diagonally, with the view of taking the flap chiefly from the inner side, or so as to split the flap into two lateral halves. Both these modes of operating are sometimes necessary from the circumstances of the case, and I have myself had occasion to resort to both; but I think that any one who has tried either

of them as well as the ordinary method will give decided preference to the latter, wherever it is practicable; and I cannot help believing that the very gentlemen who have proposed the diagonal and median incisions would themselves have suggested the transverse had it not been before in use. Not that I wish to be understood to imply that the suggestion of these modifications arose from a desire on the part of those who made them to differ from other persons; far from it; but regarding the operation of amputation at the ankle as a good one, and honestly striving to improve it, they have brought themselves to believe that the modifications above alluded to were really improvements. But the operation of a similar feeling would, I am convinced, have led much more readily to the suggestion of the transverse operation, had either of the others been that which was previously employed.

The next proposal to which I would allude is that of leaving the astragalus. My much respected friend, Dr. John Traill, of Arbroath, lately sent, through me, a communication on this subject, with an accompanying cast, to the Medico-Chirurgical Society of Edinburgh. Dr. Traill says very truly, that cases in which the astragalus could be left must very rarely occur, since disease affecting the tarsus at all generally involves the astragalus. But I think it would be very undesirable to leave this bone, even when it is not affected at the time of the operation, on account of its predisposition to caries; one of the great recommendations of amputation at the ankle being, that it removes all the bones which are liable to the disease. In Chopart's operation the calcaneum and astragalus are the only bones left, and though they appear perfectly sound at the time of the operation, yet the disease frequently recurs, as you may learn from the fact that I once performed three secondary operations, within the period of twelve months, upon stumps in which Chopart's operation had been practised. The bones I hold in my hand were removed in one of these operations; and you will observe that, as in all other cases of the same kind, anchylosis has taken place between the astragalus and os calcis, while the latter bone has been previously drawn up by the action of the gastrocnemius, so as to prevent the patient from resting on the proper part of the stump. The same preparation shows the portions of the tibia and fibula that are removed in amputation at the ankle. They include, you see, the malleolar projections and a thin connecting slice of bone, merely sufficient to ensure removal of the articulating cartilage. In preserving the astragalus, therefore, you leave a bone which is peculiarly prone to disease, but there is another objection to this proposal. When you take away a foot, you must have in view the providing some substitute for it. It is true, the patient may walk without one, but practically it is necessary that between the leather of the boot that will be worn and the end of the stump a cushion of some soft elastic substance be interposed, and if the stump were as long as the sound limb it would not admit of this. Now, it appears that, when you amputate at the ankle in the ordinary way, the stump that results is just long enough to give the instrument-makers sufficient room for the material of the artificial foot; so that if the astragalus be left the stump will be too long. Also, if you retain the astragalus, the short, easily-formed flap of the ordinary operation will be insufficient to cover it, so that there will be danger of sloughing of the flap, as I have before explained. For these reasons, therefore, I consider the preservation of the astragalus as undesirable.

The third and last modification is one lately proposed by Professor Peregoff, of St. Petersburgh, which on account of the distinguished source from which it emanated, has been tried in several quarters. The proposal is, to saw off the end of the os calcis, leaving it attached to the integument of the posterior flap, and then turn it forwards, so as to make it unite with the cut surface of the tibia. The objections to this method are, first, that the os calcis is a bone predisposed to caries, and it is, therefore, undesirable to leave any part of it; secondly, the operation is not facilitated by the change of procedure; but, on the contrary, considering the use of chain-saws, &c., involved, it is greatly complicated. Thirdly, the union of the osseous surfaces is not likely to take place easily. The fourth and principal objection is, that the integuments of the heel, which should form a cushion for the patient to rest on, are necessarily carried forwards, and the end of the stump is covered by skin, which is not designed for sustaining the weight of the body. I have, therefore, been consulted respecting the merits of this proposal, expressed a decided opinion against its adoption, and this advice has always been followed; but in some other cases, where it has been tried, the operators appear to have had reason for regretting that they did so.—*Lancet, March* 24, 1855, *p.* 307.

53.—ON AMPUTATION OF THE LEG.

By JAMES SYME, Esq., Professor of Surgery in the University of Edinburgh.

[Mr. Syme prefers amputating the leg about an inch and a half below the tuberosity of the tibia, where the bone is divided through the spongy texture, and the risk of exfoliation is avoided.]

It may here be proper to remark that I was at one period a very strong advocate for amputation by flap in this situation; but I have for many years adopted and recommended the method just mentioned. I feel no shame in making this confession; for it is no credit to any one to say that he never changes his opinions, which is as much as saying that he is incapable of improvement. If a man is continually varying his practice without good reason, he loses our confidence; but, on the other hand, as soon as a person is convinced of the superiority of any method of practice over that which he had previously advocated, he is not only entitled, but bound to adopt it. The arguments used in favour of the amputation by flap are, in the first place, that it can be performed with great rapidity; but this is now completely thrown overboard by chloroform, which makes it a matter of indifference whether the operation occupies half a minute or half an hour. In the second place, it is said that the thick muscular posterior flap provides a good cushion for the patient to rest on; and in operating on the dead body, the result of the flap operation looks very well; but in the living body the muscles are not passive and immovable, having, on the contrary, a great tendency to contract, and draw the flap away from the integument in front, and this, added to the weight of the flap itself, produces a great risk of protrusion of the end of the tibia. Few persons can have practised the flap operation without experiencing this difficulty in a greater or less degree; and, as a strong illustration, I may mention that I have in this theatre within a period of six months performed three secondary amputations for bad stumps, in which large masses of muscle hung down, a mere incumbrance, while an incurable sore existed over the bone. But there is another remarkable circumstance. Even in the event of success after the flap operation, the cushion is useless, as the patient does not rest his weight on the extremity of the stump; and while the short stump is much the most convenient for the ordinary wooden leg, in which the patient rests his weight upon the bent knee, as in this apparatus (this was the common pin leg), the curious fact is that the short stump is sufficient to allow the use of the knee-joint to be preserved. This was discovered by accident. A young man came here six years ago with the stump of an amputation performed at the middle of the leg, with a large ulcerated surface on its extremity, yet wearing an artificial limb, in which the use of the knee-joint was preserved, the stump being received into a cylindrical case. I remarked to him that he must suffer great pain in walking; he replied that this was not the case, and that he suffered pain only at night from pressure of the bed clothes upon the ulcerated surface. I performed a second amputation in the way I have described, and the patient is now before you. You see that he sits differently from a person with an ordinary wooden leg, the pin not sticking out straight before him, but being in the same position as the sound leg. You also see that in walking he does not make the end of the pin describe an arc of a circle, but that the use of the knee-joint enables him to move it forward in a straight line like the foot.

[The patient, on being asked to state what he could do with his wooden leg, said that he had walked upwards of thirty miles in one day with it, and yet was able to walk well the next day; also that, being an engineer, he was in the habit of mounting to places, in the manufactory at which he was employed, of such difficult access, that few of the other workmen would venture to them; he then gave proof of his agility by running briskly up and down the stairs of the theatre.]

You see that the stump is as short as one in this situation could well be, the tibia having been divided only one inch below the tuberosity, the stump being formed by anterior and posterior flaps, and these, although now of considerable substance, were formed of integument alone, for I was obliged to remove as much muscle as possible, to admit of a covering for the bones after removing the end of the former stump.

Mr. Liston was at one time a strong advocate for a short stump in the leg, and he even went so far, in some of his operations, as to cut out the head of the fibula; but

this is objectionable, because, with the head of the fibula, the attachment of the biceps is taken away, and the stump is apt to become permanently extended, and there is also a risk of extension of inflammation of the bursæ to the knee. Before I performed the secondary amputation upon this man, I said to the gentlemen here that I had discovered that he did not rest his weight upon the surface of the stump, but on the patella, and that therefore the still shorter stump that would result from the secondary operation would probably be still sufficient to allow him the use of the knee-joint.

The apparatus which this man wears consists essentially of a case for the reception of the stump, made of *tin*, which he finds cooler than any other material, with a wooden pin adapted to its lower part, the whole bearing considerable resemblance to an inverted quart bottle without a bottom; a broad piece of thick leather laces firmly round the stump; and at its upper edge, on which the patella rests, is a leather collar, to be received on the upper margin of the tin case; the leather case is fixed by a strap at each side to another broad piece of leather, which laces round the thigh just above the knee, and the whole apparatus is thus kept securely in position. It is a remarkable fact, showing the slowness with which improvements are received, that all those persons whom you see walking about so laboriously to themselves, and so inconveniently to their neighbours, after amputation of the leg, might walk like the man before you by the use of a similar apparatus.

The operation, as I performed it the other night, is as follows:—Take the tuberosity of the tibia, or the head of the fibula, as your guide, and make a short anterior and posterior flap of integument only, entering your knife about a hand's breadth below the bony prominence, at the point which is to form the angle of junction of the flaps at one side. Having dissected up the integuments to an inch and a half below the tuberosity of the tibia, divide the muscle behind about an inch lower down, to allow for their retraction, and then saw through the bones as high as the skin has been dissected up. The stump thus formed is, as you have seen, long enough to preserve the use of the knee-joint. The medullary cavity is not opened, nor do you cut through the dense bone that is liable to exfoliation, and the wound that results is small, and generally heals rapidly.—*Lancet, Feb.* 24, 1855, *p.* 203.

54.—*Diagnosis of Fracture.*—[Most surgeons dislike fractures of the long bones in children, especially when near the joints. The ends of these bones are more vascular and more disposed to ulceration than the middle parts. Mr. STANLEY makes the following practical remarks on this subject:]

I think 'Pott's fracture' (said Mr. Stanley, much to the amusement of the class) is the only one well understood by candidates coming up to the College. Those industrious gentlemen, the grinders, make them have Pott's fracture quite pat. Now, every broken leg is not Pott's fracture; it is, in fact, a rather insignificant accident, fracture of the lower third of the fibula, with dislocation of the tibia and eversion of the foot; it is rather a dislocation of a bad and troublesome kind, first explained very admirably by Mr. Pott, than an injury of such startling and very momentous importance as generally believed. A man jumps from a height, the foot twists outwardly, the foot is drawn up by the peronæi, and you have an injury easily recognised; but take care, when you are called to what may or may not be fracture of the upper end of the same bone and tibia, near the knee, with hemorrhage into the cancellous structure of the bone, or fracture without crepitus of the lower end of the femur, or what is nearly the same, swelling of the elbow, and a fracture not to be made out in the humerus,—in all these cases, I would advise you to act with great caution, and as if there were fracture really present. You will have, in fact, hemorrhage of the bone, and effusion into the joint, which, I would repeat again, is one of the most untoward complications of these injuries, leading, possibly, very often to false joints or ununited fractures, and reflecting no very conspicuous credit on whoever has the fortune to meet with such cases.—*Lancet, Dec.* 16, 1854, *p.* 506.

55.—ON FRACTURE OF THE THIGH.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[Mr. Syme thinks that too much attention is paid to attempts to overcome muscular contraction by extension, instead of preventing it by careful adjustment of the broken bones, and equable support of the limb.]

In children, fracture is usually in the upper third or the middle; in adults, commonly in the lower third; in old persons, generally in the upper part, through the trochanter or neck of the bone; but at all ages the treatment is extremely simple and uniform. A hair mattress is much preferable to a feather bed, which is so unequal and yielding, that I would rather treat a patient with a fractured thigh upon straw. Draw out the limb to its full length, which you ascertain by comparing the distance between the knee and spine of the ilium with that on the sound side; feel that the fragments are, so far as you can judge, in proper position; then apply at each side of the thigh a splint of wood, pasteboard, or leather, extending on the outer side from the knee to the great trochanter, and on the inner side from the pubes to the inner condyle. It is of little consequence which materials be used; wooden splints such as these (Gooch's) are very convenient for private practice, as they can be easily cut down with a penknife, and at once adapted to a limb of any size, from a thigh to a finger; pasteboard and leather, however, are more accommodating, and can be fitted with greater accuracy to the limb. The splints are retained in position by bandages, which should not be rolled continuously round the limb, but applied at intervals, in the following way: a piece, rather longer than twice the circumference of the limb, is doubled, and passed round; one of the ends is put through the loop, and tied in a bow to the other end. Three or four such "looped bandages" are applied along the thigh,—three being sufficient, if the splints be of wood, while an additional one is desirable with leather or pasteboard, on account of their softness at the time of application. The advantage of this method of bandaging is, that the state of the limb can be observed for the most part without disturbing the bandage; and if it becomes necessary to make a fuller examination, all that is requisite is to untie the bows, when the splints can be separated from the thigh without its being raised from the bed. The spasmodic action of the muscles is continued, in consequence of their being pricked by the sharp ends of the broken bones. Now, when these lateral splints have been applied, although the muscles are compressed, and the risk of displacement diminished, yet whenever the patient moves a joint of the affected limb he moves the broken bone, and it is therefore necessary to restrain the motions of the whole limb; hence the great advantage of Desault's splint, which, though powerless as a means of overcoming muscular contraction, is of inestimable value in maintaining the quiet of the limb; the patient cannot move his toes without moving the ankle also, nor the ankle without the knee, nor the knee without the hip-joint, so that if he moves his great toe there is a tendency to displacement of the fragments of the femur. It is curious that Desault has given a wrong representation of his own splint, and while describing it correctly, as extending from the false ribs to a little below the foot, has figured it as reaching up only to the great trochanter. Unless it extend to the false ribs, it has no power in restraining the motion of the hip-joint. In ordinary practice, the long splint is generally not made sufficiently broad; it ought to be equal in breadth to the diameter of the limb, or about four inches and a half for an adult; otherwise there is great difficulty in preventing eversion of the foot. The best means of fixing the limb to the splint is a sheet wrapped round the splint till a part equal in breadth to the circumference of the thigh remains unrolled; this is passed under the leg, and wrapped round it; after which its free margin is secured by strong pins to the part rolled round the splint, which serves also as a pad to the limb; it is also well to use a perineal band, not to produce extension so much as to act as an additional means of insuring the perfect rest of the limb; the foot being fixed to the lower end of the splint by a handkerchief passed round the ankle just above the heel, and crossed over the dorsum of the foot, the ends being passed through the holes or notches in the splint, and tied upon its outer surface; a broad bandage carried round the chest and upper part of the splint completes the apparatus. The sheet is far simpler and more effectual than rollers applied from the foot to the groin; two or three bandages, six yards in length, are required

for that purpose, and though they look very neat when first applied, they soon require re-application; the state of the limb can never be ascertained without either raising the thigh to take off the bandage, or cutting it up, and in the latter case the thigh must be raised in order to re-apply the bandage; whereas, when the sheet is used, on taking out the pins, and throwing the sheet aside, the limb is exposed undisturbed for examination. This apparatus has also the great advantage that it can be obtained extemporaneously; there are few patients, rich or poor, whose houses do not contain a piece of wood that would answer for a long splint, and a sheet or tablecloth to wrap round it.

I consider that it is a great error to teach surgery only adapted to hospitals. Complicated apparatus is, in my opinion, not so good as the simple; but even if the complicated means were the more effectual, the other ought also to be taught. But I would trust to the simple apparatus before you, rather than to the most complicated ever devised.

Some surgeons who would go along with me thus far, make an exception in the case of fracture of the thigh in children, saying, that the fracture being in the upper part of the thigh, the upper fragment must be tilted upwards by the action of the psoas and iliacus, and that therefore the double inclined plane is preferable to the long splint for children. This, however, is a merely theoretical objection, and ample experience enables me to testify that the long splint is as applicable for children as for adults. I admit rare exceptional cases, however; for instance, I have found it necessary to use the double inclined plane when the knee has been anchylosed in the bent position, and again in one case where the leg had been amputated. Other cases also do occur, though very rarely. A man was once brought to this hospital with a simple comminuted fracture of the thigh, for which the long splint was applied; the patient, however, obtained no relief from it, but suffered such extreme agony, that it made him even get out of bed and drag the splint after it. I confess he met with but little sympathy, being supposed to be affected with delirium tremens. After a while swelling of the limb came on, followed by ulceration, and protrusion of the bone in the ham, and I was obliged to amputate the thigh. On examination of the parts, we found that the fragments, instead of forming as usual a convexity forwards, projected backwards, and one fragment, as sharp as a needle, had penetrated the popliteal nerve, and obliterated the vein, so that the extreme pain and the swelling of the limb were both accounted for. It happened that a patient came in soon after with fracture of the thigh, with the fragments in the same position. My late house-surgeon, Mr. Bickersteth, told me that he could not make the bones lie straight with the long splint. We therefore used the double inclined plane with a perfectly successful result.—*Lancet, Feb.* 17, 1855, *p.* 173.

56.—*The Starched Apparatus in Fractures.* By CHAS. ROYSTON, ESQ., Liverpool.—[In support of Mr. J. Z. Lawrence's practice, Mr. Royston brings forward the practice adopted in Liverpool, in the Southern and Toxteth Hospital. He says,]—We apply a dry bandage next the skin; then two pasteboard (side) splints, made perfectly soft by soaking in hot water, and moulded carefully to the shape of the limb; and, lastly, a bandage which has been tightly rolled in a thick solution of starch. This becomes thoroughly dry and hard in about twelve hours, forming a very firm case for the limb. The patient is allowed to walk about on the following day, having his foot suspended on a sling, reaching from the neck to the foot, and being provided with a pair of crutches. The only cases in which we have any fear of employing it are young children of a strumous habit of body, whose skin is tender and irritable. In such cases we should immediately remove it if the little patient complained much of pain.

We often employ it in fracture of the shaft of the humerus, occasionally when the elbow-joint is implicated, and in one case of fractured olecranon it was used with very good result. It has been used here in fractures of the lower end of the radius, when there has been no great displacement, with success.

In fracture of the femur, the long splint is always kept on for a month prior to applying the starched apparatus, and the long splint (without the pad) for a day or two afterwards, until the apparatus has become thoroughly dry and firm.

Stiffness of the knee-joint often happens after fracture of the femur, if the joint has been included in the starched apparatus; we only carry the pasteboard splints as low as the condyles of the femur, and do not include the joint.

In oblique fractures of the leg, we sometimes use M'Intyre's splint for ten days or a fortnight, and then put the case up in starch.

The starched apparatus has been found to answer very well in fracture of the leg immediately above the ankle-joint.

I treated one case of Pott's fracture of the leg with it last year. The patient (a female) went out at her own request at the expiration of a fortnight, and ten days after the starch apparatus had been applied. She attended at the hospital at the end of the month, when the apparatus was removed. The bones of the leg were firmly united, and there was considerable freedom in the use of the ankle-joint.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 623.

57.—ON SUBCUTANEOUS OSTEOTOMY.

By Dr. P. Frank.

[Various have been the methods of attempting to rectify deformed bones.]

In the hands of Dupuytren, Gräfe, Dieffenbach, and Langenbeck, forced infraction of the callus has in numerous cases been attended with great success. Powerful extension and counter-extension being maintained, pressure was performed against the seat of fracture, either by manual force or by use of pulleys. In other cases, direct pressure was resorted to, either against the convexity or the concavity of the curve. The latter practice was particularly recommended by Dieffenbach, who fractured the bone in the direction of the concavity, over a wooden block, hollowed and well padded on its superior aspect. In order to fracture the deformed bone in the opposite direction, namely, towards the convexity, the machine of Booth, and the formidable dysmorphosteopalinilastes of Osterlen were invented. As above mentioned, success has often attended operations performed according to the methods described; but there is no lack of unsuccessful and even of fatal ones. It is obvious that, on account of the great density and firmness of callus, the bone can easily be fractured above or below the part designated, and that in cases where the fracture is produced in the right place, great apprehension must be entertained of laceration of muscles and integuments ensuing by the sudden and violent action with which the fragments are thrust against these parts.

If these operations are carefully conducted, these evil consequences will, in a majority of cases, be avoided; and if a moderate force is likely to suffice to produce infraction of the callus, they will still form the best resource of the surgeon, and deserve a trial before other measures are resorted to.

Where the methods above described should not succeed, or *à priori*, are not deemed practicable, as in cases where the callus is very voluminous and perfectly consolidated, or in cases of rachitic deformity where the sclerotic state of the bone is fairly developed, osteotomy alone can promise success. This was first performed by Wasserfuller, who, in a case of fracture four inches below the great trochanter, which had been allowed to consolidate with great deformity, cut down upon the angle of the bone, divided two-thirds of its whole diameter with the saw, and fractured the remaining portion over a pyramidal block.

In the year, 1852, Mayer, of Wurzburg, published an account of thirteen cases of various osseous deformities successfully treated by osteotomy, a word which was introduced by him into surgical nomenclature. Mayer cut down upon the bone, and varying his practice according to the nature of the deformity, performed either transverse, oblique, and semi-circular incisions through, or cuneiform excision out of the bone. The large wound in the soft parts, the free exposure of the bone necessary for the execution of this operation, the violent reaction resulting after it, render its performance too dangerous to admit of general adoption. These objections to the practice of osteotomy are successfully obviated by a new method, for which operative surgery is indebted to the inventive genius of Prof. Langenbeck, of Berlin.

It would be impossible to give a more lucid and condensed account of this brilliant

acquisition of modern surgery than by offering an uncurtailed translation of the paper published by Professor Langenbeck a few months ago.

I have lately seen the cases of Professor Langenbeck, and have assisted at an operation of the same kind, successfully performed by Dr. Wagner, of the Dantzig Hospital. The details of this case shall be given as an appendix to those operated upon by Professor Langenbeck himself.

"After the introduction of the narrow straight saw for the resection of bone, during the Schleswig campaign of 1848, the idea of performing subcutaneous resection could not fail to arise. In fact, several operations were made in course of this campaign which well-nigh deserved that name. I allude to the cases where I excised fragments of the humerus, exposed by suppuration, by aid of the narrow saw without any further lesion of the soft parts than a slight dilatation of the gun-shot wound.

"At that period I had already determined to perform subcutaneous re-section in other cases, and have since then frequently expounded the principles which were to direct its execution. This was particularly done in speaking about the case of ununited fracture of femur, where I cut the fragments with the narrow saw, and in several clinical lectures in the years 1851 and 1852, after failing to succeed in the forced rupture of two cases of osseous anchylosis.

"The patients were in both cases young and healthy individuals; the anchylosis was in one case the result of a penetrating wound of the knee-joint with consecutive suppuration; in the other of acute rheumatic disease of the same articulation. In both cases there was probably extensive osseous coalescence of the articular surfaces; and that of such firmness, that the employment of greater force would undoubtedly have fractured the condyles of the femur.

"The excision of a cuneiform piece out of the anchylosed joint in imitation of Barton, now appeared to be the only plan which promised success; but considering this operation highly dangerous, on account of extensive osseous wound, I proposed subcutaneous osteotomy in its stead. It was my intention to introduce a very narrow straight saw into a hole drilled through the anchylosed joint, and then to divide the bone so far, that a moderate force would afterwards suffice to straighten the limb. Both patients were ready to submit to the operation, if I would assure them that no danger would result from its execution. This I was not able to do, as osteophlebitis may be the consequence of any operation on bone, and both patients left the hospital. Another case of anchylosis where forced rupture proved unsuccessful, has since then not occurred; but in February, 1854, a case of curved rachitic tibia presented itself, in which the state of bone was such that forced infraction was utterly impracticable.

"I therefore determined without hesitation to perform subcutaneous osteotomy, but chose to defer its execution to a warmer season, erysipelas and pyæmia being at that time particularly prevalent.

"During the summer term of the present year, I have already performed subcutaneous osteotomy in three cases, and that with results so brilliant and encouraging that I believe myself justified in entertaining hopes that this new operation will supply a desideratum till now often experienced.

"The instrumental apparatus I made use of consists in—

"1. A fluted chisel-drill, two lines in breadth, attached to a centre-bit.

"2. A straight saw, 1¼ in. in breadth.

"3. A common strong scalpel.

"The operation having till now only been performed on the tibia, the execution was in all cases much the same.

"An incision, rectangular with the longitudinal axis of the bone, was made on the internal surface of the tibia, dividing the integument and periosteum at once.

"I then applied the drill in the centre of this incision, and perforated the bone transversely from within outwards. The cessation of resistance safely denotes the final accomplishment of the perforation, so that no danger of injuring the anterior tibial nerve and artery need be apprehended.

"The narrow saw is now introduced into the aperture drilled through the bone, and the greater part of the latter divided, by sawing, both in the direction of the crista or towards the posterior surface of tibia, without any further enlargement of the wound in the soft parts.

"The bone is directly straightened by fracture of the remaining osseous bridges, or this proceeding can be deferred till the consecutive inflammation and suppuration have subsided. In the first two cases, I followed the first plan, indulging the hope that the wound of the integuments would heal without suppuration; but, after having experienced that this could not be expected, I performed instantaneous fracture in the third case.

"Further observation will have to decide on the comparative merits of these proceedings; at present I am inclined to far prefer the latter.

"Two reasons have induced me to divide only a part of the bone by the saw; the first is the great difficulty which the small wound of the integuments would oppose to perfect division; the second and more important one is, that the great mobility of the fragments necessarily resulting after perfect division by the saw, is avoided by consecutive gradual fracture, which will always allow some osseous bridges to remain intact, rendering the fracture imperfect.

"Experiments and future experience will yet have to show the extent of the subcutaneous division of the bone necessary to permit of easy fracture of the remaining parts.

"In the third case which I operated upon, this act was attended with great difficulty, a powerful effort being required to fracture the hard and voluminous callus.

"This procedure will no doubt be rendered easier of execution, if the bone is perfectly divided from the centre towards the concavity of the curve, instead of allowing bridges to remain on both sides, as I have done, till now.

"As to the local and constitutional effects of the operation, they are so insignificant, that subcutaneous osteotomy cannot be compared with a resection of bone, exposed by a large external wound. The pain experienced directly after the operation, and the febrile re-action, are scarcely more considerable than after simple fracture.

"Three or four days after the operation the soft parts around the wound are slightly inflamed and painful on pressure.

"Closure of the wound by first intention happened in none of the cases operated upon, suppuration supervening from the sawn surfaces of the bone.

"In the second case the discharge was not inconsiderable, and a fortnight elapsed before it subsided.

"Suppuration ceases as soon as the drilled canal and the saw-wound are filled with granulations; the external wound is then rapidly closed, and the bone is in the same condition as in consolidatory simple fracture.

"I regard it as no inconsiderable advantage connected with this operation, that, whether it be performed to straighten a bone after falsely united fracture, or a curvature of the bone after disease, or an osseous anchylosis, the normal position need not be perfectly restored at once, but can be gradually effected during the consolidation of the bone; thus rendering it possible to avoid any undue contusion and laceration of the soft parts.

"From the experience till now collected on the operation, I think myself justified in drawing the following inferences:—

"1. Bone can be subcutaneously divided, like tendons and muscles, and subcutaneous osteotomy bears well-nigh the same relation to resection with extensive division of the soft parts, as subcutaneous tenotomy to division of the tendons after dividing the integuments.

"2. Partial subcutaneous division of the bone by the saw is preferable, because perfect mobility of the fragments is thereby avoided.

"Union, by immediate formation of callus, as in simple fracture, cannot be expected after subcutaneous osteotomy, because the small particles of bones detached by drill and saw act as foreign bodies, which must be eliminated by suppuration."—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 612.

58.—ON DISEASES OF THE BONES.

By John Bishop, Esq., F.R.S., &c.

[In this paper, Mr. Bishop enters on the subject of *exostosis*, such tumours as are situated immediately under the periosteum, and are at first disconnected with the

bone, but nevertheless, after having been formed for some time, become sooner or later firmly united with the contiguous bone. They are liable to form on the internal table of the bones of the head.]

A tumour of this kind presents to the eye the appearance, and to the touch the form, of a nodule, with an abrupt margin, and is frequently separated at its base from the subjacent bone, by a furrow of variable thickness; and sometimes there is a deep fissure between the nodule and the bone lying beneath. The density of these compact nodules is very great, and in this state the disease is termed the ivory exostosis. These ivory tumours are of greater density and specific gravity than the normal bone with which they are associated, unless they happen to be connected with bone already in a state of induration. They are always composed of laminæ, and seem never to be intermixed with spongy tissue. The density of the tumour is owing to the greater number of laminæ contained in a given space, when compared with the number of laminæ found in the same space in normal bone. The Haversian canals are small, and few in number, but a well-defined lamellar system is found surrounding them. The bone corpuscles are irregularly scattered in the substance of the tumour, and in some places they are clustered together, while in other parts larger tracts are found entirely destitute of them. The colour of these exostoses is of a yellow-white, and they are of a lighter hue than the bone to which they are attached.

Rokitansky appears opposed to the hypothesis that the origin of these tumours has any relation to syphilis, and he believes the cause of their appearance to be entirely unknown. When the exostoses are of a spongy texture, they are connected with a rarefaction or expansion of the spongy bones, termed by the German pathologists *osteoporosis*. They present many varieties in their structure, and may arise either from the spongy or from the laminated structure, or from both together; they are then compounded of an internal spongy, and an external laminated layer of the compact tissue. These mixed osseous tumours are not unfrequently found near the joints, at the head of the tibia, or on the shin, and on any of the other long bones of the skeleton. In the skull, the disease is accompanied with an expansion of the diploe, in which case there may be an exostosis on both the external and internal tables corresponding in situation with each other. In the spongy forms of exostosis, the structure may remain permanent after its development; but more commonly new matter is formed in the interior, and the structure of the bone is more or less condensed; but this altered structure is often succeeded by the bone being again rarefied, and thus the growth of the spongy exostosis outwards may be affected, and may increase to a considerable size. When this disease affects the spongy bones of the face, it has been known to produce the most hideous appearance. Some examples of these cases are to be seen in the Hunterian Museum. When the exostoses have acquired a certain magnitude, they usually continue through life unchanged. The ivory texture of the excrescence, in this disease, sometimes diminishes in size by condensation, or it may become necrosed, and thrown off.

The spongy exostoses sometimes degenerate into caries, and in this way disappear. These appear to be the processes by which the system may be spontaneously released from the osseous growths of exostosis. Independently of exostosis, the bones during inflammation exude a fluid which ossifies, and forms a layer on their compact surface; this substance has received the name of *osteophyte*. At the commencement of this process the exudation is soft and gelatinous; it afterwards becomes tough and elastic, resembling cartilage, and in the end it ossifies. It appears that this exudation is associated with nearly all inflammatory conditions, whether of abscess or necrosis of the bones. There are many variations of form assumed by these osteophytes, as well as differences in structure.

They sometimes appear to be composed of "delicate fibrils and lamellæ, which are fixed at acute angles on the surface of the bone, and give it the appearance of velvet, or felt with a very fine nap." Though these osteophytes are at first in contact with the bone, yet they may be easily raised from it in large pieces, and although for some time separated by a layer of cellular substance, they become fixed to the bone when this membrane disappears.

The exudations which terminate in osteophytes sometimes form plates, and often anchylose some of the vertebræ, by the ossification of the anterior common ligament.

They occur also in the inner table of the skull, and, indeed, wherever inflammatory processes are going on in the bones.

Osteophytes have been known in a few cases to cover large portions, or nearly the whole of the skeleton; they are sometimes associated with atrophy, and at others with hypertrophy, of the bones. Although we generally find the exudations accompanied with an inflammatory condition of the bones and periosteum, the precise condition of the system that gives rise to the inflammation and exudation is not yet determined. In the imperfect union of fracture, these plates then constitute the union of the ends of the bone. To give a minute detail of the structure of all the forms of osteophytes would occupy too much of your attention in this short course; I have therefore merely touched on the subject to show the distinction in their character which has led to their separation from exostosis.

The bones of the skeleton are likewise liable to anomalies in number, size, and form. The absence or addition to the number is not very common, but is coincident with conditions of the system in a state of perfect health, and not referable to any pathological conditions: it is not, therefore, my purpose to give any detail of cases of variations of number. With regard to variations in size depending on pathological states of the bones, the case is quite different, and deserves a few remarks. In hypertrophy of the bones, they may increase in size by the addition of new osseous matter deposited on their surface beneath the periosteum, without any change taking place in their density or specific gravity; the size of the medullary canal remains the same, but the compact substance acquires a considerable augmentation of thickness. When the increase of substance takes place in the interior of the bone proceeding from the Haversian canals, and involving the whole of the medullary system, the bone becomes more dense, both in the compact and cancellous tissues. In the latter, the walls of the cells increase in thickness, and the medullary cavity diminishes in size; the diploe disappears, and the whole bone becomes indurated. When the hypertrophy takes place both internally and externally, the bulk and density of the bone are both increased. The increase of the bones, both internally and externally, is preceded by the deposition of a superabundance of cartilaginous matter, in which the salts of lime are deposited, as in normal bone.

When the disease attacks the bones of the skull, or when it affects considerable portions of the skeleton, it becomes a serious disorder. It is a remarkable circumstance, that when the bones of the skull become hypertrophied, those of the face often diminish, so that there are actions opposite in their effects—namely, hypertrophy and atrophy, going on in the system at the same time.

The conditions of the system that give rise to these changes in the bone are not yet determined, nor do the bones themselves give any promonitory signs of the advent of the disease. The surface of the bone continues smooth, and the periosteum natural, even when the bone has acquired the compactness of ivory. In other cases, the increase in the volume of the bone is preceded by an inflammatory condition. The seat of the inflammation may be either the periosteum and the compact tissue, or the medullary membrane. When the external surface is affected, an exudation takes place on the surface of the bone, which becomes ossified into laminæ: this forms a layer, which is sometimes separated from the surface of the bone by a layer of spongy tissue. When the inflammatory process has its seat in the medullary membrane, it leads to increased density either of the compact or of the spongy tissue, or of both these structures. It may be easily imagined that these organic changes cannot take place without affecting the texture of the diseased bone; and it is found accordingly, that the substance of the bone becomes hypertrophied in consequence of the expansion and infiltration of the tissue connecting the capillaries of the medullary canals and cells. The result of these organic changes is a thickening at the affected portions, the other sections of the bone remaining free, so that the surface often presents a rough and uneven appearance. The inflammatory state of the bones under consideration is frequently experienced by persons labouring under a rheumatic, syphilitic, or gouty constitution, and the treatment must depend on the nature of the causes that are associated in the production of the disease; the local treatment is that common to other organs affected with chronic inflammation.

The opposite state to that of hypertrophy, is atrophy of the bones. Whatever causes lead to the undue nutrition of bone, tend at the same time to diminish its

volume. There are, consequently, many states of the system which may lead to atrophy, such as indifferent diet, want of action, exhausting diseases, palsies, fractures, anchylosis, and, indeed, anything which tends to impede the flow of blood to the bones, are all accompanied with atrophy. From the nature of the various causes just mentioned, any of the bones of the skeleton may be affected. In many cases, the bones diminish both in length and thickness, and the medullary canal becomes contracted; this condition of the bone is termed *concentric atrophy.*

Independently of special causes, the bones of aged persons often become atrophied; they are then brittle, and break with a comparatively slight force. It appears that atrophy always begins in the medullary canals and in the diploetic structure, the cells of which enlarge, and the walls and lamellæ of the cancellous structure become attenuated, and finally disappear. The compact tissue becomes changed, and resembles the spongy diploetic structure; and the outer layer only remains unaffected, but almost as thin as paper. As the atrophy of the spongy substance advances, the external layer only remains, and incloses a cavity, with mere traces of spongy tissue at its periphery, or a soft substance with large cells. When the spongy substance is entirely removed, the thin external walls of the bones approach each other, and form a single plate. When the cavity within the bone is enlarged, it is called by Mr. Curling *ex-centric atrophy.*

The con-centric form of atrophy occurs in the larger medullary canals, the ex-centric in the bones of the pelvis, ribs, and vertebræ. It will be easily imagined, that when such great organic changes take place in the bones, their physical characters alter in proportion; and accordingly, the bones thus affected become flexible, and crack when they are bent. As the loss of the internal portion of the bone proceeds, the external portion becomes diminished, and hence the skeleton in *senile* atrophy, together with the whole weight of the body, is less than in the normal state. The volume of the bones may be also diminished by continual pressure, such as that produced by tumours and aneurisms; but to these purely mechanical causes I do not wish to engage your attention.

The following proportions were found to exist between the inorganic and organic matter, in a healthy, thickened portion, and in an exostosis formed on the same bone, analyzed by Lassaigne:—

	Healthy.	Hypertrophy.	Exostosis.
Inorganic	57·	58·4	54·
Organic	43·	41·6	46·

Having now given a description of the nature and growth of osseous tumours, I shall occupy a few spare minutes of your time with some very brief remarks on the nature and diagnosis of the soft tumours affecting the bones. Perhaps there is no subject in which the microscope has been of greater utility to the practical surgeon, than in determining the structure of the soft tumours of the osseous system. Up to a very recent period, tumours of the bones of the most heterogeneous and diversified character have been assembled together, and no distinction has been made between those of the mildest and those of the most malignant tendency. Thus, the older writers grouped together, under the term "*osteo-sarcoma,*" the cartilaginous and osseous, the osteoid, the myeloid, and the enchondromatous tumours, and confounded these non-malignant with the scirrhous, medullary, and alveolar tumours. Under these circumstances, we cannot wonder that much discussion should arise amongst surgeons on the propriety of, and the varied success resulting from, the removal of these mixed forms of tumours.

When we look into authors on osteo-sarcoma, we find that they had no very precise idea of the nature of the tumours comprised under this term. Some wished to restrict the term to those which are decidedly scirrhous, while others describe them to be of a cartilaginous texture, and none seem to have satisfactorily determined whether they are or are not wholly malignant. With such a state of confusion and complication, we need not be surprised that some operations have been attended with complete success, while in other cases the diseases have returned and destroyed the patient. To give an idea of the complete difference in the views entertained by surgeons on this subject, it is only necessary to refer to one or two of those who have confounded together the malignant tumours with those of the non-malignant character.

Thus, according to Callisen, osteo-sarcoma is a disease by which the texture of the bones is converted into a lardaceous substance, having a tendency to carcinoma. Boyer considers it a disease analogous to cancer. Dr. Cuming, of Glasgow, considers that though all varieties of osteo-sarcomatous tumours are highly formidable, yet he says "they are not all really cancerous." Mr. Mayo observed of these tumours, that they have not "much malignity;" so that if all the portion of the bone involved, with part of the adjacent sound bone, be removed by amputation, the complaint seldom reappears, either in the part, or in another bone. It would be an almost endless task to enumerate the different opinions, with the various results of these tumours; but we see how little dependence can be placed on the results respecting the cases that have been detailed, unless we could be secure of the real nature of the tumour removed. But the diagnosis of the character of the tumour is in some cases by no means easily made, and I have occasionally been in consultation with the most distinguished surgeons of the metropolis without arriving at anything like a satisfactory result.

The diagnosis of the nature of many tumours connected with the bones is one of the problems in surgery most difficult to solve; the data in many cases are not sufficient for the purpose. The hereditary tendency to diseases of an organic type, the aspect of the patient, the situation of the tumour, its character to the touch, and the history of its formation, are all circumstances to aid in the research; but all these are often insufficient to enable the surgeon to arrive at more than an hypothetical conclusion. An example will suffice to show the truth of these remarks.

A few years ago I was solicited by a gentleman to remove a tumour situated at the back of the thigh. On examination, I found it was deep-seated, soft to the touch, and apparently moveable on the bone; it was of a considerable size, and had only been recently detected by the patient. Having some suspicion of its character, I advised his having another opinion before its removal, and Mr. Guthrie was consulted; but that gentleman being of the same opinion as myself—namely, that it presented a formidable aspect, it was agreed to take the opinion of the late Sir Astley Cooper. In this consultation, Sir Astley stated, that although he was of opinion it was very likely to turn out malignant, yet that its removal would give the patient a greater chance of a prolonged life. However, considerable alarm having been excited in the minds of the patient and his family by these consultations, he was advised to take the opinion of Mr. Lawrence, and subsequently that of the late Mr. Earle. The result of their several opinions was, that it was a tumour of an uncertain character, but all agreed on the propriety of its removal.

At length the family decided that Mr. Earle should be selected to perform the operation. On cutting down on the tumour, it was found to be a medullary cancer connected with the bone, with ramifications so extensive that its complete removal could not be accomplished. The consequence was that the wound did not heal, and the patient sunk from exhaustion at the end of about three months. In this case it will be observed some of the best surgeons in London were consulted, notwithstanding which nothing but an uncertain knowledge could be formed of the character and connexions of the tumour, and it was, as the result proved, a case which required either amputation at the hip-joint, or to be left unmolested.

In order to assist us in forming a correct diagnosis of the nature of tumours, it has been suggested, and attempts have been made, to ascertain their character by means of a grooved or hooked probe, but with what success this plan has been attended I am not prepared to state; still it seems to hold out a feasible prospect of enabling us to form a better opinion of such cases before operating for their removal. It is the opinion of many surgeons that malignant diseases in the bones are not so liable to return after operation, as those of the soft parts; but as these opinions were given when the real nature of tumours was less understood, it will require further investigation to determine this question. Myeloid and enchondromatous tumours were, until very lately, considered malignant; and this may have given rise to many of the opinions formed of the curability of the patient suffering under malignant tumours of the bones by means of their removal.—*Lancet, Dec.* 16, 1854, *p.* 500.

59.—CASE OF MALIGNANT DISEASE OF THE ILIUM.

By W. Fergusson, Esq., F.R.S., Surgeon to King's College Hospital.

[The following interesting case gave rise to some valuable clinical remarks by Mr. Fergusson.]

T. B., aged 56, a bookseller at Birmingham; states that he has always been very healthy, although subject latterly to great debility. About last Christmas—that is, nearly four months ago—he began to notice a pain in the posterior part of the left haunch bone, which was at first trifling, though increased by the slightest exercise, and always relieved by rest. The pain gradually increased in severity until six weeks since, when it came on rather more sharply than usual, and then it was that he noticed a swelling in the situation of the pain. This swelling was then as large as the top of his thumb; and the pain, when it commenced, was, according to his feelings, situated in it. He moreover states, that, when he first discovered the tumour, he found it pulsate distinctly. From its appearance to the present time the tumour has been steadily and slowly increasing. Present condition:—The patient is in a state of great debility, which has been slowly coming on for the last two years. Bowels much confined; appetite good; sleeps well; pulse 90 and weak. There is a tumour situated on the posterior part of the left dorsum ilii, above and slightly over the great sacro-sciatic foramen. The crest of the ilium can be felt distinctly above it. The dimensions are, five inches in one direction, and four in the opposite; its circumference is nearly circular, and it stands out from the surface of the body about an inch. When the fingers are placed upon the tumour, it may be distinctly felt to pulsate, each pulsation being synchronous with one at the wrist. If the stethoscope be applied to it, a blowing sound in successive puffs is plainly audible. It is not tender to the touch. The skin over it is freely movable, and not discoloured. A bladder filled with ice is kept for two or three consecutive hours over the tumour, till the skin becomes quite numbed. This has a very decided effect on the tumour, causing it to become hard, with the pulsation diminished.

From the history, I think you would naturally suppose the case to be one of aneurism of the gluteal artery; and for such it was taken by the surgeons who sent the patient to me from Birmingham; indeed, so certain were they, that it was considered a proper instance for ligature of the internal iliac artery. I have never seen an aneurism of the gluteal artery, and I believe spontaneous aneurism of that artery to be extremely rare.

As a young surgeon, I was early called upon to give my opinion regarding a somewhat similar tumour, situated on the back of the pelvis. My particular attention, I remember, was directed to the great exhaustion and emaciation of the patient, such as is seldom found in conjunction with ordinary aneurismal disease. Ligature of the common iliac artery was suggested, but I did not sanction its performance. The patient ultimately died; and the disease was found to be, not an ordinary aneurism, but a large, soft tumour of bone. Within the last twelve months, I have been consulted concerning a pulsating tumour in the popliteal space, greatly resembling a popliteal aneurism; but, from the condition of the patient, and other symptoms, I was led to believe the disease to be a malignant tumour of bone, in which opinion I was supported by Sir B. Brodie. The patient has since died; and the opinion I offered was found to be correct.

But to return to the case under consideration. This person had been sent to me as a private patient; and, on my first visit, I found him in a very weak and emaciated condition, with various important indications sufficient to raise doubt in my mind as to the accuracy of the opinion that had been given. Finding it needful that he should remain in town for some time, I strongly advised his removal to the Hospital, where he would receive every attention, and where you, as well as I myself, might have the opportunity of studying a disease which is comparatively of rare occurrence.

After the admittance of the patient into the Hospital, I made a careful examination; and this is what struck me,—Here was a man lying in bed, with a large pulsating tumour, resembling in many ways an ordinary aneurism; pallid, thin in the limbs, hardly able to walk, and experiencing great pain in and about the region of the tumour. Now, contrast this with a case of spontaneous aneurism of the

popliteal or other large artery of the extremities, but especially with one situated within one of the large cavities. The patient labouring under the disease is much annoyed at being kept to his bed; he imagines that little is the matter with him, for he may be capable of undergoing great fatigue, perchance walking great distances to seek opinions regarding his malady. In fact, it is not a feature in ordinary aneurismal disease for the patient to be out of health. I mentioned that great pain was very early complained of in the tumour, whereas it is rare for those labouring under an aneurism of a large artery to make such a complaint at so early a period.

Regarding the character of the tumour: Where I made pressure in the site of the gluteal artery, as it leaves the sciatic notch, and turns up on the outside of the pelvis, all pulsation in the tumour ceased, which fact certainly seemed very strong in favour of the supposition of a gluteal aneurism, and an indication which, taken by itself, was not unlikely greatly to bias the surgeon in his diagnosis; but I confess this had little or no weight with me, for I have met with malignant tumours of bone in various parts of the body possessing great pulsation, which could be arrested by pressure on the main artery supposed to lead to the disease.

The size of the tumour and its situation were very much against it being considered an aneurism of any one branch of the gluteal; indeed, I could not fancy such constitutional disturbance to be dependent upon such a source. It is seldom that the surgeon is consulted about an aneurism at so early a period as this case came under notice; but, if he should meet with such a disease, at what is termed its first stage, it is generally possible to empty the blood out of the tumour; but, as you saw during my visits to the patient, I frequently tried so to do, but was always unsuccessful. In the earlier stage of the disease, the crest of the ilium could be distinctly traced above the upper border of the tumour; but, after a short time, the part became entirely involved in the disease. There was one feature in which common-place anatomy greatly assisted me. I noticed that the insertions of the long extensor muscles of the back, as the disease increased in its dimensions, became elevated above the surface of the tumour; whereas, if the swelling had been an ordinary aneurism of the gluteal artery, they would doubtless have been beneath. These were, then, the principal points from which I framed my diagnosis, in the early stage of the disease; and I came then to the conclusion, that the man was not the subject of a gluteal aneurism, as was at first supposed, but that he was labouring under malignant disease of the ilium, and that the tumour was what is commonly called a pulsating tumour of bone, possessing more vascularity than was usual in such cases; and hence the well-marked pulsation.

This so-called aneurism of bone is, in my opinion, nothing more than aneurism by anastomosis in hard structure, somewhat modified in its condition and symptoms, and which might in many instances be called medullary sarcoma, or fungus hæmatodes. There is generally a very large expansion of bone, seeming as if the bone itself had been blown up, the interior of which may contain a quantity of medullary or melanotic matter, or resemble a sarcomatous mass, containing cysts with fluid blood, and possessing great vascularity. Though thoroughly satisfied in my own mind as to the nature of the disease, I was anxious, nevertheless, to avail myself of the advantage of other opinions; and, accordingly, after requesting the opinions of my colleagues, I invited surgeons of the highest eminence in London to see the case, the majority coming to the same conclusion that I have already expressed.

It was quite evident that an operation could in no way benefit the patient, and that the only treatment that could avail was rest and kind attention to his wants. He daily grew weaker, until at length, becoming delirious, he ultimately died from pure exhaustion. Here let me impress upon you how important it is to avoid all surgical interference in those instances in which such a proceeding is unlikely in any way to better the patient, but which, on the other hand, is calculated to bring discredit on the surgeon, and the worst possible result to the sufferer. This disease so nearly resembles gluteal aneurism, that surgeons of experience have actually mistaken its true nature, and treated the case as if it really had been an ordinary aneurism. Among such instances I may mention two cases which occurred to Mr. Guthrie and Mr. Stanley, in which the main artery was ligatured; but subsequent results showed the error that had been committed. These gentlemen have candidly laid the whole proceedings before the profession, and therefore I have no hesitation in mentioning their names.

By studying these unfortunate cases, which are justly held out to us as landmarks, we are enabled to avoid falling into similar errors; and I often think that in our profession as much is learned from mistakes as from the performance of good surgery.

Some time before the patient's death, while paying my visit one day, I found that the left thigh-bone had spontaneously broken just below the trochanter, no cause being assigned for the accident. Now, in malignant affections, or certain forms of cancer, it is not by any means uncommon for bones to become friable, and break with the least possible exertion. This was, therefore, another very conclusive proof in support of the malignant nature of the disease.

As I have mentioned that this case was mistaken for one of aneurism of the gluteal artery, it may be advisable to say a few words regarding the treatment of that formidable disease. There are few cases on record of spontaneous aneurism of this artery, although, from its situation, it is liable to injury, which may demand surgical interference. We are all familiar with John Bell's celebrated case of wound of this vessel, in which he cut into the enormous tumour formed by the accumulation of blood under the skin, turned out the clots, and secured the bleeding vessel. But the procedure that is more in accordance with the established rules of surgery in cases of spontaneous aneurism is that recommended by Dr. Stevens, late of Santa Cruz, author of the treatise lately published on the saline practice in cholera, who succeeded in curing a large aneurism of this artery by placing a ligature on the internal iliac. Had the case we are considering been of this nature, I should have adopted the plan of treatment recommended by Dr. Stevens.

These were the principal points of interest in this remarkable case; and the *post-mortem* examination proved the correctness of the leading opinions that had been formed during life. Most of you saw that examination; but I will briefly detail the morbid appearances which were presented. When the body was put on the table, you saw the extreme emaciation to which the patient had been reduced by the malignant tendency of the disease. On reflecting the skin from the left buttock, and dissecting back the gluteus maximus, which was greatly wasted, a large tumour was found, reaching from the sciatic notch to the brim of the pelvis,—in fact, covering the whole of the haunch bone, and resembling more the substance of brain than any I remember to have seen. The tumour, I think, might with propriety be called by half-a-dozen different names, each being equally applicable, as medullary sarcoma, encephaloid disease, soft cancer, &c.

The disease had evidently commenced in the cancellated structure of the ilium, and found its way into the cavity of the pelvis, as well as outwardly. That part projecting into the pelvis had elevated the iliac vessels, and put upon the stretch the large nerves of the sacral plexus, which fully accounted for the constant pain complained of in the thigh, extending to the foot. That portion of the ilium in which the disease commenced was entirely destroyed, the free margin of the bone being left rugged and bare.

The gluteal artery, much enlarged, could be traced, shortly after leaving the sciatic notch, winding over the lower part of the tumour, and distributing large branches which ramified freely over the surface, and fully explained the reason of the well-marked pulsation during life. The large sacro-sciatic nerve was pressed upon by the lower part of the tumour, which doubtless caused the excessive and frequent pain to the patient. The disease had extended to the left side of the two lower lumbar vertebræ, sacrum, crest of the ilium, and upper part of the femur. The remaining bones of the body were not found similarly affected, but the kidneys and lungs presented well-marked specimens of cancerous deposits.

I have been particular in directing your attention to this case, both because it was fraught with interest, and that it created as much thought and speculation in my mind as any I can at the present time remember.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 585.

60.—ON TUMOURS OF THE UPPER JAW.

By Henry Hancock, Esq., Lecturer on Surgery at the Charing Cross Hospital.

It is a common opinion that these tumours originate within the antrum of High-

more. Mr. Hancock does not agree with this opinion. Mr. Ormerod, of St. Bartholomew's, has taken pains to classify these tumours, which he does as follows:]

1. Epulis.

2. Cystic tumours, consisting of the walls of the bone expanded on or into a sac, with more or less of solid growth; the walls in some parts being quite membranous and transparent, and the sac filled with glairy fluid, or a firm substance contained in cells, or accompanied with a granular fatty substance, partitioned by fibro-cellular substance; also consisting of a single bony cyst lined by a membrane, with a second canine tooth adherent to it, and having its cavity filled by a glairy fluid.

3. Cartilaginous and osseous tumours from the upper jaw-bones, as round tumours growing on their outer surface here and there, but chiefly as a mass growing from their inner surface, so as to fill up the maxillary sinuses, the septum nasi and spongy bones being also affected. The thickening is dense, hard, and ivory-like.

4. Fibrous tumours from the upper and lower jaw-bones, consisting of white or pale yellow, firm, and nearly homogeneous, with or without specks of bone, or of dense, more or less fibrous, or even obscurely fibrous, substance, containing at times one or more small cavities filled with pus, with a glairy fluid, or with blood; growing on a healthy bone and periosteum, or from the alveolar and outer surface of the bone, or originating in the cancelli, and being accompanied with a perfectly healthy condition, with absorption or consolidation of the surrounding bone.

5. Medullary tumours from the upper or lower jaw, consisting of round, lobed, or nodulated masses, with a smooth membraneous covering, or with a rough fungous or ulcerated shreddy surface; invested by a thick capsule, by a dense periosteum, or by partial thin laminæ of bone; composed of round lobes, connected together by cellular tissue, or formed of an almost homogeneous mass; made up of a structure of a soft medullary, brain-like, spleen-like, or firm fatty nature, with cells and bony spiculæ, sometimes commencing in the neighbouring glands, and extending to the jaws secondarily.

In addition to these, Mr. Stanley enumerates fatty and erectile tumours of the jaw; whilst Mr. Paget adds what he terms myeloid tumours of the part; but whilst the examples he quotes resemble so much, on the one hand, the fibrous, on the other medullary tumours; their true character, whether innocent or malignant, is so very doubtful, that I should hesitate in admitting them as a distinct class.

I have never met with a fatty growth so invading the upper jaw as to require the removal of any portion of that bone, and therefore I do not presume to offer any opinion upon that form of disease.

Mr. Stanley states that the erectile tumour is a very rare form of disease; but that it occurred to M. Gensoul in one of his successful cases. The tumour was soft and vascular, and quicksilver, impelled into the morbid structure, readily pervaded it throughout. I have not seen a similar case, nor have I been able to meet with any recorded elsewhere. It is occasionally very difficult to form a correct opinion of the exact point of origin of a fibrous tumour of the upper jaw. It will sometimes commence by a contracted pedicle, enlarge, and spread out, and not only fill the antrum, but extend into the nares; by its peculiar and irregular appearance giving rise to the supposition of its being a polypus of the nostril, and leading to abortive attempts for its removal by the polypus forceps." Mr. Stanley, although not mentioning the disease, has the following observations bearing upon this point: —"A tumour gradually increasing within the antrum may occasion yielding of the walls equally in all directions; but in some cases, disease extends chiefly in one direction, and causes difficulty of diagnosis. In a case I saw, a morbid growth originating in the antrum had extended only in the direction of the nostril, and portions had been extracted by polypus forceps."

[Osseous tumours are the most frequently met with amongst the non-malignant growths of the upper jaw. Mr. Hancock has two specimens of this disease.]

The first case is that of a young man, aged 22, a farmer, residing in Lincolnshire, who was sent up to me on the 1st October, 1848, by Mr. Young, of Gosberton, whose patient he was. This case was a striking example of what has been affirmed elsewhere, that "osseous growths are in some instances combined with hypertrophy

of the surrounding bones, producing general enlargement." The whole of the bones on the right side of the head and face were very much enlarged and thickened, the bone at the fronto-malar suture presenting a surface of more than a square inch, and this, I believe, is the usual origin of the tumours in question. I believe that they should not be regarded as originating within the antrum, or from causes connected with the teeth, but from excessive development or hypertrophy of the bone itself, which, in its growth, extends in various directions, invading the antrum of Highmore, and in some instances entirely obliterating that cavity. Mr. Paget, in his Lectures on Surgical Pathology, in allusion to these tumours, says, "More commonly, it is almost limited to the antrum;" but he singularly disproves his assertion, and confirms my views, by his illustrative example. He says, "In this case, it may exist with little deformity. In the museum of St. Bartholomew's Hospital is a specimen, in which both the antra appear nearly filled by the thickening and in-growing of their walls; only small cavities remain at their centres. The new bone is hard, heavy, and nearly solid, yet it is porous or finely cancellous, and is neither so compact nor so smooth on its cut surface as that of 'ivory exostosis.' The same disease is manifest in a less degree upon the outer surfaces of the maxillary bones, and on the septum and side walls of the nose."

I have operated upon three cases of this form of disease. One of these I have just related, and in the other two the antrum remained; in one of these there was general hypertrophy of the bone, causing great disfigurement, and necessitating its entire removal. The swelling did not arise from, and was not confined within, the cavity, but was, in truth, a portion of the general thickening and enlargement of the bone. I have a preparation, which was taken from a child of six years of age, sent to me by Mr. Calthrop, of Gosberton, Lincolnshire, upon whom I operated in April, 1852, and is a well-marked example of the fallacy of Mr. Stanley's position, that "morbid growths mostly arise from either of the lateral parts, not from the front of the jaw—a fact which might be explained by the consideration that irritation more frequently originates in a molar than in an incisor tooth," as does the following case:—

A child was admitted, under my care, into the Charing Cross Hospital, in Aug. 1852, with tumour of the left upper maxilla. It appears that in Dec. 1851, she fell and bruised her face. Soon afterwards a tumour was observed on the left side, just below the orbit. This was unattended with pain, but it gradually increased in size until, at the time of admission, it was about the size of a walnut. Upon careful examination, I found the hard palate and gums perfectly healthy; the tumour was smooth and solid. Upon carrying my finger behind the soft palate, I could not detect anything wrong in that situation; but as I otherwise could not detect the extent of the mischief, or what was the extent of operation required, I introduced a small exploring trocar into the tumour, and felt it enter a solid mass, which prevented my giving the instrument any lateral motion. I next perforated the upper jaw above the alveolar process corresponding to the molar tooth, and found the instrument enter a cavity, in which I could freely move its point, and from which I decided the case was hypertrophy of the anterior portion of the maxillary bone, and I acted accordingly, confining the operation to the simple removal of the part affected, without interfering either with the floor of the orbit or the roof of the mouth.

The last form of abnormal growth is the encephaloid or medullary sarcoma; and it is here that a correct idea of the origin of these growths becomes of so much importance. I have throughout this paper been at great pains to disprove the opinion so universally maintained, and so decidedly advocated by Mr. Stanley, that morbid growths commonly originated within the antrum, not only because I felt, from what I had observed, that such opinion was wrong, or rather open to very many exceptions, but because the statement, backed by such an authority, gives rise to the supposition that medullary sarcoma usually commences within the antrum; and that, if attacked sufficiently early, it may be eradicated by operation. At the risk of appearing tedious, I feel that the importance of the subject fully justifies my again quoting Mr. Stanley. He says, "But even with the help of the most careful examination, whenever the disease fills the antrum and nostril, it will be uncertain whether or not it extends posteriorly beyond the front surface of the pterygoid processes of the sphenoid bone. Whilst Mr. Liston has observed, 'If anything is to be done, it ought to be undertaken with a thorough determination to go beyond the limits of the morbid growth,

to remove the cavity which holds it, and thus get quit, if possible, of all the tissues implicated, or which may have become disposed to assume a similar action.' I know from experience that this step, if adopted in time, may prove successful." It may appear presumption in me to differ from such authorities as these, and men of such experience, but my own experience leads me to an opposite conclusion.

I entirely differ from the opinion that medullary sarcoma commences in the antrum of Highmore, and extends backwards to the pterygoid processes; but, on the contrary, from what I have observed, I firmly believe that the disease commences in the cancellated structure of the body of the sphenoid bone and bones at the base of the cranium; and that, however early we may perform the operation, we never succeed in eradicating the mischief, which is sure to return at a longer or shorter period, according to circumstances.

I have performed, and assisted at many operations for the removal of the upper maxilla for this disease, and where the mischief, as far as external examination and the introduction of the finger behind the soft palate went, appeared to hold out every reasonable prospect of its complete removal, but in no one instance was this satisfactorily accomplished, and in no one instance did the disease fail to reappear; whilst post-mortem examination demonstrated its growth from the body of the sphenoid bone, the basilar process of the occipital bone, &c. This disease will sometimes extend into the orbit, in some instances causing the eyeball to protrude, in others presenting us a firm tumour on one side of the eyeball. But it should be borne in mind that, notwithstanding the absence of external swelling of the cheek, these growths rarely, if ever, originate in the orbit, or are confined to that cavity, but that the swelling in the orbit is almost invariably an extension of the disease in that direction, its origin being in the cancellated structure of the bones at the base of the cranium, and that it mostly, at the same time, invades the spheno-maxillary fossa and antrum of Highmore, so that it can never be entirely removed by dissection from the orbit.

I have a lady at present under my care, who consulted me for protrusion of the left eyeball, with apparent commencing chemosis of the conjunctiva and indistinctness of vision. The protrusion is not very great; but pressing the base of the eyelids near the margins of the orbit gives the sensation of an elastic body between the orbit and the eyeball. There is no swelling of the cheek; but upon examining the mouth I find the gum is beginning to separate from the molar teeth, and the dental margin to become everted, whilst separation is also commencing between the hard palate and its covering, the bone appearing softer than natural when pressed upon. Carrying my finger behind the soft palate into the spheno-maxillary fossa, I can distinctly feel an irregular growth in that situation.

This case shows the impropriety of operating upon orbital growths of this description, the tumour in that situation being commonly but a local manifestation of disease extending into the upper jaw and elsewhere, where the knife cannot penetrate. In some instances the tumour will be more isolated or partial, and equally, if not more malignant, if we may judge from the rapidity of its growth. It will not present the same elasticity to the touch, feeling more like an enchondroma, and when cut into, it has very much the appearance of a fibrous growth.

A woman brought her child, aged two years, to me at the Ophthalmic Hospital with a conical tumour of the orbit, at the inner and upper part; it was very firm to the touch, and the mother said it caused the child great uneasiness. She had perceived the swelling about a month before. I entertained some doubts as to the nature of the swelling, and expressed them to the mother, at the same time telling her that I could not recommend any operation. At this time the disease was apparently confined to the orbit, as by careful examination I could not detect any manifestations elsewhere. She pressed me to do something, and therefore I endeavoured to remove it, and found it to consist of dense fibrous structure, invested by yellow, firm substance; it penetrated so deeply, however, that I could not get the whole away, and the swelling rapidly reappeared, the disease seeming to be lighted up with fearful intensity. The child lived two months afterwards, but had become a most frightful object. The tumour protruded from the orbit, extending upwards towards the forehead outwards, invading the temple, and downwards through the upper maxilla and mouth to the neck.

These cases justify the opinion that fungoid or medullary growths of the orbit rarely exist as independent diseases, but that they almost always co-exist with the same disease in the antrum and spheno-maxillary fossa; at all events I do not recollect an instance where this was not the case, and I have seen many of them.

Having endeavoured to point out the inadvisability of performing operations in cases of medullary sarcoma of the upper jaw, I will now advert to some of the most prominent means of diagnosis between this disease and other abnormal growths of that part. The diagnosis between fibrous tumours and medullary sarcoma is very obscure: some idea may be obtained from puncturing the tumour, and from the degree of resistance to the lateral motions of the instrument presented in the former disease, as well as from the absence of spiculæ of bone discernible in some forms of the latter; but the principal diagnostic signs which we possess are, the condition of the teeth, gums, and bones of the part. It is true that the bones, in certain conditions of health—as in the case to which I have alluded—may, from pressure, become carious, or even destroyed by necrosis; but they are never converted into that soft degeneration so characteristic of medullary sarcoma, or the malignant disease. Pressure on the hard palate is met with firm, hard resistance; whereas in medullary sarcoma—in all cases where the tumour projects from the cheek, and in very many of those wherein the disease manifests itself in the orbit—the palate upon the affected side is elastic to pressure, usually more congested than natural, whilst the gums are congested and thickened, the dental margin is thickened and everted from the teeth, which become irregular, loosened, and projecting. When seen for the first time, this condition of the gums and teeth may be mistaken for abscess or disease connected with the teeth, with which, however, it has nothing to do, and from which it may be distinguished by the comparative slight pain which accompanies it, by its gradual progress, by the absence of inflammatory swelling of the cheek, and by the character of the enlargement of the gums.

In this affection the gums are spongy and elastic, and, when pressed upon, are found, in a great degree, to have lost the support of the alveolar process, which becomes softened by the disease; whereas in inflammation and abscess resulting from disease of the teeth, or alveolar sockets, of a simple character, the pain is very great, the swelling of the cheek is evidently of an inflammatory character, whilst the enlargement of the gums is smooth and firm, and the firm resistance of bone beneath may almost always be detected. In medullary sarcoma this state of the gums should be regarded as the result, not the cause, of the disease; and for the reasons which I have already given in the early part of the paper, the teeth, though loose and comparatively useless to the patient, had better not be meddled with.

Bony tumours, though more easily distinguished from medullary sarcoma than fibrous growths, may, upon superficial examination, create doubts in the minds even of well-informed surgeons. As I was going down into the operating-theatre, to remove the upper jaw from a child aged six years, a surgeon of some standing, who had examined the child in the ward, said, "Surely, Mr. Hancock, you will not meddle with that; it is malignant." He was misled by a certain degree of elasticity presented at one point of the tumour, and which I have observed in all the cases which have fallen under my observation. They have not been of that form denominated ivory exostosis, but have all presented a certain degree of yielding to the touch; but they have this marked distinction from the medullary sarcoma: in this latter disease the surface of the tumour is uneven and elastic, but the elasticity is superficial; whereas in osseous tumours the elasticity is deeper seated, and the yielding or elastic substance is evidently covered by a thin shell, which recedes when pressed upon, but appears to recover itself when the finger is removed. We have not the crepitation in these cases as in osseous cysts, but the elasticity is covered by a thin, smooth investment, and is not so immediately upon the surface as in osteo or medullary sarcoma. Puncturing these tumours with an exploring needle, or what I prefer, a small trocar, will also assist us in our diagnosis. In medullary sarcoma, the instrument appears to enter a soft mass, and with but little force a considerable degree of lateral motion may be given to the instrument; but not so in osseous tumours. Here, though it may be introduced with difficulty, still it will appear to be clung to by the structure into which it penetrates, and lateral motion cannot be obtained except with that degree of violence which we should be unwilling to employ; and, again, as in fibrous growths, the condition of the gums and teeth will

pretty surely mark the non-existence of malignant disease. The teeth will, in many instances, be perfectly sound; and though, in others, the patient may have lost some of those organs, the gums may have healed, and will not show any of that spongy appearance, or tendency to eversion, so characteristic of the malignant disease. I never saw a tumour of the upper jaw of a malignant character unaccompanied by mischief about the palate and gums; but an osseous tumour may have attained great size, and cause considerable deformity, without projecting into the mouth, or complicating the palate or gums at all; and this is an additional reason for employing an exploring needle or trocar, this instrument, in some cases, affording us the only means of ascertaining the extent of operation to be performed—whether the whole, or a portion only, of the upper maxilla is to be removed. Mr. Fergusson has laid down that rapidity of growth is a diagnostic sign of malignant tumour; this, however, is not entirely to be depended upon, as the growth of bony tumours will sometimes be extremely rapid, as in one case wherein the tumour attained a great size in less than five months.—*Lancet, Jan. 6 and 13, 1855, pp. 5, 30.*

61.—ON TUMOURS OF THE JAWS.

By James Syme, Esq., Professor of Surgery in the University of Edinburgh.

[In a clinical lecture on this subject, Mr. Syme makes the following important general remarks, which will apply to tumours affecting both upper and lower jaw.]

There are some anatomical differences between them, which lead to pathological differences. In the upper jaw, the hollow of the maxillary antrum exposes to a condition which cannot exist in the lower jaw—viz., inflammation and suppuration of the cavity. The antrum also serves as a receptacle for polypous growths, which, however, are mere intruders from the nasal cavity, and not diseases of the jaw itself. In both jaws the bone is apt to die from the effects of inflammation. In the upper no reproduction of bone takes place after removal of the exfoliation; in the lower the bone is denser, and the periosteum more capable of reproduction, and there is no instance in the body of a more complete restoration of bone. I have repeatedly seen almost the whole lower jaw, including the condyle, thrown off as an exfoliation, yet complete reproduction has taken place. These are all the differences which it is necessary for me to notice now. There has been a notion prevalent, that tumours of the upper jaw originate in the antrum. This is erroneous; their origin is in all cases similar to tumours of the lower jaw.

Tumours of the jaws are chiefly of five different kinds—three solid and two fluid. Of the solid, you have one growing from the surface, and two in the interior of the bone. The one on the surface is denominated "epulis," or "epulotic tumour;" it appears on the gum, and increases in size without limit to its growth, and at last becomes inconvenient from its bulk; it is also liable to bleed, and may become as serious, in this respect, as if it were malignant. Epulis grows from the surface of the bone, and requires for its remedy the removal of the part to which it is attached. The knife never cures the disease completely, and it is always requisite to take away more or less of the alveolar process, in order to do which you must extract one or more teeth.

The other two forms of solid tumour originate in the osseous substance, and are divided into simple or local, and malignant, or those where there is some constitutional derangement connected with the local condition, which produces a tendency to a return of the disease after its removal. The first kind are generally of firm consistence, and slow growth, and accompanied by a healthy state of the constitution; the second are of soft consistence, and rapid growth, and the patient's constitution commonly unhealthy. But these kinds pass, by various degrees, insensibly into each other. The slower the growth, the firmer the consistence, and the more healthy the constitution, the greater will be the probability that the disease is entirely local, and that the removal of the tumour will be followed by a permanent cure. But tumours of the most rapid growth and softest consistence have sometimes been taken away, without any return of the disease; and hence the general rule is, that if such tumours are distinctly limited to the jaw, they should be removed. You must bear in mind, however, that a malignant growth is apt to

extend its roots beyond the bone from which it originated, in this respect remarkably differing from the simple, local, or fibro-cartilaginous growths, to which your attention was lately directed. The other day, a lady, said to be labouring under polypus of the nose, consulted me. I found, however, that the soft mass which showed itself in the nostril was really a tumour proceeding from the superior maxillary bone; and further, a swelling at the inner canthus showed that the disease was not limited to that bone, but extended up to the ethmoid, and probably to the base of the skull. I therefore advised her against any interference; but had the disease appeared limited to the superior maxilla, I should have recommended excision of the bone. I could mention several cases, where the removal of soft tumours of the jaws of rapid growth has been followed by permanent recovery.

The fluid tumours of the jaws are of two sorts, being collections either of serous or purulent fluid; the serous or cystic are not unfrequently mistaken for solid tumours; they grow in the substance of the bone, and are generally at some parts of their extent as hard and as unyielding as bone, so that if their examination be limited to these parts, the tumours will be supposed to be solid. Even so accomplished a surgeon as M. Gensoul, of Lyons, had once made his incision and exposed a tumour of the jaw, when all at once the knife entered a cavity, from which serous fluid escaped. I have myself met such cases, and seen them in the practice of others. It is therefore necessary to examine a tumour of the jaw over its whole surface; then, if it be cystic, you will be almost sure to find some yielding point where the bone gives way like pasteboard, and is perhaps felt to crackle under the finger.

These cysts frequently exist independently of any other morbid condition, but sometimes, and especially in the upper jaw, they are connected with displaced teeth, one of the permanent set having taken a wrong direction, so that, when the cyst is opened, the tooth is found lying in its interior: I have, in this theatre, more than once found such a state of things present. Whether this be the case or not the treatment is the same—viz., to open the cavity freely and stuff it with lint. The prognosis is, however, more favourable if a tooth be found in the cyst; it is then almost certain that the cavity will contract and heal. This may occur in other cases, but it has also been noticed that cysts, after being opened, are not unfrequently followed by the formation of a solid tumour. I could mention to you cases where I have opened cysts containing nothing but serum, in the place of which solid tumours have afterwards been formed. It is also possible that the cysts may be complex, and so extensive as to require removal. A patient left the hospital only a few weeks since who applied to me five years ago on account of a large cystic tumour of the lower jaw, which I opened, and gave exit to a large quantity of albuminous fluid, and stuffed the cavity with lint. The swelling afterwards contracted to a considerable extent, but never entirely disappeared, and she returned two years later with the tumour as large as it was when I first saw her, and affecting the jaw more extensively. I again opened and stuffed it, and the tumour again contracted to a certain extent. It, however, again increased, and I repeated the process for the third time, but with the same result, the patient returning last autumn with the tumour larger than ever, and extending now from the symphysis to the coronoid process. I removed half the jaw by disarticulation, and on examination we found that the cyst, instead of being simple, was complex, consisting of four chief cavities, the walls of which were studded with smaller cysts of various sizes. It was clear that in this case nothing short of excision of the portion of the bone affected could have produced a cure.

Tumours depending on purulent collections are not at all uncommon. They are almost invariably in connection with the stump of a tooth. A portion of a stump may remain after the extraction of a tooth and be forgotten, and the tumour may be supposed by the surgeon to be solid. Generally, however, the parietes, like those of serous collections, will be found to yield at one place, and you find an additional guide to your diagnosis in the presence of the diseased tooth. I lately saw a tumour of the lower jaw, which was removed under the supposition that it was solid throughout; but on being sawn through, it proved to be an abscess, the osseous wall of which had undergone immense thickening, except at one small point in immediate connexion with a diseased tooth, which had never caused any pain. In that case, it may be doubted whether it would have been possible to afford relief

otherwise, even if the real nature of the disease could have been made out, from the difficulty of affording a free outlet for the discharge.—*Lancet, March* 10, 1855, *p.* 253.

ORGANS OF CIRCULATION.

62.—ON POPLITEAL ANEURISM.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[In a clinical lecture on this subject, in which will be found some valuable hints as to the mode of tying the artery, and especially with respect to meddling with the vein, Mr. Syme speaks as follows:]

We are called upon to adopt some means of treatment, and you are aware that there are two methods for us to choose between—namely, ligature of the vessel, and the application of pressure over it. It may be said, in favour of the former, that it causes no pain whatever; since, if chloroform is used, the patient does not feel the operation, and he suffers nothing after it, his only duty being to keep quiet for a fortnight or three weeks. The advantages alleged in favour of compression are, that it is safer, ligature being attended with risk of hemorrhage or of mortification of the limb, while compression is devoid of these dangers, and therefore preferable. On the other hand, if the operation of ligature be properly executed, it is almost absolutely free from danger; pressure, moreover, does not always succeed, even in the most experienced hands, and generally requires to be applied for weeks or months before any appreciable effect is produced by it, while the patient requires to be kept almost under military discipline, in the endurance of very considerable pain, as you will understand if you will apply a tourniquet on your own persons. In an institution like this, the length of time occupied is a serious consideration; for instance, a patient lately in the hospital, whose case has been published, was for seven months under treatment by compression, and at last ligature of the artery was resorted to. Here was a bed occupied for this protracted period which might have been the means of relief to many cases, with proportionate instruction to those who study here. But it may be said, in addition, that pressure is not quite so safe as it is generally stated to be; it may lead to mortification, and other disturbances, which have been described in some cases that have been published, and which, I have reason to believe, would be found more frequent if all the cases in which compression has been tried, were placed before the public. After a careful consideration of the subject, I have come to the conclusion that, if the operation of ligature be performed properly, with due attention to the niceties that render it safe, it is preferable; but if circumstances prevent this, compression should, by all means, be had recourse to. I can understand many such circumstances; for instance, a surgeon in the public service, or some remote district of the country, who has little opportunity of practising operative surgery, may feel that compression would be the safer treatment. In short, the question seems to me very similar to that of whether a catheter should be passed, or the bladder punctured, for retention of urine; if the urine can be drawn off by catheter, by all means let this be done, but if the circumstances of the case should be such that not one surgeon in a hundred could do so, it would be incumbent on the ninety-nine to obviate the risk of rupture by puncture of the bladder; and hence, whilst I regard the introduction of the catheter as the preferable means of relieving retention of urine, I have never undervalued puncture of the bladder as an operation in surgery. I say this the more because I read, a few days ago, in a London periodical, the statement that I "have endeavoured to cast a certain amount of discredit on those who have ably devoted their time and energy to the trial of a more simple and less hazardous cure of this formidable disease."

This is a very serious accusation against any man, and particularly so when coming from one teacher of surgery against another. It is indeed quite true that I have given no countenance to all the claptrap performances of what has in recent times been denominated "conservative surgery," but it is not true that I have in any way opposed the efforts of those who have striven to improve the practice of surgery. The same lecturer says that the treatment of aneurism by compression was "in vogue," as he expressed it, "two hundred years ago," but was "abandoned

after the trial of Hunter's operation in 1780." This is entirely incorrect, and for very obvious reasons, since compression as now practised was only thought of by following out the principle of the treatment of Hunter and Desault, (for Desault's name should always be mentioned along with Hunter's in speaking of this subject.) Hunter's operation contrasted strongly in its results with the old method, which was, to cut into the aneurism and tie the vessel both immediately above and below it, a proceeding which was so rarely successful, that Dr. Wilmer of Coventry (1779) says that he knew of no instance of its being so in this country. When, therefore, it was learned that the operation of Hunter was commonly followed by success, a very great sensation was produced in the medical world; but unhappily, it soon appeared that the operation was not unaccompanied with risk of hemorrhage, and you will find that various expedients were in consequence introduced with the hope of diminishing this danger. Instead of a simple piece of silk or fine thread, ligatures of reserve, as they were called, were also passed, to be used if necessary; thick cords or tapes were employed, a pad of lint was interposed between the vessel and the ligature, the artery was exposed and forceps (*presse-artères*) were applied for a time instead of a permanent ligature, and with similar views the artery was compressed at the seat of Hunter's operation, without exposure of the vessel; and if you will look into M. Boyer's Surgery, you will find two cases recorded in which this treatment had been used successfully, and in reading the accounts it will seem to you as if you were perusing recent cases instead of those half a century old. Now I beg you to notice what took place subsequently. The cases I speak of were in 1804 and 1806; in 1807 a great step was made by the publication of a work by Dr. Jones, a graduate of Edinburgh, on the process by which hemorrhage is arrested. He clearly made out the following important points: that the safety of the operation of ligature is the greatest when the artery is exposed to the smallest possible extent; also, that the risk is less the less the surrounding parts are disturbed and torn, and the more exclusively they are divided by a sharp cutting edge; further, that the ligature, which is a foreign body, should be as small as is consistent with strength, and tightly tied. It was found that if these points were attended to, the ligature of arteries was accompanied with much less risk than had previously been the case. Following out these principles, I have tied the femoral artery twenty-three times, without any bad effect. I do not by any means wish to say that every artery I happen to tie will be tied successfully, but I do say that this fact is sufficient to warrant me in maintaining that the operation, when properly performed, is not in itself a dangerous one.

Now let me explain distinctly what I mean by the operation being properly performed: I do not wish to imply something done with extraordinary dexterity, or what only a few persons can do, but I would insist upon the necessity of bestowing sufficient care upon the procedure. Take, if you please, half an hour, or a whole forenoon about it; but do not pass the needle till the artery has been sufficiently separated from the contiguous structures, to enable you to avoid all risk of injury to the vein, which is the great source of danger to be dreaded.

[In one patient Mr. Syme tied the artery on January 9th, with his usual precautions with respect to the vein. On January 18th, nine days afterwards, Mr. Syme says,]

The man who was operated upon for popliteal aneurism nine days since is going on perfectly well; there is nothing he can complain of, except confinement to bed, which will probably not last many days longer. When you see a patient labouring under a formidable disease cured by an operation which gives him, through chloroform, no pain at the time, with the expenditure of only a few drops of blood, and little or no subsequent suffering, you naturally regard the treatment as very near perfection. Why, then, you will ask, is it not universally adopted? On account of danger to which it is said to be liable from two sources—viz., mortification and hemorrhage. I wish to consider at present how far these two sources of danger are unavoidable, and upon what they depend. And first of mortification. This is said to be due to the limb being weakened in its vitality when the artery is tied, in consequence of the blood not flowing sufficiently freely through the anastomosing vessels, and this, it is alleged, operates more especially in small aneurisms, where there has not occurred sufficient dilatation of the anastomosing vessels to form an efficient subsidiary channel. Mortification is said to happen in two ways: in one

the weakened limb is for some time after the operation cold and impaired in sensation; then a period of reaction occurs, accompanied with pain, swelling, heat, and redness; and if this reaction be excessive, it may prove fatal to the limb by the inflammation. In the other mode of occurrence of mortification, reaction does not take place, but the limb remains cold, and its colour changes to blue, its moisture exudes, and it finally becomes dry and black, like a mummy, and the death of the patient, or amputation of the limb, is the inevitable consequence. These are very frightful statements, and I confess that in the first case on which I operated I felt considerable alarm. The aneurism was of small size, and the patient (a gentleman of no great firmness of constitution originally) had been much reduced by the treatment to which he had been subjected by the Professors Thomson and Turner—viz., confinement to bed on very low diet, with the object of promoting spontaneous cure, but with no beneficial result. To my astonishment, however, I found after the operation that there was scarcely any appreciable difference of temperature between the two limbs; that this soon passed off, and that no inconvenience of any kind was experienced. This is now, I believe, the twenty-fourth case in which I have tied the femoral artery, and I never met with any other result. There has never been any difference of temperature, except a slight coolness, which required careful examination with the hand or the thermometer to detect. There has never been any threatening of reaction, never even a partial slough, yet my patients have been old and young (I once operated on a boy, nine years old), and the aneurisms both small and large, as well as of recent and of long duration. I have, however, seen mortifications occur in the practice of others in both the ways I have mentioned. The case with reaction was one on which Mr. Liston operated; the patient had suffered pain previously, and had been in the habit of relieving it by the use of warm baths, of which he kept an establishment. The pain continued after the operation, and the hot bathing was liberally used the same evening and next day. Swelling and reaction came on, terminating in mortification, and I assisted Mr. Liston in amputating the thigh at the middle. In other cases I have seen the limb become cool after the operation, and in the evening cold as a dead body, then blue, and finally mummified. Now there must be reasons for results such as these, and I confess that this was to me at one time a subject of very anxious inquiry; but all my own investigation would perhaps have been vain, had it not been for the observations of Mr. Guthrie, which are important on many points, but especially on the subject of injuries of arteries. He observed that the artery alone might be obstructed without death of the limb, but that this was the invariable result if both artery and vein were obstructed together.

[Mr. Syme has long been of opinion that where the results of this operation vary so much from his own experience, it is owing to some intermeddling with the vein. He says:]

You are aware that the femoral vein is intimately connected with the artery, much more so than in the case of some other vessels that may require ligature—*e.g.* the iliac artery is but loosely connected with the vein, and great freedom, or even great roughness, may be used with it without bad result. It is not so, however, with the femoral; if you do not detach the artery from the vein with the greatest nicety, you will very likely either pierce or bruise the latter. But if you do the operation in the way that I shall presently describe, you may fairly expect that, instead of mortification, either with or without reaction, the only change that will occur in the limb will be a slight diminution of temperature, productive of no inconvenience, and which will disappear in a few days.

Now we come to the other source of danger—viz., hemorrhage; and in order to consider this, I must call your attention to the process of separation of the ligature. The first effect of the ligature is to cut through the internal and middle coats, and to draw the external coat into a small compass, not wider than a probe. The next result is the coagulation of the blood lying in the artery near the ligature, forming a conical coagulum on each side, tapering away like a worm as far as the nearest large branch. The next change is an effusion of lymph on each side of the ligature, sealing up the ends of the vessel; then a process of ulceration takes place for the separation of the foreign body, viz., the ligature, with the portion of external coat included in it. If you examine the ligature when it comes away a few days hence, you will find that it includes a substance, which is the piece of external coat that has

died, in consequence of the constriction it has suffered. If the sloughing or ulceration go beyond due bounds, the lymph will be no longer able to prevent the clot from being forced out by the heart's action. The clot could do little good of itself, but it diminishes the force of the blood upon the barrier of lymph, and the longer the coagulum, the more effectual will it be in this way; hence the further the ligature is removed from a large branch, the greater will be the patient's safety. Now, the femoral artery varies in its point of origin from the common femoral, and it is very important that the ligature should be applied considerably below its lowest origin; hence one great element for safety is to tie low enough, and I do not doubt that some cases of hemorrhage have occurred in consequence of the ligature being applied too high up. I have seen many persons perform this operation at a higher point than I am in the custom of doing, which is where the artery has become overlapped by the sartorius muscle. The operation is rendered a little more difficult by the overlapping of the muscle, but this is altogether a secondary consideration. There is, however, a limit to the distance downwards: if you go beyond the sartorius, as Hunter did, you will be too near the aneurism. Whatever tends to denude the artery from its connexions will increase the extent of sloughing of the arterial coats; and whatever bruises or lacerates the surrounding textures, and so interferes with union by the first intention, will have the effect of exposing the vessel, subsequently to the operation, during the process of suppuration that will ensue.

In performing the operation, I recommend you to use nothing but an ordinary knife, a pair of dissecting forceps, a needle for passing the ligature, and some hooks for holding the parts aside. Do not allow your assistant to draw the skin towards him while you make your incision, but, in order to keep the integument from slipping, place your hand across the thigh, and cut from it along the course of the vessel. After opening the fascia, you may perhaps be immediately upon the artery; this you may ascertain by feeling for its pulsation with your finger, which may be used for this purpose, but never for separating the textures. I think it better to divide the fascia over the sartorius, which is recognised by the direction of its fibres, when a little dissection brings you into the muscular interspace. I beg now to show you a thing, which you may think of small consequence, but which I have found very useful—viz., this form of hook for keeping the parts aside. I met with it accidentally amongst a set of old instruments, but it has proved of great service, as it not only holds the muscles to one side, but also presses them down; and in this respect the difficulty of the operation is greatly diminished by chloroform, which prevents the tension of the muscles which occurs when the patient is conscious. You will find in this, as in many surgical operations, that the result often depends upon some apparently trivial matter, on which, however, turns the whole question of a successful or fatal issue.—*Lancet, Feb.* 10, 1855, *p.* 145.

63.—ON THE TREATMENT OF ANEURISM BY COMPRESSION.

By Wm. Fergusson, Esq., F.R.S., &c.

[On the practice of compressing the main artery leading to the aneurismal tumour Mr. Fergusson makes the following remarks:]

No doubt this plan is far less brilliant in its details than a daring cutting operation; and more credit is due to those who were the means of its revival, when it is remembered, that it was resorted to at a time when a successful operation was in continual practice with the most satisfactory results.

The advantage of compression in the treatment of external aneurism, when practicable, over that by ligature, is at once evident. By inflicting one kind of injury, we are often enabled to get rid of a still greater mischief, and this is considered good surgery; but if, by a process less dangerous and hazardous, and without performing a cutting operation, the same disease or injury can be cured, surely it is a better style of surgery, and one that approaches nearer to the perfection of our art.

The history of the case is short; it is briefly this:—A. B., aged 22, married. Has always enjoyed pretty good health. The first symptom of her disease occurred twelve months ago. This was pain, confined to the popliteal space of the left leg; she could assign no reason for its coming on, as she never had any accident to that

leg. The pain, which has lasted to the present time, occurs in paroxysms of variable length, sometimes lasting for a whole day, and at other times not an hour, and accompanied with intervals of perfect ease. The pain is always aggravated by exercise, or by merely extending the leg. It has never been bad enough to prevent her following her ordinary occupation. Soon after its first occurrence, she noticed a hard, throbbing tumour, the size of a hazel-nut, in the centre of the ham; it was not tender to the touch, although it seemed to be the seat of the pain. It has remained almost unchanged since its first appearance; the only alteration that has taken place is a trifling increase in its size. It is quite firm and pulsating. Pulsation is completely arrested, if the superficial femoral be compressed. A bruit is heard on applying a stethoscope to the tumour.

There are one or two very singular features in this case. The first is, the occurrence of a popliteal aneurism in so young a person. You may never again see a similar case. I never met with one before in so young a subject, and I imagine hardly a similar instance is to be found in the records of English surgery. The chief point, however, to which I wish to direct your attention is, the plan of treatment that was pursued. It is one that was in vogue with surgeons two hundred years ago, and various accounts of it are to be found in the works of older authors. After the successful trial of Hunter's operation it was given up; but within the last few years it has been revived by some of the most eminent surgeons, especially those of Dublin, who have taken precedence in this interesting subject. A full account of the practice will be found in some of the established modern works, and a slight sketch in my own work on 'Practical Surgery.' Since the revival of this method, there has been sufficient success to strike most surgeons with astonishment; for out of a long list of cases treated by compression, success has been great beyond anticipation, and much greater than when the disease had been subjected to the treatment by ligature.

This method of treatment is now very generally sanctioned by the profession, and I am not aware of any distinguished surgeon, save one, who strongly objects to at least a trial of this plan. I allude to Mr. Syme, who has been remarkably successful in a great number of cases in which he used the ligature: for he states that in tying the superficial femoral for the cure of popliteal aneurism in upwards of twenty cases, he has not met with a fatal result. But I have seen the artery tied equally well by other surgeons of eminence, in well selected cases, though not with the same beneficial effects. Mr. Syme has expressed his opinion so strongly in favour of the ligature over compression, that he has endeavoured to cast a certain amount of discredit on those who have ably devoted their time and energy to the trial of a more simple and less hazardous cure of this formidable disease. On the revival of compression for the cure of popliteal aneurism, several improvements were made with regard to the manner of applying the pressure, and old notions regarding the duration of time it was necessary to employ were soon eradicated. It was early found that there was no need to apply the pressure so forcibly as to keep the sides of the vessel together, thereby inducing inflammation and subsequent adhesion, which led to the obliteration of the canal; for the operation of Bradsor and Wardrop of placing a ligature on the distal side of the tumour, showed that to obtain a satisfactory result it was not necessary to cut off the entire flow of blood through the tumour; and subsequent experience has proved, that in the treatment of compression it is not requisite so to do, but that gentle and continuous pressure modifying the force of the current, is quite sufficient, in the generality of cases, to cause consolidation within the tumour.

Again, the pressure need not be constantly applied to one particular spot, but may be shifted from place to place along the course of the artery, so as to give the patient as little pain and annoyance as possible.

The result to be hoped for in the treatment of these cases is, coagulation of blood and deposition of fibrin in the aneurismal tumour, which, as I have said, can be produced in many cases by simply modifying the flow of blood through the main artery. The femoral artery is never obliterated at the point where pressure is applied, but can be traced almost down to the tumour itself. If the case go on favourably, and the patient be a proper subject for the plan of treatment, a cure may take place in a few days, or in a week or two; but if, on the other hand, the patient be not content to put up with the inconvenience and pain for a time, the cure may be protracted for

a lengthened period. This was well illustrated in a case I had under my care some time ago, when, owing to the extreme irritability of the skin and the condition of the patient, sloughing occurred in the groin, occasioned by the pressure, which greatly retarded the cure.

I do not intend to enter more fully into the merits of the several methods for the cure of aneurism, but shall content myself with stating the treatment I employed in the case under notice, and the results which followed. The aneurism belonged to that class which by Scarpa was termed a "mixed aneurism;" that is, there was probably a dilatation of all the tunics, and then subsequent rupture of the two internal coats.

I employed the instrument that is most in favour among the Irish surgeons, Carté's "circular compressor," in which a very ingenious addition to the common screw force is applied by means of bands of caoutchouc, whereby a certain resiliency is acquired, which keeps up effectual pressure, and at the same time possibly obviates any injurious effects from the screw. Among other contrivances, I may mention the tourniquet of Signoroni; also the one invented by my friend Mr. Bulley, of Reading, which has two screw pads, allowing the pressure to be applied at different spots at various periods. There are other equally ingenious instruments, which possess their individual merits.

At first, I must confess, I did not anticipate a favourable result, as the patient did not appear to be fully aware of the great danger of the disease, and was seemingly very loth to submit to the treatment; but, by explaining to her the importance of resolutely bearing the inconvenience attendant upon such a plan, in all probability a more serious operation would be avoided, she accordingly determined to aid us in the cure; and the following sketch of the treatment shows the satisfactory result:—

I applied the pressure to the common femoral immediately below Poupart's ligament, regulating the force to such a degree as to keep up an amount of pressure just sufficient to stop the pulsation in the tumour. It was continued for twenty hours, when some uneasiness being complained of, it was omitted for an hour or two, and then re-applied. On the third day the pulsation was not so full or throbbing as before. On the ninth day the pressure was modified to such an extent, sufficient to weaken, but not completely to arrest, the flow of blood into the artery. On the fourteenth day, the woman not feeling at all well, all pressure was removed for a few days. On the nineteenth day it was re-adjusted, but, in the evening of that day, the pulsation in the tumour became feeble in a marked degree, and some slight pain was complained of in the ham. On the twenty-first day all pulsation had completely ceased, and the tumour was found diminished in size and of considerable hardness. It was now evident that a cure was effected; and no return of the disease has as yet shown itself.

The most interesting point in the treatment of this case is the rapidity and easiness with which this disease was cured; and it also well illustrates the fact, that it is not always absolutely necessary to make continuous pressure on the artery for any length of time.—*Med. Times and Gazette, Dec.* 30, 1854, *p.* 657.

64.—*On Popliteal Aneurism.*—Mr. Cock placed a ligature on the femoral artery in a case at Guy's Hospital on the 24th ult. in which a large popliteal aneurism appeared on the point of bursting, and in which the mode of cure by compression had not succeeded as well as he had anticipated. It was interesting and instructive to find, as far as London is concerned, that such an operation now at Guy's and several other hospitals is entirely exceptional, the general impression of the surgeons of the larger hospitals being quite in favour of the milder plan of treatment by compression. The man is still under treatment, and doing well; and though a few years ago such operations by the ligature were the rule, and presented little of novelty, the case is of interest as an exception rather to an opposite rule, the plan now generally adopted.

No little controversy has been excited of late years as to what is the best plan of compression in aneurism—a screw, a weight, or a spring. By the almost unanimous consent, however, of all our hospital surgeons, *elastic* pressure seems to be that now generally adopted. In aneurism of any of the arteries of the upper extremities,

elastic pressure is more particularly advisable, while a weight of lead or shot may in many cases of popliteal aneurism be found available in controlling the circulation, more particularly at the groin, the screw apparatus having been entirely discarded; the screw tourniquet or clamp, always, amongst other disadvantages, gives rise to impatience of pressure on the part of the patient—he feels as if "pinioned" down or nailed to his bed; while, on the other hand, the elastic spring, if properly placed on the limb, is rather agreeable to the patient than otherwise, and will be found quite sufficient for restraining the current of the blood through the aneurismal sac. Indeed, in a case lately given, a patient continued the spring apparatus of Dr. Carté for seven hours without stopping, at the end of which period the pulsation in the tumour of the aneurism had ceased; the latter had become solid, and nothing else was required to complete the cure, absorption going on rapidly; the screw clamp, on the contrary, in this case, could not be borne even for half an hour. Mr. Teale, of Leeds, lately gave a case cured by compression in twelve days. We have attended with some interest to a case lately treated by Mr. Adams at the London Hospital, and others under Mr. Fergusson.

The statistics of compression are now very satisfactory: of 23 cases of aneurism treated in this manner in London up to last year, 14 had been cured, while in 9 it had partially failed. Very considerable judgment is necessary in selecting cases, and the most vigilant care is required to watch that the few simple directions necessary are properly carried out by the patient; half the cure is in the patient himself. It should not be forgotten, too, that compression, if it do not succeed, as in this case of Mr. Cock's, may do positive harm; if the instrument shifts its place off the vessel, the treatment is less than nugatory, as it leads to engorgement of the limb, and diffusion of the contents of the sac of the aneurism. It is too often said, perhaps, the "clamp" can do no harm; but this is not the case. Besides these 23 cases, it is reported that 47 cases have been treated in Dublin with similar results: 36 were completely cured. These are all the cases from 1842 to 1854, taken indiscriminately.—*Lancet, Feb.* 24, 1855, *p.* 210.

65.—*Treatment of Aneurismal Tumour by Acetate of Lead.* By M. LASSANA.—[The diagnosis of aneurism is sometimes obscure, especially when we have to distinguish between an aneurism of the facial artery and a nævus. This subjoined case was one of nævus.]

M. Gelmi, a young girl, aged 22, had from earliest times a soft fluctuation and smooth tumour in the substance of the left cheek, between the commissure of the mouth and the angle of the jaw. It was ovoid in form, and the size of a nut. A puncture gave issue to a jet of blood. In the month of February last M. Lassana punctured the tumour from the mouth with a sharp lancet; again a rapid jet of blood flowed out; but he immediately plunged the canula of a glass syringe, filled with a solution of acetate of lead, into the wound; he propelled from the instrument eight or ten drops, and, after having withdrawn it, held his finger applied to the puncture for about a minute. There was no further flow of blood. The tumour, fluctuating and soft as it was before, became solid and hard in less than a minute, and was also somewhat increased in size. No inflammatory phenomena supervened. The swelling diminished by degrees, and the tumour, hard and indolent, became reduced to the volume of a small kernel.—*Annali Univ. di Med.—Med. Times and Gazette, Jan.* 27, 1855, *p.* 93.

66.—*Case of Varicose Aneurism cured by Injection of Iron.* By M. JOBERT.—A young man, named Leopold Goëb, aged 18, consulted M. Jobert on the 20th Feb., about a small tumour, situated at the bend of the elbow, which presented all the characters of a varicose aneurism. The tumour disappeared wholly on pressure, but only partially upon compression of the brachial artery. It seemed to be formed by the passage of the blood from the brachial artery into the median basilic vein. Its pulsations were synchronous with those of the heart. It was occasioned by venesection.

Having previously compressed the brachial artery, M. Jobert introduced a trocar

into the tumour. Venous blood escaped through the canula. Six drops of a solution of perchloride of iron were injected into the sac through the canula, and the external wound was closed by means of adhesive plaster. Next day a coagulum seemed to have formed in the tumour, the pulsations were less vigorous, and the aneurismal *bruit* less distinctly audible. In a day or two, however, the apparent solidification had disappeared, and the pulsations and *bruit* had returned. On the 24th Feb., M. Jobert repeated the injection; and on withdrawing the compression of the artery, a jet of red blood, mixed with the perchloride, was thrown out. Jobert therefore injected the fluid while the artery was compressed. The tumour solidified, and its pulsations ceased; but violent pain was felt in the forearm, and a sensation of heat extended along the course of the radial and ulnar arteries. The muscles of the forearm were spasmodically contracted, and the patient was feverish and sleepless. However, the disagreeable symptoms finally abated, and the tumour continued to become harder; and three days after the operation it was quite solid, resistant, and non-pulsating. The brachial artery had become obliterated for about an inch above and below the tumour, and was converted into a solid fibrous cord.

M. Jobert attributes the failure of the first attempt to the perchloride not having been mixed with a sufficient quantity of blood. The coagulum formed should completely obstruct the artery. He believes the constitutional symptoms that occurred to have resulted from arteritis; hence he thinks this method of cure should only be tried in cases where the aneurismal sacs are of recent formation, and are free from inflammation and disease.—*Gaz. Méd.—Monthly Journal of Med., Dec.* 1854, *p.* 557.

67.—ON THE TREATMENT OF VARIX.

By Henry T. Chapman, Esq., late Surgeon of St. George's and St. James's Dispensary.

[Mr. Chapman believes that varix is not cured by obliteration of the diseased vein, and that, moreover, the limb must suffer more or less from this obliteration, besides the necessary risk of phlebitis. Most surgeons seem to found their success on the degree of inflammation to which the vein is subjected, not excepting the recent practice of galvano-puncture. Mr. Chapman says,]

When complete obliteration of the vein, therefore, has happened after galvano-puncture, it is clearly not due to the mere coagulation of its contents, but is the result of acute inflammation producing the consolidation of a considerable portion of the vessel.

I should not have thought it worth while to revert to the rather hasty conclusions drawn from the employment of galvano-puncture, were it not for the probability that similar claims may ere long be advanced in behalf of other agents, such as the perchloride of iron, which also possess the property of coagulating the blood within the vessels. But should any hopes of the kind be entertained, the following observations of Hasse ought to be sufficient to dispel them entirely.

"It is surprising," he remarks, "in how short a space of time a plastic plug, extending through numerous branches, and even filling a tolerably large trunk, will disappear, so that canals previously impervious are again opened to the circulation." He adds in a note, "This fortunate termination of phlebitis, I have experienced in my own person. The whole system of the saphena, up to its insertion in the crural vein, had become blocked up by plastic lymph. Even the minute twigs of the corium had, by the formation of pustules beneath the epidermis, given proof of active participation in the disease; and yet *within five weeks* of the inflammation subsiding, the circulation was fully restored in almost all the veins as before."

In the modification of ligature by the needle and twisted suture, more recently introduced by Mr. Henry Lee, (*i. e.*, removal of the needle and division of the vein on the second or third day after constriction,) he may possibly attain one of his objects, and prevent absolutely the future restoration of the canal of the divided vessel itself. But granting that it never again becomes pervious, the patient is, in the long run, no gainer by its obliteration, if, as we have seen it explicitly affirmed by Sir B. Brodie, Mr. Hodgson, Mr. Vincent, and others, before many months have elapsed its place is supplied by one or more veins of inferior calibre, of course abnormally dilated, to convey a larger volume of blood than is natural to them.

That this is not merely the occasional but the ordinary sequel of obliteration of a venous trunk, whether it take place naturally or artificially, will scarcely be disputed. A case I have published elsewhere, will presently be cited, in which the saphena interna was converted into an impervious cord by the insertion of five needles beneath it; and yet within twelve months a varicose vein had been developed, alongside this cord, quite equal in size to the original varix, and occasioning even a greater degree of suffering. In July, 1853, I met with a similar example, but in this second case the relapse occurred in less than four months after the occlusion of the trunk, the operation having been performed in the March previous. Both these patients informed me that they suffered most severely during the process of obliteration, that they were confined to bed for some weeks, and that it was very long before they were able to walk without pain and lameness. Had the veins been divided after ligature, in accordance with Mr. Henry Lee's practice, it is very probable that much of this suffering would have been avoided, and that their recovery would have been incomparably more rapid.

Nevertheless, if the danger of phlebitis is considered by Sir B. Brodie to be a sufficient reason for abandoning the simple subcutaneous section of the vein, Mr. Lee's operation cannot be quite so safe as he appears to regard it. A case, indeed, fully bearing me out in this opinion, which was operated on by him, November 12th, 1852, is referred to in the *Lancet* for March 18th, 1854. Ligature and division of the vein were followed by inflammation, abscess in the leg and groin, and pyogenic fever. On the thirty-eighth day the foot and ankle were still in a painful œdematous condition; and in addition to these accidents, which so commonly attend the operation, it is recorded that "during the height of the fever, the patient had been seized with severe pain in his right wrist. This pain subsequently diminished, but in the eleventh week again became very acute, the fingers were quite powerless and extended, and the forearm and wrist perfectly useless, though no external ulceration or inflammation had taken place." This occurred in the winter 1852—3: and the return of the patient to King's College Hospital in the spring of 1854, afforded the reporter of the case an opportunity of ascertaining that, after a lapse of more than a year, the paralysis of the forearm still continued.

By those who still practise the ligature and section of veins for the cure of varix, —and even of varicose ulcers,—it is urged that, in the majority of instances, no serious consequences ensue, and that death from this cause is a comparatively rare accident. But phlebitis once provoked, is so little under our control, that, until he can say to this disease,—"Thus far shalt thou go and no farther,"—I cannot bring myself to believe that any surgeon is justified in deliberately encountering so formidable a risk, remote as he may deem it, for the sake of an advantage which is at best merely transitory. A single example of such an operation terminating fatally, ought, in my judgment, to outweigh five hundred cases attended by no immediately disastrous results.

I shall now proceed to describe the plan of treating varix, alluded to at the commencement of this paper, which has at any rate the merit of being both safe and simple.

Curative treatment of Varix by wet strapping and bandaging.—One of the writers, I might almost say the only one, who has spoken confidently of the actual curability of varix, is the late Mr. Vincent, whose lengthened experience as Senior Surgeon to St. Bartholomew's Hospital entitles his opinion to great consideration. At pp. 226 and 228 of his "Observations on some of the points of Surgical practice," he expresses his conviction that veins of the leg, when diseased, will, if the limb is maintained in the horizontal position, and in perfect repose, gradually recover from their thickening and enlargement. "This fact," says Mr. Vincent, "I have observed." Again he remarks, "I have no doubt, that if patients would submit to the confinement, that by means of continued rest, and leeching where this step is required, all cases of varix may be fully relieved."

Since the immediate source of varix is an inability on the part of the coats of the veins to withstand the pressure from within to which they are subjected in the erect posture, the most natural and direct course we can pursue for their relief, is the adoption of measures calculated to diminish that pressure as much as possible. This may, of course, be instantly effected by confining the patient to the recumbent position, as Mr. Vincent has proposed, when all trace of the malady will entirely

disappear. But the maintenance of this posture, excepting under very aggravated circumstances, is manifestly impracticable. The remedy would be worse than the disease. All that is usually attempted, therefore, is to endeavour to assist the vein in the discharge of its function by giving it support from without, and thus palliating the infirmity.—*Med. Times and Gazette, March* 17, 1855, *p.* 256.

68.—*On Galvano-puncture in the Treatment of Varices and Aneurisms.* By Dr. Werner Steinlein.—The favourable results which have been obtained by the employment of galvano-puncture should induce surgeons to seek for yet surer methods of using it, especially such as expose the patient to less risk of inflammation and suppuration.

Baumgarten and Würtenburg, after the fact announced by Schuh, that coagulation is produced more promptly at the positive than the negative pole, instituted experiments upon the action of the poles taken separately. They obtained the following results:—

1. If the negative pole was introduced alone into a vessel, the positive being applied against the neighbouring parts, there was no coagulation. 2. The two poles introduced into the vessel produced slow, feeble, and rarely complete coagulation. 3. The positive pole, introduced alone, determined rapid and complete coagulation. To turn these results to use, it becomes necessary to inquire, What is the action of the poles upon albumen, fibrin, &c.? M. Steinlein has undertaken this, employing Grove's battery, with poles of platinum, and zinc: the first positive; the second negative. The experiments upon albumen gave the following results:—1. If two conductors of platinum are plunged into a solution of albumen, no coagulation is produced, and test-paper shows no re-action. 2. If the platinum needle of the positive pole be replaced by one of steel, little bubbles are disengaged from the negative pole, forming a thickish layer; the reddened test-paper, applied to the platinum needle, becomes blue; the steel needle at the positive pole becomes slowly covered by coagulated albumen, with no disengagement of bubbles. This needle also becomes bromeo-coloured. 3. If a zinc needle be used instead of one of iron or steel, the coagulation is much more rapid.

It follows, from these experiments, that the coagulation of albumen takes place only at the positive pole, and that the coagulating force varies according to the nature of the metal employed. The author then endeavours to explain these results according to physical and chemical laws, and he advises, in acupuncture, that the steel needles should be superseded by those of zinc, or rather by needles covered by a layer of this metal. The galvano-puncture should then be practised in the following manner:—A certain number of needles, connected with the positive pole of a galvanic battery, are inserted into the tumour, whether the sac of an aneurism or the cavity of an aneurismal varix. The negative pole should be supplied with a plate of platinum, which must be put upon the skin adjacent to the aneurism, after having augmented the conductibility of the epidermis by moistening it with a saline or acidulated solution.—*Zeit. der Gesell.—Med. Times and Gazette, Dec.* 16, 1854, *p.* 620.

69.—ON BENZOIC ACID DISSOLVED IN ALUM AS A NEW HÆMOSTATIC AGENT.

By M. Monsel, and Professor Hannon.

[The following experiments have been tried to test the effect of this agent.]

Application of the Hæmostatic Agent prepared by M. Monsel to the Facial Artery of a Horse.—Experiment 1.—The animal having been shackled and thrown down, the left facial artery was exposed on the inferior maxilla in front of the masseter, and was opened by a longitudinal incision of 4½ lines. The arterial jet having filled several glass tubes, each holding about half an ounce, a sponge soaked in the hæmostatic agent was put upon the wound, and held there by tight compression. The imbibition was often renewed during a quarter of an hour. The hemorrhage recommenced when the compression was relaxed. It being thought that the violent

plunges of the animal interfered with the success of the experiment, the sponge was introduced under the integument, which was then tied over it by sutures; the animal was raised, and the head tied high to a manger. From this moment no further bleeding ensued.

The Blood in the Glass Tubes.—Into the tubes, before the addition of the blood, there had been poured half an ounce and ten mimims of both the eau Pagliari and of Monsel's liquid. Coagulation took place equally rapidly in the two sets of vessels.

Employment of the same Fluid on a Dog.—Experiment 2.—Having ascertained on the day following (May 28) that the hemorrhage was definitively arrested in the horse, whose head was still maintained in the same position, a large superficial wound was made in the inner part of the thigh of a middle-sized dog. The solution, applied by means of a sponge, formed a clot in ten minutes; and, by the tight adjustment of a circular bandage, the hemorrhage was definitively arrested.

A few minutes afterwards, a large and deep incision was made into the right thigh of the same dog, and a profuse flow of blood in jets issued from the femoral artery. The hæmostatic agent was applied to the gaping vessel directly by means of a tube; but it failed to arrest the bleeding. Compression was then made at the bend of the thigh upon the vessel, and the soaked sponge, held to the wound, sufficed, as in the former trial, to stop the flow of blood in ten minutes. A tight bandage was applied, and the dog was permitted to rise. The animal licked its wound, and crouched under the table; but there was no return of the hemorrhage, as was half expected by those about. The day following (29th) the dog was alive, to the great surprise of all; and it was seen, from the spot where he lay, that no bleeding had taken place during the night. M. Renard then exposed and opened the carotid artery, and treated the wound in a similar way with equal success.

Autopsy of the Dog.—The animal, whose exhaustion was extreme, died, and was examined. The end of the femoral artery (which vessel had been completely cut through) was obliterated by a plug of coagulum, of red colour, and 2¼ lines long. In the neck an oval red clot, the size of a nut, was implanted by a pedicle into the carotid artery; this pedicle extended upwards and downwards for some extent within the vessel. The prolongation towards the brain was in the form of a T.

The carotid artery was opened and simply plugged in another dog. Hemorrhage was temporarily arrested by these means; but, owing to the movements of the animal, the wound reopened; the bleeding recommenced, and terminated fatally. Upon examination, no clots were found in the wounded vessels.

Continuation of the Experiment on the Horse.—On the fourth day the dressings were removed, and there was no return of bleeding, no functional disturbance.

June 9. The animal was killed. The facial artery was obliterated, but was found, together with the vein, the nerve, and Steno's duct, imbedded in a quantity of lardaceous matter, the result of the glanders, which had been preceded by farcy. The other morbid changes were, thick white pus in the left frontal and maxillary sinuses; hypertrophy and ulceration of the mucous membrane in those sinuses; ulceration of the pituitary membrane of the same side; miliary albuminoid tubercles in the lungs, the spleen, and the liver.

Employment of the same Liquid on Man.—Hôpital St. André; service of M. Jacquot.—Experiment 4.—Profuse hemorrhage having flowed from the recently cut edges of an old ulcerated bubo in the groin, M. Geoffroy tried in vain to arrest it by means of the ordinary compress. A pledget of charpie, soaked in the hæmostatic liquid by M. Monsel, and placed upon the part, instantly and definitively stopped the bleedings.

Two similar and equally successful trials were made also by M. Renard upon patients in the same Hospital.

M. Monsel is pursuing his investigations still further, with an energy which does him infinite credit. Upon adding ten grains of tannic acid, and a scruple of alum deprived of its iron, to an ounce and a half of rose-water, he obtained a fluid very efficacious in coagulating the blood issuing from an open vein.—*Gazette Méd.*

Professor Hannon recommends benzoic acid, part one; alum, parts three: ergotin, parts three; water, parts twenty-five; the whole to be boiled thirty minutes, with constant stirring and renewal of water; then it must be evaporated to the consistence of an extract. The colour is chocolate-brown; the taste strongly astringent;

the smell that of ergotin. A layer spread over this bleeding vessel produced instant coagulation. Compression is not necessary.

For *internal hemorrhage* is used, acid benzoic gr. i., alum. pulv., ergotin, aa. gr. iij. Ft. pil. xvi. One pill every two hours.—*Presse Méd.—Med. Times and Gazette, Jan.* 20, 1855, *p.* 64.

ALIMENTARY CANAL.

70.—ON THE RADICAL CURE OF HERNIA BY INJECTIONS OF IODINE.

M. JOBERT lately presented to the Academy of Medicine three cases of inguinal hernia in which this new mode of treatment had been productive of the best results.

The first case was that of a young man, 18 years old, with complete inguinal hernia of the left side, which, though it did not interfere with his ordinary avocations, rendered him ineligible for the military service. The tumour did not descend to the bottom of the scrotum, and it was separated from the testis by a constricting band. It was easily reducible by taxis, but reappeared upon the patient coughing or assuming the erect posture.

At the patient's request, M. Jobert performed the following operation on the 12th May. An incision, 2 centim. in length, having been made over the inguinal canal, a fine trocar was introduced into the sac, through which 5 grammes of pure tincture of iodine were injected. The wound was then closed by means of twisted sutures, and dressed with simple ointment. The patient suffered almost no pain. In the evening there was slight tumefaction and pain of the inguinal region, but no fever or constitutional disturbance.

May 14. The swelling and redness were considerable. The sutures were removed, and the wound was found imperfectly healed.

In a few days the external wound cicatrised, and the swelling and redness disappeared. The left testicle was not atrophied. On 5th June the patient was able to go about, and to cough without the least inconvenience or reappearance of the tumour. As a precautionary measure, he continued to wear a suspensory bandage for some time.

The second case was that of a patient, æt. 34, who entered the Hôtel Dieu Nov. 18, 1853,having not only a congenital inguinal hernia, but also a hydrocele of the same side. These were separated from each other by an annular constriction of the tunica vaginalis, which contained the protruded bowel above, and the testicle and spermo fluid below. M. Jobert resolved to obliterate the tunica vaginalis, for the purpose of curing both the hydrocele and the hernia at the same time. He accordingly passed a trocar obliquely into the lower portion of the constricted sac, and evacuated the serosity therein contained; and then, having previously made pressure with the finger over the inguinal canal to prevent the possibility of there being any communication between the tunica vaginalis and the peritoneum, he injected some tincture of iodine into the tunica vaginalis and the hernial sac. The external wound was finally closed by adhesive plaster. Next day the scrotum was reddened, somewhat tender to touch, and distended with fluid effused from the tunica vaginalis; but there was no pain or constitutional disturbance. On the 5th Dec. the swelling, which had been gradually subsiding, had nearly disappeared; and on the 12th the scrotum had regained its normal size. The scrotum and the spermatic cord on the affected side were firmer than before; and a cylindrical cord extended along the whole of the inguinal canal, as well as along the spermatic vessels down to the testicle. The patient was completely cured, and he left the hospital able to walk easily and without fatigue, and without any reappearance of the hernial tumour.

The third case occurred in a young man, aged 27, who had had a large oblique inguinal hernia of the left side, for eight years, for which he had worn all sorts of trusses without benefit. At the desire of the patient, and after a consultation with his colleagues, MM. Rayer, Begin, and Cloquet, M. Jobert punctured the hernial sac, and injected the tincture of iodine into the tunica vaginalis. The patient was laid in the horizontal posture after the operation; his legs were flexed on his thighs, and maintained in this position by means of a bolster. The hernial sac swelled im-

mediately after the injection, and in fifteen hours it was as large as the hernial tumour, and had a fluctuating, half-solid feeling. The parts continued thus for eight days, and then the swelling speedily diminished. On the twenty-eighth day after the operation the cure was complete; the track of the spermatic cord was occupied by a hard and compact cylindrical substance, which completely prevented any return of the hernial protrusion, and allowed the patient to go about without any recurrence of his malady. Four months had elapsed at the time when M. Jobert related his case, and the cure was still as complete as at first; the inguinal canal was filled with a hard, solid cord; and there was no atrophy of the testicle.

Remarks.—When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous—M. Jobert recommends puncturing the sac at once with a trocar, without previously making an incision. The trocar is to be inserted perpendicularly at first, then obliquely upwards, and, when it has reached a certain depth, it is to be directed cautiously backwards so as to puncture the sac—the accomplishment of which process is known by the operator no longer meeting with resistance, and by his feeling the canula move about without obstruction in the empty sac. Having entered the sac, the pure tincture of iodine should be injected into it by means of a glass syringe—whereby we may be able to know how much fluid has been used. M. Jobert ordinarily injects one or two spoonfuls (*cuillerées*) of the tincture, but often much less. The quantity injected should always be in proportion to the size of the serous pouch. The injection should be suspended whenever we feel an obstacle to the entrance of the liquid. Instead of allowing the fluid to escape through the canula, M. Jobert withdraws it by means of the suction of the syringe.

When the sac is thin, moveable, and easily displaced, Jobert exposes it by dividing the integuments with a bistoury, and *thereafter* punctures it with the trocar—*never with the knife.* In such cases he unites the lips of the external wound by means of twisted sutures.

What occurs after the operation? Immediately after the injected fluid is withdrawn, a new liquid begins to be thrown out within the hernial sac. After twenty-four or forty-eight hours have elapsed, the sac is filled with this fluid; it continues in this state for eight days, and after the first week it begins to diminish gradually and visibly in bulk. In proportion as the tumour diminishes it begins to acquire solidity, and becomes as firm as wood; nay, it eventually becomes as hard as bone or ivory.

M. Jobert affirms that this method by injection is superior to all the other operative procedures which have yet been devised for the radical cure of hernia, inasmuch as it is at once simple and harmless. M. Jobert, in conclusion, acknowledges that this operation is not invented, but merely revived, by him; inasmuch as M. Velpeau, in his Annales de Chirurgie, published ten years ago, first suggested the likelihood of injections of iodine proving beneficial for the radical cure of reducible hernia.—*Gazette des Hôpitaux.—Monthly Journal of Med., Dec.* 1854, *p.* 555.

71.—*On Strangulated Hernia.*—Mr. STANLEY'S long experience has taught him the danger of too much handling or manipulating of hernia when incarcerated or strangulated. Indeed, in seven out of ten of the cases sent to the hospital, he is inclined to think one will find omentum, not intestine, as the chief constituent of the tumour, which it is impossible to get back by the rough usage adopted.

In fact, from what Mr. Stanley sees every year, he finds it necessary every season to caution his class against this great error of routine practice, and has adopted the wise maxim of Desault—"think favourably of every hernia if the taxis has not been tried." Mr. Stanley advises to make one fair trial of the taxis when first called in; the little round swelling of femoral hernia is always discoverable in front of the pectinalis muscle. We should relax Poupart's ligament by having the thigh turned up and knee inwards—ten to fifteen minutes will be enough; if pain is present, not even so long. He next recommends a trial of the taxis in a warm bath, and we have seen at St. Thomas's Hospital, in place of this, the topical application of ice. If either of these fail, he now recommends the *dernier ressort*, chloroform and the knife—one careful trial, but the last, under the anæsthetic, before cutting down. If

the parts are gangrenous, he lays much stress on the necessity of disturbing them as little as possible, so as to prevent the contents of the sac getting into the peritoneum.—*Lancet, Feb.* 17, 1855, *p.* 185.

72.—*Hernia treated by the Topical Application of Ice.*—We were particularly struck by some observations lately made by Mr. Clark at St. Thomas's Hospital. He sees some reason to doubt the opinion of Sir Astley Cooper, so generally followed, that the inguinal canal is endowed with muscular contraction, which, under the influence of the abdominal muscles, of course, would close or open it. But Mr. Clark agrees, we believe, rather with Scarpa, Pott, Cloquet, &c., that the canal is altogether passive; the chief obstacle to the return of the hernia being its great size. Mr. Clark believes even that tobacco, chloroform, tartar emetic, the warm bath, &c., are not so effectual in reducing an inguinal hernia as the diminution of its size induced by local application of ice, and that the operations on hernia at St. Thomas's Hospital will bear him out in this conclusion, that such medicines and appliances too often protract the cure of the case, or do not act except in a passive or negative form. The topical application of ice, on the other hand, corrugates the tumour, and makes its volume obviously smaller. Mr. Clark seems, curiously enough, an advocate also for opening the sac in femoral and inguinal hernia. He believes, as we have heard him say, that not opening the sac in strangulated hernia is founded on a false idea or analogy—viz., that we can irritate a membrane *more*, already perhaps irritated as much as it will bear. He does not think an unhealthy hernial sac is at all the same as a *healthy* peritoneal membrane, which he should not, of course, wish to wound; and we should never run the risk, he says, of not examining the hernia fully. The question is obviously one of very great practical every-day clinical importance. The opinions of all our various hospital surgeons, however, are not in accordance with those in vogue at St. Thomas's. As far as we can learn, Sir Astley Cooper recommended the use of ice and cold applications to hernia. It was tried in his time chiefly in conjunction with tobacco enemata, the anæsthetic influence of chloroform or ice at that time not being understood. Since then, during the last half dozen years, the use of ice, as a local agent, as an anæsthetic, has given it an importance not at first perhaps suspected. Sir Astley Cooper pounded the ice into rough powder, and placed it in a bladder over the hernia. When ice was not procurable, he mixed equal parts of nitre and sal ammoniac, ten ounces of such salts being added to a pint of water. "If, after four hours," he says, "the symptoms of strangulation become mitigated and the tumour lessens, this remedy may be persevered in for some time longer; but if they continue with unabated violence, and the tumour resist every attempt at reduction, no further trial should be made." This seems a very long time; and a caution is added, not to freeze the scrotum and bring on sloughing.—*Lancet, March* 3, 1855, *p.* 238.

73.—*On the Value of Cough-Impulse as a symptom of Hernia.*—Several instances have recently come under our notice illustrating the value of the knowledge of the fact, that *in tightly strangulated hernia no cough-impulse is ever felt.* In all the reducible or merely incarcerated forms the information derived by making the patient cough is so great and so conclusive, that it is not surprising that attempts should be made to apply the same test to cases in which strangulation exists. Entire forgetfulness of the condition of the bowel could, however, alone induce any expectation that impulses generated within the abdomen would be communicated to it. In all cases in which the constitutional symptoms of strangulation are present, the diagnosis as to whether the tumour be hernial or otherwise must be made without reference to its movements during coughing. Much valuable knowledge on other points may, however, be yet derived from the test. If—in the certainty from other signs that the tumour is really a hernia—there is not the least impulse, a fair inference may be drawn that the stricture is tight. If impulse be felt in the upper part of the neck of the tumour, and not in other parts, the seat of stricture may be accurately determined, being, that is to say, just below the spot where the perception of impulse is

lost. On this latter point Mr. Luke has, in his paper "On the Operation for Strangulated Hernia," 'Medical Gazette,' 1839–40, insisted strongly; indeed, we believe he was one of the first to direct attention to its value. With regard to the absence of cough-impulse in strangulated hernia generally, our surgical manuals have, for the most part, neglected to make any observation respecting it, while in some, from the terms used, it might be inferred that it was an ordinary symptom. It is still not infrequent to see cases reported in which its absence is noted as having been a source of difficulty and doubt. Most deeply would it be to be deplored should absence ever lead to delay in performing the operation, since it is, in fact, a conclusive indication of its necessity. We have not made the statement printed in italics above without first instituting much inquiry among those of most experience, and also availing ourselves of every opportunity which has of late occurred for acquiring data in respect to it. The following sentence in its support we quote from a note by Mr. South, in his edition of Chelius:—"I have on more than one occasion heard surgeons of eminence speak of dilatation of a strangulated rupture on coughing, which, I must confess, I think impossible if the rupture be more than incarcerated.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 616.

74.—*Fistula in Ano.* By JAMES SYME, Esq., Prof. of Surgery in the University of Edinburgh.—The discovery of the uniform existence of an internal opening near the anus may be said to have perfected the operation, but in consequence of the old errors having been so long prevalent, the treatment has not hitherto been nearly so satisfactory as it should have been in the profession at large; and hence the explanation of the fact, that patients affected with fistula in ano come here from all parts of the country, under the impression that the operation they are about to undergo is a very serious one, and involves long confinement to bed. The operation, though very simple in principle, and easy of performance, is still one that requires care and patience. Whenever you examine a fistula of six weeks' or two months' standing, you must proceed on the supposition that an internal opening exists. The track that leads to it may be tortuous, but you must search carefully again and again, if you fail to find the aperture in the first instance, and be very slow to be persuaded that it is not there. A piece of lint is placed in the wound at the time of the operation, and the only other dressing required is washing the part occasionally with soap-and-water for a few days.

The patient, R. McL., aged twenty-three, a coach-builder from Stirling, was then brought into the theatre, and stated that the disease had commenced as a small suppurating point near the anus, which caused no great pain, and broke of itself, discharging considerably less than a teaspoonful of pus, unmixed with feculent or gaseous matter. The external opening was very small and inconspicuous, but the internal opening was readily found, a little above the anus, by Mr. Syme, who, in order to demonstrate the existence of the inner aperture, and the small amount of soft parts between it and the surface, brought out the end of the probe at the anus, and cut out the probe with the bistoury, remarking that though he had pursued this course in the present instance for the sake of demonstration, yet it was one to be recommended in all cases where much difficulty was experienced in discovering the internal opening, as the most certain means of securing the object in view. A piece of lint was placed in the wound; no hemorrhage whatever occurred after the few drops of blood that were lost during the operation. He was discharged on the 25th December.—*Lancet, Jan.* 20, 1855, *p.* 57.

75.—ON THE TREATMENT OF PROLAPSUS ANI BY STRYCHNIA AND BY THE ACTUAL CAUTERY.

[Mr. ATHOL JOHNSON communicates some interesting cases of this affection, which were treated at the Hospital for Sick Children.]

Case 1.—Isabella Addington, aged two years, a thin child, of strumous aspect, was admitted May 6, with the rectum protruding about an inch, forming a large,

solid, red mass, with the aperture of the bowel in the centre, the sphincter being much dilated around its base. The child had been subject to prolapsus for some months, but for the last fortnight the bowel is said to have been down almost constantly, her mother being unable or afraid to return it.

By a little gentle manipulation, the gut was replaced; but the sphincter remained relaxed, so that two fingers could easily be introduced; and on the child beginning to cry, the prolapsus recurred immediately. The intestine was again returned, and this time kept up by means of a piece of gutta percha enveloped in lint and secured by a bandage. Mild laxatives, along with tonics, were administered, and a nourishing diet given. The bowel ceased to come down. The sphincter partially regained its tone, and the child was discharged May 16.

On the 3rd of June, she was again admitted. She had evidently been much neglected. The bowel was now protruding about an inch, and had been allowed to remain so for some days. The sphincter was much relaxed. It was now determined to try to produce a more permanent contraction of the muscle. Accordingly, a small blister having been applied to the cleft between the nates, one-twentieth grain of strychnia was applied. No convulsive twitches were noticed; but two hours afterwards, when a motion took place, the bowel did not descend.

On the following day, a fresh prolapsus took place, but not to the same extent as before. On the fourth day, one-sixteenth grain of strychnia was applied to the side of the anus, the cuticle having previously been removed. Pain was complained of, and the child seemed uneasy; but no muscular contractions were noticed.

Five days afterwards, the bowel still protruding a little after having a motion, one-sixteenth grain of strychnia was again applied in the same situation. From this time no further descent of the rectum occurred; the sphincter was more firmly contracted; and on June 30th the patient was discharged, and has not since applied for relief.

In this case the affection was not very severe, and, had the child received a fair amount of attention at home, it is probable that no surgical measures need have been had recourse to; as it was, however, the application of the strychnia certainly seemed attended with benefit; but some rather strong objections attended its use, which will be alluded to at the end of the next case.

Case 2.—Julia Seymour, aged 4, was admitted July 2, with the rectum protruding quite two inches; the mucous surface being inflamed and coated with a muco-purulent discharge. There was said to be great pain on any motion passing, so that her friends had allowed her bowels to be much confined, from their dread of ever giving her any opening medicine. She had been subject to prolapsus for about six months, and for the last six weeks the bowel has scarcely ever been in its natural position. After a little manipulation the gut was replaced; the sphincter, however, remained much relaxed, allowing a couple of fingers to be introduced through the anus, without any resistance.

Under the influence of rest and laxatives the mucous membrane became more healthy; but the sphincter not regaining its tone, and the bowel constantly descending, on the 6th July one-eighth grain of strychnia was applied over the tip of the coccyx, the cuticle having been previously removed. On the 7th the strychnia was repeated, and again on the 8th. The application of the strychnia produced a good deal of "twitching" about the sphincter, which lasted for upwards of half-an-hour, and, on one occasion, rather severe convulsive action of the muscles of the lower extremities.

Some, but no very decided benefit, followed the use of the strychnia, and its application was attended with so much inconvenience as to cause its discontinuance.

On the 26th July, the rectum still protruding to a considerable extent every time of a motion being passed, and the aperture of the anus being still in a relaxed state, the patient was rendered insensible by chloroform, and the actual cautery applied in four places, at the junction of the skin with the mucous membrane. The first time of having a motion after the operation the bowel descended slightly, but for some days afterwards it did not come down at all, and the anus remained in a much more contracted state. On the 23rd August, however, there being a little return of the prolapsus, the cautery was again applied, in one spot to the side of the anus.

There was again a slight repetition of the affection on the occasion of administering an injection to bring away some thread worms. On the 29th Sept., there having been no prolapsus for some time, she was discharged as cured.

Remarks.—In the ordinary run of cases of prolapsus ani among the children who apply at the hospital, attention to the state of the bowels, tonics, and astringent injections are usually sufficient. In those children, however, who have been neglected, and in whom the rectum has been suffered to remain down for a considerable time, the aperture of the anus becomes much dilated, the sphincter appears to have lost its tone, and the above-mentioned treatment fails to effect a cure. Is the local application of strychnia of service in these cases? In the comparatively mild forms of the disease, I think that it is of some use, but in the more aggravated cases it cannot be depended upon. Practically, too, I found considerable objections to its use in children. It is necessary first to remove the cuticle, and then to apply the strychnia several times. This proceeding, however, is attended with a certain amount of pain, and it becomes each time more and more difficult to keep the little patient sufficiently quiet to allow of the strychnia being properly administered; either more is used than is desired or even safe, or else, perhaps, in the struggle some is lost, and a sufficient quantity is not employed. Moreover, in practice it is not desirable, if it can be avoided, to have to repeat any operation of this kind frequently in young children; the little patient becomes fretful, impatient, and suspicious, the struggles are violent, and almost as much harm is done as good effected.

A more efficacious, as well as a more convenient measure, in these severe forms, would seem to be the application of the actual cautery, in the mode suggested by M. Guersant, of the Hospital for Children at Paris. This operation can be performed easily under chloroform; is, of course, unattended with suffering; and seldom requires to be repeated.

The iron, having a button-shaped point, well heated, is applied to the junction of the skin with the mucous membrane at the verge of the anus, usually in four separate points, to such an extent as to make a pretty decided eschar. It may be mentioned, that even when the patient is insensible, the bowel generally descends with some force during the operation, so that it is necessary to be prepared with a piece of sponge or lint to protect the protruded gut while the iron is being applied to other points. Scarcely any pain is complained of afterwards, the child being ready to play and laugh almost as soon as it recovers from the effects of the chloroform. Simple dressings may be applied to the resulting sores, which usually heal without much trouble or difficulty.

With regard to the *modus operandi* of this operation I am inclined to think, from the immediate benefit produced, that it acts, to some extent, at any rate, by the powerful stimulus to the sphincter muscle, increasing its tone, rather than by the resistance afforded to the descent of the gut by the contraction of any cicatrix which may subsequently be produced.

From the little experience I have already had, I certainly prefer this operation to the application of strychnia in those cases of prolapsus ani in which any surgical measures are required.—*Med. Times and Gazette, Nov.* 18, 1854, *p.* 521.

76.—*Prolapsus Ani treated with Nitrate of Silver.*—[Mr. Lloyd, of Bartholomew's Hospital, has succeeded in curing many of these tiresome cases.]

The plan is to smear the whole surface of the protruded bowel with the solid caustic, and then return it. The application is repeated once in a week or fortnight, as may be requisite. Mr. Lloyd informed us that he rarely found it necessary to employ it more than three or four times; and further, that although the plan had been one invariable resort with him, for a long series of years, that he had never known any untoward consequences to result. In cases in which the protruded bowel has become swollen, and is difficult of reduction, the effect of the caustic is surprising. In one such case, in which we witnessed its application, the mass could be easily seen to diminish in size under its influence. Mr. Lloyd does not limit the use of this remedy solely to prolapsus, but adopts it also in cases of hemorrhoidal congestion, and thickening of the mucous membrane about the verge of the anus.—*Med. Times and Gazette, Feb.* 10, 1855, *p.* 136.

77.—*Treatment of Piles.*—The following is the formula of an ointment often found very useful as an application to piles, when not of very large size. It is copied from the Pharmacopœia of Guy's Hospital:—

R. Gallarum contrit. 3ij.; opii (emolliti aquæ cum 3j.) 3ss.; liq. plumbi diacet. 3ij.; adipis ℥j. Misce. Ft. ung.

We have here combined the astringent properties of lead and of galls with the sedative and anodyne ones of opium. Of course, in having resort to local measures, the general ones which address themselves to the cause of the disease should never be neglected. On the use of mercurials and castor-oil, as the radical treatment of hemorrhoids, we had occasion to comment a few weeks ago. There is no doubt, however, that the sufferings of the patient may be materially mitigated by topical applications. If the piles be large, ulcerated, and very painful, the free application of nitric acid will be the best; if in a more chronic condition, the above ointment may be ordered, or lunar caustic may be freely smeared over the parts. A good formula for an astringent ointment is valuable, even if the other means be in a general way preferred, since, in private practice, cases are not unfrequently met with in which the surgeon must prescribe without being permitted to inspect the part.—*Med. Times and Gazette, April* 14, 1855, *p.* 362.

78.—*On Hemorrhage from Piles.* By M. Trousseau.—A woman, aged 38, pale, anæmic, and very weak from loss of blood from the rectum, entered M. Trousseau's wards. On examination the hemorrhage was found to depend upon piles. She was ordered injections of nitras argenti, of various degrees of strength—from 20 to 40 centigrammes of the salt, without any benefit. Enemata of rhatany were next tried, but also unsuccessfully. M. Trousseau therefore ordered an injection of 50 centigrammes of sulph. cupri, dissolved in a glassful of water, to be administered daily. The hemorrhage gradually became less, and in eight days had quite disappeared. The patient was ordered chalybeates internally.—*Gaz. des Hopitaux.*—*Monthly Journ. of Med.*, *Dec.* 1854, *p.* 562.

URINARY ORGANS.

79.—ON STRICTURE OF THE URETHRA.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[Mr. Syme's operations have now been sufficiently long before the public to be tested as to the permanency of the cures. The following case will show for itself.]

J. R., aged forty-four, a fisherman, from Prestonpans, was admitted into the Hospital on the 17th of March, 1852, on account of a stricture in the urethra of sixteen years' standing. Previously to admission he had been treated by bougies on various occasions, and under this mode of treatment had always suffered severely from rigors and febrile symptoms, without experiencing any benefit of longer duration than the period of dilatation. Latterly his symptoms had become more aggravated, and every attempt to pass instruments had proved ineffectual. A full-sized instrument was arrested at the bulb, and, not without considerable difficulty, a No. 1 bougie was introduced into the bladder. The introduction of this instrument was followed by a severe rigor, and next day the patient suffered from retention of urine, to relieve which a No. 1 catheter was introduced. Dr. Mackenzie made repeated attempts to continue the treatment by bougies, but the constitutional disturbance produced by them was so severe, that he determined without further delay to divide the stricture. Accordingly having introduced a grooved staff, the size of a No. 1 bougie, into the bladder, he made an incision in the middle line of the perineum, over the induration at the seat of stricture, and divided the contracted portion of the urethra to the extent of about three-quarters of an inch. A No. 8 catheter was passed with great facility into the bladder, and retained there, not more than half an ounce of blood being lost either during or after the operation. There was subsequently a slight degree of febrile disturbance, which subsided when the catheter was removed, at the end of

forty-eight hours. During the following ten days, the urine passed in nearly equal proportions by the urethra and the wound, but before the end of three weeks had entirely resumed the natural channel. He remained in the hospital a few weeks longer, during which a full-sized bougie was passed occasionally without the slighest difficulty, and was dismissed on the 12th of June. He was seen again on the 9th of June, 1853, when a No. 12 bougie was passed with ease; and the patient stated that, although he had been much exposed to cold and wet in deep-sea fishing, he had never experienced the slightest difficulty of micturition. In April, 1854, he applied for admission to the hospital on account of symptoms supposed to be rheumatic, but which were traced to aneurism of the innominata. He suffered from no urinary symptoms whatever, but was admitted on account of the aneurism, and died in the beginning of June.

The symphysis pubis, bladder, and urethra, are now before you, and the symphysis has been divided, so as to allow the urethra and neck of the bladder to be laid open from above. A white depressed line of cicatrix, on the inner surface of the urethra, extending, in the mesial line, half an inch forward from the extremity of the bulb, corresponds exactly with the line of cicatrix in the integuments and intermediate textures. The urethra in this part is rather wider than natural, from having a slight funnel-shaped depression on the lower surface, and before being opened, easily admitted a No. 12 bougie. In other respects the coats of the urethra present a perfectly natural aspect, and the spongy tissue of the bulb, as well as the neighbouring parts, are free from induration. There are two false passages in the prostate, evidently of very old standing. They are both seated at the upper surface of the urethra, one of them commencing at the triangular ligament, and forming a cul-de-sac in the prostate; the other, confined to the gland. The bladder, with the exception of being a little thicker than natural, presents nothing remarkable.

No one, looking at the urethra, which has remained thus sound and ample at the part where it had been so long and so tightly contracted, although two years had elapsed since the operation, without any means being used to prevent relapse, can entertain a reasonable doubt as to the recovery proving permanent, however long the patient might have lived; and such a result should stimulate our exertions to discover, so that they may be avoided, the errors of performance to which any recurrence of the symptoms that may have happened should be attributed, rather than to the principle of the operation.

But the parts now before you afford a no less instructive lesson in regard to another most important point in the treatment of stricture. This is, the position of the disease, respecting which the most vague and conflicting opinions still exist in the profession, as you may judge from one of the most esteemed authorities, Sir B. Brodie, representing the membranous portion of the urethra as the most common seat of contraction, while I am prepared to prove that it never occurs in this part of the canal. The erroneous opinions which exist on the subject have proceeded from two sources. Of these, the first is the difficulty of discriminating between the resistance to passing bougies into the bladder which is caused by the contracted part of the canal grasping the instrument that has been carried through it, from an obstruction to the progress of the point; while the second is the deceitful feeling of a false passage, which allows the introduction, without much force, so far as the surrounding textures are soft and yielding, but cannot be extended beyond any firm texture, or still less permit an entrance into the bladder. The truth is, that if you divide the urethra into three portions, of prostatic, membranous, and the remaining part, which is covered by corpus spongiosum, it is to the last alone that all strictures may be referred. However completely any one might be satisfied in his own mind as to this, he would probably find it impossible to convey the same persuasion to others, so long as he merely appealed to the practicability of passing instruments through the strictures alleged to exist in the posterior part of the canal, because success in doing so might be attributed to chance or good fortune on the part of the operator. But having now had occasion to operate more than a hundred times upon strictures of every sort and degree, by cutting externally upon a grooved director, so that the precise seat and extent of the incision could be determined, and having never been prevented from passing a full-sized instrument into the bladder, although I have never cut beyond the bulbous part of the urethra, I am entitled to conclude, and without any qualification to affirm, that the fact is as has just been stated. Now, if

you look at the prostatic portion of this urethra, you will see that various false passages have been made into its substance, so as to establish a cul-de-sac, which must plainly have resulted from the pressure of an instrument; and there can be as little doubt, that when the operators found their progress obstructed here, they believed that they had to deal with an impermeable stricture at the neck of the bladder. But you see that Dr. Mackenzie did not cut further back than the bulb, that he was nevertheless able to pass, without difficulty, a full-sized catheter, into the bladder, and that the patient was completely relieved from his symptoms. There is at present under my charge a gentleman who has come from England, having been from home nearly two years, under the treatment of an eminent practitioner, who, he says, introduced instruments, with few exceptions, every day, for the remedy of stricture which, he said, was seated at the verumontanum. You will now be able to understand the explanation of such a case.

This operation for stricture has passed through the period of opposition, and is now exposed to the danger of being adopted by persons who certainly do not, and either cannot or will not, understand the principles upon which it is founded. These principles may be said to be—

First.—Stricture is never seated posteriorly to the bulb, and therefore the incision should never extend further back than the bulb.

Second.—A grooved director must be insinuated through the contracted part, without injury or abrasion of the lining membrane of the urethra.

Third.—If the incision has been properly performed, there is no need to dilate sinuses, which are sure to close when the stricture is removed.

Fourth.—A catheter should be introduced into the bladder after the operation, and retained for forty-eight hours; not less, on account of the risk of extravasation of urine; and not longer, because it is unnecessary, and apt to do harm.

You may fancy with what astonishment I read in the journals from week to week statements to the effect that strictures are very often seated in the membranous part of the urethra, and that incisions are therefore required there, or even at the neck of the bladder. I also find it stated that in cases of impermeable stricture the bougie must be pushed onwards by force in the direction which the anatomical knowledge of the surgeon leads him to suppose to be the course of the urethra. In a journal published the other day, a case is recorded, in which the operator is said to have divided a stricture upon a silver catheter. Consider for one moment the danger of such a proceeding; even if the urethra were healthy there would be great danger of wounding the artery of the bulb if the knife should glide off the catheter; but considering the induration of the parts divided, and the uncertainty of the precise seat of contraction, I do not hesitate to assert that it is impossible to divide a stricture with certainty or safety in this way. For my own part, I would as soon think of dancing on a tight rope as of making such an attempt. It is further reported that in this case a number of sinuses were laid open, and that a catheter was retained in the bladder for eleven days, and yet it is said by the reporter that this operation was conducted emphatically after my manner. I say emphatically that it was not conducted after my manner, but was a miserable travesty of it, which would be simply ludicrous were it not that its result was so tragical.

[The patient having been brought into the theatre, and chloroform administered, Mr. Syme passed a No. 2 bougie into the bladder, and afterwards introduced a No. 9 bougie for a short distance into the urethra, to make sure that a stricture near the orifice had been dilated sufficiently to admit the thick part of his new staff. This is a steel instrument of the same general form as a catheter of short curve, with the curved part of the size of a No. 1 or 2 bougie, and deeply grooved on the covexity. The rest of the instrument up to the handle is of the size of the No. 9 bougie, and destitute of a groove. The groove in the thin part terminates anteriorly in a notch in the abrupt commencement of the thick part. Having introduced this instrument through the stricture, he made an incision, about two inches long, in the raphé of the perineum, and cut carefully down in the middle line, through the mass of indurated textures, till he felt the staff with the forefinger of his left hand. Taking the staff into his own hand, he introduced the point of the knife into the groove, about an inch and a quarter behind the commencement of the thick part, holding it with the handle upwards, and the back supported in its entire length on the forefinger, which he was thus able to use to feel where the knife was going, while it removed

all risk of the point of the instrument slipping deeply on either side of the staff. Having depressed the handle, he then pushed the knife forwards along the groove, through the tough substance of the stricture, till the point was arrested by the notch at the commencement of the thick part of the staff. To ensure the complete division of the anterior part of the stricture, he now pushed forward the staff and knife together for a short distance. He then introduced into the bladder a No. 11 catheter, placing the left index finger in the wound as a guide. This was necessary on account of the great depth of the incision through the indurated perineum, which measured full two inches. The catheter was provided with a stop-cock, and the end curved downwards to prevent the inconvenience of the trickling of urine upon the body. Everything went on favourably, the urine being discharged from the wound entirely for a week, and then gradually resuming its natural course, so that a fortnight after the operation, a few drops only escaped by the wound, the swelling of the perineum had entirely disappeared, he no longer suffered from incontinence, and passed his urine in a good stream. The operation was performed on the 27th of November, and the patient was discharged fit for duty on the 16th of December.]—*Lancet, Jan.* 13, 1855, *p.* 27.

[Mr. Syme brings forward the following interesting case, on which he makes some very important remarks. The patient was aged 37, a seaman, treated by dilatation eight years ago.]

At present, micturition is very laborious and frequent, sometimes hourly, and the stream of urine extremely slender. On examination, I found a bougie of moderate size arrested, an inch and a half from the orifice, by an obstruction, which could not be passed by the smallest bougie; I then tried a common probe, but this also could not be passed. I now had recourse to the instrument which I hold in my hand, which was made for cases of this description. You see it is considerably finer than a common probe at its extremity, and gradually increases in thickness towards the handle. This instrument passed without the slightest difficulty. If the stricture had been seated at the bulb, it might have been said that the fact of my being able to pass only so small an instrument was no certain evidence of an extreme degree of contraction, as there is difficulty in guiding the instrument with precision at that part of the canal; here, however, there is no such source of fallacy, for we can direct the instrument with certainty, in the course of the urethra, and, small as it is, it is firmly grasped. The first remark I would make regarding this case has reference to what is called impermeability of strictures. Now, the No. 1 of our set of bougies is the smallest size that is made, while many sets have none so small; so that this stricture would be impermeable to all ordinary bougies. On the other hand, the very small instrument I have just shown you, passed without the slightest difficulty; and this case, therefore, furnishes a good example of the principle that all strictures that allow urine to pass out, will admit of an instrument, sufficiently small, being insinuated through them. The next point to which I would direct your attention, is the situation of this stricture, anterior to the scrotum. It is here that the tightest strictures have been observed to occur, which is fortunate, for if there should be a stricture of the same degree of tightness behind the scrotum, its remedy would be attended with extreme difficulty. In the present case I feel the induration of the stricture like a small pea, somewhat elongated. Now, experience shows that of all strictures of the urethra, this condition is the most unmanageable by bougies. The patient tells us that so far from having experienced benefit from the introduction of instruments, his symptoms have rather become more aggravated, and I believe we might continue dilatation for any length of time without advantage. Instead, therefore, of pursuing this unsatisfactory course, I propose to divide the stricture at once. But you will say, How can it be divided? We must use a proportionately small director, the employment of which would be attended with great difficulty at the bulb, but will be very easy here. In this part of the canal there is no occasion for a long incision in the skin; the point of the knife being entered into the groove of the director, anterior to the stricture, will be pushed backwards through it, while the end of the penis is held firmly; the same principle being followed in cutting backwards here, as in cutting forwards for a stricture at the bulb—viz., that of proceeding from the side where there is greatest resistance. After this I shall pass a full-sized instrument, and I expect it will go freely into the bladder. Of this we cannot be certill the stricture at the anterior part of the canal has been divided; but, as far as

I remember, I never met with a tight stricture behind the scrotum along with an extreme degree of contraction anterior to it. There will be no need to retain a catheter in the bladder; the urine will flow freely, and there will be no risk of extravasation; even if it did occur to the small extent which is alone possible, I should regard it as a favourable circumstance, on account of the effect it would have in preventing the urethra from uniting by first intention.

[The patient was now brought into the theatre, and Mr. Syme, having shown that the No. 1 bougie was arrested an inch and a half from the orifice, while the delicate instrument above described passed the obstruction without difficulty, endeavoured to introduce a very fine grooved steel director. This, however, could not be passed beyond a certain distance; but Mr. Syme, on feeling the instrument through the tissues of the penis, found that the point of the director had passed the stricture, which he felt like a pea upon it, and that it was only in consequence of the instrument being tightly grasped that it refused to pass further. He, therefore, proceeded to operate as he had proposed, after which he introduced successively three different sized bougies through the seat of stricture, and pushed on the largest (No. 8) with ease into the bladder, thus proving that no obstruction existed behind the one divided. Scarcely a drop of blood escaped during the operation. Mr. Syme then made the following remarks:—]

This, then, is, as you have seen, a very simple process; yet the patient may be almost said to be entirely cured. I think it likely that, instead of leaving the hospital very imperfectly relieved, after several months of dilatation, he will in a few days go away, without requiring any further precaution than the occasional introduction by himself of a steel bougie, two inches in length, to prevent the risk of contraction during the healing process. You will, I trust, not soon forget that remarkable fact which you have just witnessed—viz., that, after I had introduced the very small director fairly through the stricture, the firm grasp of the contracted part made it impossible to push it further without a degree of force, which I did not feel justified in using.

[The operation was performed on the 15th of January. On the night after the operation the patient retained his urine for seven hours, and never afterwards voided it with unnatural frequency. It flowed in a full stream by the urethra, but for a few days issued also to a small extent by the wound, in the neighbourhood of which the cellular substance showed some signs of irritation by swelling and tenderness. Full-sized bougies were occasionally introduced, and he left the hospital to join his ship on the 6th of February.

In his lecture on the 25th of January Mr. Syme said:—]

There are two patients who have been in the hospital for the last seven weeks, with stricture of the urethra at the bulb, as tight as you ever met with in that situation. It was at first difficult to say which of these cases was the worse; both had extreme difficulty of micturition, and both incontinence of urine, and both required, in the first instance, the smallest-sized bougies, which I experienced some difficulty in introducing. By passing instruments regularly at intervals of three or four days, I was soon able to introduce a large sized bougie in both cases, but neither appeared at first to improve in their symptoms under the dilatation. By and by, however, one of them did derive benefit; he lost the incontinence, and could pass his urine in a much better stream—in short, he is now so much improved that I think in his case we might be satisfied with dilatation. You observe I express myself with some hesitation; for the more I see of these complaints in this obstinate form, the more doubt I entertain as to the expediency of trusting to dilatation for their cure. The other patient, however, has not experienced the relief that we desired; his difficulty of micturition is almost as great as ever, and his clothes are still wet both day and night from incontinence of urine. I also found after leaving him for a few days longer than usual without the introduction of instruments, that, instead of going on with the large-sized bougie I had used on the last occasion, I was obliged to begin again with No. 1. On my next examination of the urethra, ten days later, I was unable to pass even the smallest size. In this case, therefore, if we had no means except dilatation, we should be puzzled and at a loss what to do next; but I need hardly say that we shall feel no such perplexity, but shall proceed to the other mode of relief for stricture—viz., dividing it by external incision. But before having the patient brought in, I will mention to you a striking case, at present

under my care in private. During my short residence in London this gentleman came up from Aberdeen, and placed himself under my care, in order to obtain relief from a stricture, which had been treated for some time as impermeable by several surgeons. I at once succeeded in introducing a small instrument, and, after employing dilatation for three weeks, the urethra admitted a full-sized bougie, and he went away apparently well, and with the hope of continuing free from his symptoms, with the occasional introduction of an instrument. Instead of this, however, he has ever since—that is to say, for the last seven years—been perpetually under the care of surgeons in different parts of Great Britain. On one occasion he had complete retention, and several surgeons then failed to pass a catheter. But the remarkable circumstance is, that he has been during this period repeatedly treated by dilatation to the full extent. And now comes the most important point of all:—this patient, though in full general health, not above forty years of age, married, and leading a regular life, and therefore not in danger of contracting the disease afresh, yet, even when the stricture was fully dilated, has never passed urine with ease; he has even felt an insecurity of being able to void it at all, and has been frequently obliged to go out of church or to leave company in consequence of the importunate desire for micturition which has accompanied his complaint. His life has, in short, been miserable, and this, you observe, at times when bougies of full size could be passed into the bladder. This is an example of that form of the disease which first led me to aim at once at some other means of effecting that for which dilatation was evidently inadequate. These cases were formerly the opprobria of surgery. It is only three days since I divided the stricture in this gentleman, yet he told me yesterday (I have not seen him to-day) that he felt as if something was taken away that had made life miserable; an incubus had been removed that had been weighing him down during many long years of suffering. (The relief in this case proved permanent.)

[The patient was now brought into the theatre, and chloroform having been administered, Mr. Syme succeeded, after some little difficulty, in passing the thin part of his staff through the stricture. The rest of the operation was easily completed in the usual way. Mr. Syme, while he had his finger in the wound, observed that he could feel the artery of the bulb beating at the side, and remarked that this showed forcibly the danger of deviating from the middle line in making the deep part of the incision. Mr. Syme then easily introduced into the bladder a No. 11 catheter provided with a stop-cock. The patient having been removed, Mr. Syme made the following remarks:—]

You had, not long ago, an opportunity of seeing the operation performed in a case complicated with fistulas, and great thickening and induration of the perineum; those are the cases in which the operation presents the greatest difficulty. Again, ten days ago, you saw me divide a stricture anterior to the scrotum, in which the operation presents the greatest simplicity, and is easiest of performance. That patient, I may remark, continues well, passing his urine without pain in a good stream, and the urethra admitting a full-sized instrument. The operation you have just seen holds a middle place as regards difficulty of the operation, the stricture being seated as far back as it is ever met with, but the perineum free from induration. You see here on the table the blood that was lost during the operation; though he is a young man of thirty, in full health, and with rather greater disposition to bleed than is usual, yet two teaspoonfuls would be a liberal allowance for the amount of the hemorrhage.

[Everything afterwards went on favourably; on the removal of the catheter at the end of forty-eight hours, the urine passed in a perfectly full stream by the urethra, and continued to do so, none having escaped by the wound. In a few days he was going about the ward, and might then have been dismissed, but remained under observation until the 27th of February, nearly five weeks after the operation, when he got his discharge after a full-sized bougie had been passed with perfect facility into the bladder.]—*Lancet, March* 10, 1855, *p.* 254.

80.—STRICTURE OF THE URETHRA OF TWENTY YEARS' STANDING.

By Henry Lee, Esq., King's College Hospital.

This patient is a hair weaver, about forty years of age, of a thin, sickly look, and observed the first symptom of stricture twenty years before his present admission. At that period retention of urine suddenly came on, when a personal friend of the patient, seeing the state he was in, shaped a piece of *whalebone from an umbrella* into the form of a bougie, and passed it up the urethra. Immediately after the operation, the urine flowed copiously.

The patient now remained well for three or four years, when symptoms of stricture again presented themselves, but not to such a degree as to cause him much inconvenience, the use of the catheter being, however, now and then required.

Four eears before his admission here, the distress from the stricture became suddenly so great as to induce Mr. Guthrie, under whose care he then was, to divide the stricture with Stafford's instrument. After this operation, the patient passed his urine freely, for a short time, but soon the stricture returned, and he has been suffering from it more or less up to his admission. When first seen in the hospital, Sept. 14th, 1854, there was pain in the lower part of the abdomen, and across the loins, and the bladder could be distinctly felt, through the parietes of the abdomen, to be hard and full. There was total inability to pass urine, which fluid only came in drops. The endeavours to introduce a catheter, both without and within the hospital, having proved useless, Mr. Lee had the patient brought into the theatre, passed down an instrument as far as the obstacle, and with no better guide than the end of the catheter, freely divided the urethra. When this perineal section was accomplished, No. 6 catheter was introduced into the bladder, and there retained for five days.

The urine a few days afterwards passed both through the wound and the urethra, and the patient after remaining several weeks in the hospital is on the point of being discharged. The wound is all but healed, and scarcely any urine escapes through the perineum.

Mr. Lee, in a clinical lecture on this case, after giving a sketch of its principal features, and having pointed out the different kinds of unpleasant consequences following stricture, and the various methods which have been devised for the cure of contraction, said—

The plan of dividing stricture by instruments passed down the urethra has been variously modified; sometimes an instrument of the shape of a trocar has been made to protrude from the end of a catheter, so as to perforate the stricture; sometimes the instrument used has been made in the shape of a lancet, and sometimes a long very thin knife has been passed down to the stricture upon a director. Another plan has lately been adopted in France where an instrument could be introduced into the bladder. The instrument used resembles in construction a lithotrite, but the blades are capable of being separated from each other to the extent of about a quarter of an inch. The instrument is then introduced, and the blades suddenly separated, so as forcibly to tear open the stricture.

Now all these plans have the disadvantage, besides that of endangering the vitality of a portion of the mucous membrane of the canal, of not providing an escape for any portion of urine which may flow from the urethra through the opening which they make. Under these circumstances it has been proposed to make an incision in the perineum, and to divide the stricture from without. This plan, of which I now propose to speak, has at least the advantage, when properly performed, of being free from the dangers arising from making false passages, extravasation of urine, and purulent deposits.

The cases for which this operation has been recommended are of three kinds:—

1. Where the stricture presents an extreme degree of irritability, and resents, by violent local and constitutional disturbance, any efforts to produce dilatation.
2. Cases in which the stricture, when dilated, rapidly contracts again.
3. Cases in which, after the dilatation of the passage, micturition is nevertheless painful, difficult, and uncertain.

To these three classes we may add a fourth, not admitted by some surgeons—namely, cases in which no instrument can be passed into the bladder.

For these affections a free incision of the contracted part of the urethra has been maintained to be the proper mode of treatment, and to be in fact required.

The mode of performing this operation, as described by Mr. Syme, is as follows:

A grooved director is first introduced through the stricture, where this can be done, (and Mr. Syme is of opinion that, with care and attention, there is no stricture through which an instrument may not be made to pass.) The patient then being placed upon his back, at the edge of the table, with his legs bent, as in the operation for lithotomy, an incision, about one inch and a half in length, is made exactly in the raphé of the perineum. The whole of the thickened, indurated, and contracted texture is then divided upon the director, to the extent of an inch or two, or more if necessary. A No. 8 silver catheter is then passed into the bladder, and allowed to remain there for at least two, and not more than three days.

Great stress is laid upon the fact of the incision being made exactly in the middle line of the perineum, in order to avoid the artery of the bulb which lies upon the side of the canal.

The only sources of danger alleged to exist are, hemorrhage and extravasation of urine. But if the knife is kept exactly in the median line, the only vessels that are likely to bleed are, the smallest branches of the superficial perineal artery, and the cells of the corpus spongiosum; the bleeding from these may be checked, should it be desirable, by placing a piece of folded lint between the edges of the wound, and applying the slightest degree of pressure for a few hours.

The liability to extravasation of urine after this operation, I will presently consider, reserving it for separate remarks, as I conceive the liability or otherwise to its occurrence forms the grand distinction as to this operation being admissible or not.

After the operation of perineal section, the catheter is tied in the bladder, and the patient put to bed. At the end of 48 hours the catheter may be removed. A full sized bougie should be introduced once a week for three or four weeks, and then at more distant intervals, according to circumstances.

Now this operation, as I have described it, appears a very simple affair, and if this were all, there is little doubt which is to be preferred, the pain and inconvenience (to say nothing of the danger of a stricture) or the simple operation of dividing a portion of the urethra on a grooved director. But, unfortunately, the operation has not been found to be one of such a very simple nature in its consequences, even where an instrument could be got into the bladder, much less in cases where no staff could be passed.

Mr. Syme, of Edinburgh, asserts that he has performed this operation a great number of times, without any ill effects, excepting only some consequent symptoms of nervous irritation. But in other hands the most serious mischief has often supervened. Patients have been attacked with a shivering fit the day after the operation; there has been a quick, irritable condition of the pulse, accompanied perhaps by profuse perspiration, and want of sleep. These symptoms have continued, the patient's tongue becoming brown and coated, and, in a certain number of cases, the patient has died. On a post-mortem examination, some purulent infiltration has generally been found about the neck of the bladder, or some secondary inflammation in other parts.

Now how are we to account for such different results in the practice of various surgeons of equal skill? I believe that something like a solution may be arrived at by an attentive consideration of the description which Mr. Syme has given of this operation.

In the way in which he performs it, the only fascia concerned, as he says, is that which lies immediately under the integuments. In other words, he divides only the skin, superficial fascia, and the urethra. Now it is clear from this description, that all the strictures which he has operated upon, have been situated anterior to the membranous portion of the urethra; for had he operated upon any stricture in the membranous portion of the canal, even though situated quite at its anterior part, he must have been in danger of wounding the deep perineal fascia as well as the superficial. When a stricture is situated in the bulb of the urethra, it may be divided from without, and any urine which escapes from the passage is sure to pass out at the external wound. But the circumstances are different when the knife, in passing along the grooved director or sound, perforates the deep perineal fascia, and

wounds the urethra as it passes through this part. The urine which escapes from the passage may then lodge in the wound made in the deep perineal fascia, and a drop or two may become infiltrated behind this dense structure. It will there give rise to inflammation, and having no means of escape, will produce violent constitutional irritation. When once inflammation is established in the cellular tissue of this part, its products will permeate the areolar tissue, and may thus propagate the inflammation to the outside of the bladder, and to the cellular tissue within the pelvis, thus giving rise to the abscesses and the purulent infiltration which I have mentioned.

But where the incision is confined to the superficial fascia, and to the bulb of the urethra, there is, as I have said, very little danger of any of these accidents occurring; and by choosing his cases, (as it were,) and confining himself to those in which the stricture is situated anterior to the membranous part of the urethra, Mr. Syme has met with the success to which I have alluded.

Hence there appears to be two classes of cases: those in which the stricture is situated in that part of the urethra corresponding to the corpus spongiosum, and those in which the stricture is at the anterior part of the membranous portion of the urethra. In the former situation, as far as our present evidence goes, the stricture may be divided with comparative impunity; in the latter, most severe and even fatal symptoms have followed.

But practically, it will be asked, how are we to know that a stricture is confined to the bulb of the urethra, or to any part in front of this? Where an instrument can be introduced, the point may be made out in this way, and Mr. Syme never operates unless he is able first to introduce an instrument into the bladder.

In the case to which I have drawn your attention, I was led to believe that I might with impunity divide the stricture, from the circumstance of Mr. Guthrie having divided it by internal incision before, without any ill effects. This proved to me, either that the stricture was confined to the bulb of the urethra, or, if situated further back, that the surrounding parts were so consolidated by inflammation as to preclude the danger which might arise from infiltration of urine.—*Lancet, Nov.* 25, 1854, *p.* 436.

81.—*New Instruments for Retention of Urine and for Fistula.*—Some cases of retention of urine from obstinate stricture, requiring puncture through the rectum, have ended fatally, lately, in hospitals. The instrument unfortunately slipped out, but was obliged to be replaced. It has been suggested to try an instrument which would not slip out, and one of this description is now used at Guy's, a modification of Mr. Wakley's, which expands by a spring when once in the bladder.—Mr. Partridge lately operating for fistula, used a very ingenious probe-pointed bistoury, with "movable sheath," made of silver, which slides off to any extent when the instrument is introduced, thus offering a cutting edge of greater or less extent, according to circumstances.—*Lancet, Dec.* 23, 1854, *p.* 528.

82.—*Nocturnal Seminal Emissions treated by Digitaline.* By M. Laroche.—Digitaline, of late much extolled and employed with success, according to many practitioners, in cases of nocturnal seminal losses, has yielded, in the hands of Dr. Laroche, a result which deserves to be mentioned. A young man aged 18, presented himself some months ago to be treated for nocturnal seminal losses. The disease had come upon him about two months previously. For twenty days, the pollutions had not omitted a single night; his strength was gone, the appetite had left him, and his sleep was troubled by unpleasant dreams. M. Laroche prescribed three granules of digitaline (equal to a grain of the powder of the plant). The night following the administration of the medicine, the pollutions intermitted for the first time. In three weeks he was cured.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 473.

83.—*Incontinence of Urine in the old.* By JOHN Z. LAWRENCE, Esq., Surgeon to the Farringdon Dispensary.—[In a case of this description in an old man, owing probably to some want of power in the bladder, and not to diseased prostate, Mr. Lawrence found the following useful. On the case he first makes these remarks:]

He generally passes his water twice a day, once before retiring to rest, when he often felt a desire to micturate more than he was able. His stream of urine had always been a full one, but he had experienced latterly some pain in the small of the back whilst micturating, and his water had been rather turbid from a reddish sediment. No enlargement of the prostate could be felt by the rectum. What the incontinence depended on in this case was not very clear to me. Viewing it as being due to some loss of tonic contractility of the muscular fibres at the neck of the bladder, I prescribed the following mixture.

℞. Tinct. ferri sesquichloridi ʒii.; balsami copaibæ ʒi.; strychniæ gr. i.; infusi quassiæ ℥xii. M. Fiat mistura, cujus sumatur ℥i. ter die.

To my surprise, the next time I saw the man he informed me that after taking two doses of medicine the same day, he had no incontinence that night, and had not been troubled since. I certainly expected some effect from the remedies, but a cure so sudden and so permanent was as remarkable as it was gratifying—*Assoc. Journal, Nov.* 10, 1854, *p*, 1017.

84.—APPARATUS FOR INJECTING THE BLADDER.

In various morbid conditions of the bladder the employment of injections, either acid, alkaline, or simply diluent, has long been a favourite practice with many surgeons. An ingenious expedient for the effecting of that procedure was recently brought under our notice by Mr. Wormald, who was then having recourse to it in a case under his care in St. Bartholomew's Hospital. It consists in the employment of a short piece of India-rubber tubing as a means of connexion between the syringe and the catheter. The appended woodcut will best illustrate the mode. C represents the end of a catheter, B a portion of the elastic tube, and A the nozzle of a common ear-syringe. The tube being a little smaller in bore than the size of either of the two instruments to which it is adapted, fits tightly, and requires nothing to secure it in place beyond its own elastic grasp. The adjustment of the three is perfectly easy: the tube having been fitted to the end of the catheter prior to the introduction of the latter, the conical nozzle of the syringe is afterwards readily admitted into its other end, and passed so far as to be firmly held. The advantages of the expedient are the following:—1st. The medium of connexion being flexible, all shaking of the catheter during the fitting of the syringe is prevented. This is of great importance, for it must be remembered, that in almost all conditions requiring the use of injections the mucous membrane of the bladder is more or less inflamed, and can ill tolerate an additional irritation. When the whole instrument is metallic, it is almost impossible during the fitting and removal of the syringe, to avoid more or less of joggling the catheter. 2ndly. It obviates the necessity for a stop-cock, since, as the tube is compressible, the finger and thumb may very conveniently serve all the objects of that appliance. 3rdly. It will connect any sized catheter with any sort of syringe. It is always expedient for the surgeon to be as far as possible independent of the instrument-maker, and suggestions which enable him to dispense with complexity of mechanical aids are very valuable. In injecting the bladder there are ordinarily employed a syringe of peculiar construction, and a stop-cock with various nozzles of different sizes made to screw on, and graduated to fit catheters of different dimensions. By means, however, of a couple of inches of India-rubber tube all this complexity is obviated, and a common ear-

syringe, or any other that may be at hand may, *extempore*, be adapted to a catheter of any size or sort. The plan under consideration has been subjected to many practical trials, both by Mr. Wormald himself and by the writer of this notice, and has been found to fully answer its intended objects. As an exhausting apparatus it is of course useless; but, by inserting the tube within one of a size larger, so as to make its sides sufficiently rigid, it may even be made to fulfil this purpose also. Exhausting syringes for the bladder, are, however, but very rarely required.—*Med. Times and Gazette, Jan.* 6, 1855, *p.* 8.

85.—*Treatment of enlarged Testicle by Hutchinson's Apparatus.*—It is more than a year since the Clinique Allemande borrowed from the Wiener Medicin. Wochens. the description of Hutchinson's Air Compressor. This apparatus, which hitherto the English Journals (the Medical Times and Gazette excepted) have not thought fit to notice, consists of a double sac of impermeable stuff, folded on itself like a nightcap. An opening in front receives a short tube with a stop-cock, by which air can be admitted into the cavity. The testicle is put into the double sac, and the apparatus is fixed to the spermatic cord; the amount of air introduced between the two layers determines the degree of pressure. The organ is thus protected from direct violence; and any other medication may be simultaneously employed. Mr. Hutchinson considers pressure a most important agent in promoting absorption, and in controlling subacute and chronic inflammatory affections of the testicle. He thinks, also, that, with care, it may be the means of removing certain forms of hydrocele without the necessity of the use of the trocar.—*Annales Méd.*—*Medical Times and Gazette, Jan.* 27, 1855, *p.* 93.

DISEASES OF THE SKIN.

86.—GENERAL OBSERVATIONS ON CHRONIC DISEASES OF THE SKIN.

By Thomas Hunt, Esq., Surgeon to the Western Dispensary for Diseases of the Skin.

As these remarks will have a general bearing upon almost every kind of skin-disease, which is not specific in its origin, it will be convenient to introduce them in this place.

I. Local Treatment.—Perhaps no greater error in therapeutics is ever committed, than in supposing that cutaneous diseases of constitutional origin can be safely and permanently cured by topical applications. They may, indeed, by these means, be moderated, checked, relieved, and even dissipated; but, unless the application is absorbed, so as to effect a change in the circulating fluids, it is clear that, the source of the disease being untouched, it cannot be thus cured: and it is very doubtful if the cure can even be accelerated by local treatment. Relief may, however, be afforded by it, and sometimes to a great extent. Among these remedies may be enumerated:—

1. *The Tepid Bath.* This may be used daily, or less often. It acts first, by cleansing the skin, and removing extraneous sources of irritation; secondly, by cooling the surface, and thus arresting or assuaging inflammation; thirdly, by promoting insensible perspiration, which is often of general use to the system, as well as to the skin.

2. *Glycerine.* This fluid, when mingled with five or six times its bulk of elder-flower water, is very useful where the skin is harsh and dry, or covered with hard crusts and scales, or disposed to crack. One application of this lotion will soften the skin, and moisten it for many hours; for the glycerine does not sensibly evaporate like other fluids, and the skin becomes permanently lubricated. It is said to exercise a specific and curative action in lepra; but of this I have seen no proof.

3. *Creasote*, in a very diluted form, will sometimes allay irritability, and act as a charm.

4. The *Solutions of Metallic Salts*, such as of the bichloride of mercury and the

sulphates of zinc and copper, will occasionally be found useful for a time, when applied to the face, in acne or lichen, very much diluted with water.

5. The *German Poultice*, which consists of wet rags covered with oil-silk, is sometimes a source of great comfort in eczematous and scaly eruptions on the limbs.

6. *Ointments*, which generally do harm, are found useful in affections of the scalp, when the hair is not removed. They tend to separate the crusts. Those composed of calomel, white precipitate, and oxyde of zinc, or the nitrate of mercury, are the most useful. Tar ointment is in high repute; but it should only be used in hospital, dispensary, or parochial practice, and even then the remedy is often as annoying as the disease. These are all the applications I have found generally useful; but, as a general rule, it is better to apply nothing but what is necessary to cleanse the skin. Local treatment is apt to obscure the disease, or to drive it to other parts of the body, thus masking the progress of the improvement effected by the internal or general treatment, and leaving the practitioner ignorant of what is going on.

II. General Treatment.—The state of the general health is always the first thing to be inquired into. If that is obviously defective, it is useless to think about treating the cutaneous disease. About one-third of the patients suffering under chronic eruptions, complain that their health is not good. Constipation of the bowels, headache, and general dyspepsia, are very frequent complications. Suppression of the natural exhalation from the skin, of the urinary or uterine secretion, and of other natural or morbid discharges, are frequently associated with cutaneous disease. Occasionally all the secretions are more profuse than natural, and a state of general plethora exists. Sometimes an anæmic or strumous diathesis prevails. All these require appropriate treatment in the first instance; and, if this be successful in rectifying the general health, the skin commonly recovers its tone at the same time. If, however, it fails to do so, the case may be treated specifically. The medicines generally useful may be described as *purgatives*, *sudorifics*, *diuretics*, *tonics*, and *alteratives*, which may be given alone or combined, according to circumstances.

Purgatives are the most generally useful. Almost every eruption is more or less benefited by their action; and there are many cutaneous affections which probably arise from the absorption of fæculent matter accumulated in the bowels. But, in order to derive the full benefit from purgatives, they should be administered for several weeks together; and the materials of which they are composed cannot be too numerous, or too frequently changed. The saline class are the most important; and, although they seldom suffice to cure the disease, they render the cure by alteratives much more easy and rapid. In these days of asthenic disease, when depletion is too generally condemned *in toto*, we are apt to lose sight of the invaluable advantage of clearing the bowels daily. If the diet is not stinted, debility seldom results; and it often happens that, by unloading the bowels, and thus enabling the function of defecation to be more easily performed, they act in effect as tonics, improving the appetite and spirits, and actually adding to the strength. When the patient is weak, great advantage is found in combining the sulphate of magnesia (for example) with the sulphate of iron, the disulphate of quina, and dilute sulphuric acid. This often proves the best treatment of inveterate eczema in asthenic subjects.

Sudorifics are useful when the skin is dry and harsh, and the extremities cold. The warm bath, the liquor ammoniæ acetatis, and the potassio-tartrate of antimony are the most valuable. Dover's powders are sometimes useful in allaying extreme irritation.

Diuretics are not only useful when the blood is loaded with urates, but, when the urine is scanty in leucophlegmatic subjects, they are often essential to the cure. The acetate of potash and spirit of nitric æther in full doses, with or without squills and colchicum, are very efficacious. If they fail, saline purgatives may be substituted.

Tonics are seldom required, except by the weak and ill-nourished class of patients. In dispensary practice, I have found the bebeerine an excellent substitute for quinine, in doses equal to those of the latter drug; and it can be obtained at about half the price. In emaciated or strumous subjects, Dr. de Jongh's light brown cod-liver oil is highly useful. This oil, though somewhat more expensive, goes three times as far as any other I have tried. One drachm is equal to half an ounce of the pale

oil; and children will generally take it without objection. In some cases of lupus it acts as a specific. The iodide of iron is also useful in these cases.

Alteratives are either primarily or ultimately essential to the successful treatment of a large majority of cutaneous cases. It is generally necessary in idiopathic cases to use some preparation of arsenic; for in most of them *all other alteratives fail.* The most manageable preparation is Dr. de Valengen's "solution of solvent mineral," now introduced into the Pharmacopœia under the cognomen "liquor arsenici chloridii." The dose is ten or twelve minims thrice a day, for all ages above two years. This dose is equivalent to five minims of Fowler's solution. This should be borne in mind, as its chemical composition has betrayed writers on pharmacy into the error of representing it as equal in strength to the liquor potassio-arsenitis. It should be given always immediately after a meal, and the dose slightly reduced if the eyes become irritated, or the eye-lids puffy. Patients will take it for many months without injury. If it fail, or disagree, the solution of arsenite of potass may be substituted for it; but it is far better that, in preparing this solution, the compound spirit of lavender should be left out. It is then tasteless and clear, and will keep without becoming musty or decomposed, at least ten times as long. The dose is five minims for both adults and children. Children require, except in early infancy, as large a dose of arsenic as adults, and often larger. But some children are very susceptible of its toxical effects; and it is safer to begin with a smaller dose, which may be increased at the end of a week, and afterwards decreased, if necessary.

The signs of an over-dose of arsenic vary a good deal in different individuals; and yet they are comprised within a very few palpable symptoms, the most common of which is a horizontal streak of red injection in the membrane lining the lower eyelid: this is soon followed by a slight itching or pricking in the eyelids, an increased secretion, and a puffy appearance in the lids and upper part of the cheek, as though the patient had been weeping. If the medicine is now pushed in full doses, or in increasing doses (as was formerly the practice), the tarsi become affected by pustular irritation at the roots of the eyelashes, which, if unheeded, may terminate, as I have once seen, in chronic induration of the tarsi.

The next most usual sign of an over-dose is irritation in the mucous membrane of the nostrils and fauces, with slight tickling cough, and a sense of fulness or stuffiness of the throat, simulating a common catarrh; but this symptom is so rare in summer, that I have seldom found it necessary to pay much regard to it. Still it is certain that arsenic will aggravate a severe catarrh.

Irritation of the bowels I have found to be a very rare result of arsenical treatment, provided the medicine is not given on an empty stomach. Still more rare is irritation of the urethra; but I have seen, in two cases only, a slight gleet, and in one a purulent gonorrhœa, produced by arsenic, in each case recovering as soon as the medicine was withheld. Now and then, tenderness, and vesication of the soles of the feet occur, but not until after a lengthened course of arsenic; and, in fair individuals, a very delicate desquamation of the skin in the parts covered by the clothes, giving a dirt-brown appearance, is by no means uncommon. This gradually disappears on the termination of the course, leaving the skin more delicate, fair, and smooth, than before its commencement. Some individuals (about one in two hundred) are unusually susceptible of the toxical effects of arsenic. The face swells; the voice is husky and thick; the breathing is short; a sense of sickness, oppression, and fulness, at the precordia, is felt; and the nervous system is prostrated. These symptoms I have once seen produced by half a drachm of Fowler's solution, in divided doses. Some of these patients will bear one minim for a dose very well, and the skin will recover under it; but it is more prudent, in other cases, to abandon the medicine altogether. In some patients, arsenic affects the nervous system at first, producing general depression, tremor, and insomnia; but these symptoms are often only temporary, whereas the vascular disturbance is apt to increase by perseverance with the remedy. In others, the appetite fails; but this seldom continues long.

I have now exhibited arsenic in several thousands of cases, and have kept a minute record of almost every case; and the above are the only toxical symptoms I have noticed which could be reasonably attributed to the medicine in the cautious method of administration above described. I have generally found it perfectly manageable;

and, when prescribed under favourable indications, generally useful. In very few cases have I been obliged to abandon it.—*Assoc. Journal, Jan.* 5, 1855, *p.* 13.

87.—USE OF AN EXTRACT AND DECOCTION OF THE COMMON STINGING NETTLE IN SOME CHRONIC SKIN DISEASES.

By Dr. J. Bullar, Physician to the Royal South Hants Infirmary.

[The following cases of Dr. Bullar's are interesting. The medicine seems to have been chiefly useful when the general system of the patient was *cachectic.*]

Case 1.—A young lady, aged 14, of strumous habit, had been liable, for some years before I saw her, to occasional eruptions of psoriasis diffusa in large patches over the flexures of the knee and elbow-joints, attended with considerable redness, irritation, and heat. Twice (in Dec. 1849, and June 1850,) it was removed by liquor arsenicalis prescribed by myself. The arsenic gave her headache; so that she was averse to taking it. The eruption returned a third time after an interval of three or four months (Nov. 1850); patches of psoriasis also appeared on her forehead, neck, and chest, and resisted the liquor arsenicalis although given for several weeks. I then ordered a quarter of a pint daily of the decoction of the urtica dioica, made by boiling an ounce of the leaves and stems in a quart of water until it became a pint. She rapidly improved, and after taking five pints was completely cured, and has had no return. The only effect she perceived was an improvement of her appetite.

Case 2.—Mary C., aged 30, pale and cachectic, with habitually costive bowels, had a dry, chronic, scaly eruption surrounding the ankle-joint and lower part of the right leg, of three years' standing. The same disease had affected the whole leg; the upper part had been cured, but the lower remained stationary, and for the last twelve months she had been subjected to no treatment. It was like the remains of chronic eczema. She was ordered to take a quarter of a pint of the decoction of nettle, and she returned in a month quite cured, having felt no other effect from the medicine.

Case 3.—Edward H., a delicate boy, aged 4, had an inveterate eruption of lichen agrius covering nearly the whole of the back, chest, and abdomen, of two years' standing. The edges alone showed its papular nature, the rest, from inflammation and scratching, was covered either with dry scabs or with thin scales, thickening and hardening the skin itself. I recommended that he should take half a pint of the decoction of nettle as his common drink, and he soon liked it. To my surprise, in five or six weeks the whole eruption was removed, and his skin was clean, smooth, and natural, and not even marked. In this case, the cure of the skin disease seemed to have been too rapid, as he was soon brought to me again complaining of pain in his loins, and looking pale and ill: his bowels were costive. These symptoms were relieved by mild aperients and alkalies. Subsequently there was a slight return of the eruption, requiring a return to the decoction. During its use, he sometimes complained of pain in making water.

The rapidity of the amendment in this case of a chronic eruption, affecting almost the whole surface of the trunk, in a cachectic child, which had lasted two years, and was completely removed in five or six weeks, gave me a strong impression of the power of the remedy.

Case 4.—Edward W., aged 8, a delicate strumous child, of an unhealthy mother, had had for six months a patch of chronic eczema covering both eyelids, the eyebrow, and part of the cheek of the left side. It was covered with a dry scab. He was out of health, languid, and complained of headache. Constitutional treatment, with calomel ointment and subsequently pitch ointment, were of little service. He was then ordered a quarter of a pint of the decoction of fresh nettles daily. In a month the eczema was well. He was much better in health, having lost the languor and headache. The ointment of nitric oxide of mercury was used locally.

The preceding cases are selected to prove the beneficial action of the decoction of the nettle; the following cases show that the extract has similar virtues.

Case 5.—J. S., aged 27, a strong man, of fair complexion and light hair, and of healthy appearance, had patches of chronic lepra covering the elbows and knees, and extending above and below them for a considerable distance. The diseased surface was covered with thick scales like white mortar, entirely obscuring the red

surface beneath them. The eruption had existed twenty-four years. His general health was excellent. He had never taken medicine to remove it; but, as he was now confined to his bed in the South Hants Infirmary with a broken leg, he was willing to have the disease removed, and was a very fit case to try the remedy. He commenced with five grains of the extract three times a day. After three or four days, there was slight improvement; after sixteen days, when he had taken half an ounce of the extract, the improvement was striking, as the thick white scales covering the affected parts were removed, and the parts beneath were almost as smooth as natural. During the prevalence of dry easterly winds the disease was stationary, and the dose of extract was increased to twenty grains daily; and, five weeks after he had commenced, he left the infirmary quite well, with only the marks on the skin where the eruption had been. He was conscious of no perceptible action of the medicine; but his breath had a smell of herbs.

Case 6.—W. L, aged 22, a young man with light hair, fair skin, blue eyes, and otherwise healthy aspect, had been covered with lepra vulgaris for nine months. His body, legs, and arms, were closely spotted over with circular patches; and the forehead and hairy scalp were covered with a more diffused scaly eruption. He had been three months under the care of a physician in a neighbouring town, of much practical skill, who had fairly tried the liquor arsenicalis, but without any improvement. After taking five grains of the extract of urtica dioica three times a day for a week, the eruption itched more, but the scales were less numerous; and, on persevering for a month, the forehead and scalp were quite well, and the eruption on the body was fast dying away; the scales diminishing, and the surface beneath becoming paler. At the end of six weeks he reported himself as quite well, and I found the marks alone remaining.

Case 7.—M. N., aged 40, a stout woman, who was nursing a child three months old, was admitted into the South Hants Infirmary, with inveterate eczema impetiginodes of twelve years' standing. The eruption completely covered both hands and the forearms; and there were large patches on the arms, shoulders, chest, neck, and face, disfiguring her, and much interfering with her work, as her hands were chapped and sore. The itching was very troublesome. Her general health was good. She had been under medical treatment for many months by an experienced surgeon in the neighbourhood, who at last, as she did not improve, sent her into the infirmary. I ordered a warm bath and five grains of the extractum urticæ dioicæ to be taken three times a day. After taking this for a few days, she complained of a stinging feeling in her skin, which she had not had before. The eruption slowly but decidedly diminished. Her hands and arms, which had been covered with an agglomeration of pustules, vesicles, scabs, dry scales, excoriations, and deep chaps, were in about six weeks quite well. She was discharged eight weeks after admission, with merely the remains of vesicles on the shoulders and neck, and with directions to take the decoction of the fresh nettle at home. She returned, two months afterwards, perfectly well. During the course of her treatment in the infirmary, pitch ointment was applied to the eruption on one of the shoulders, to relieve the distressing itching, and with some advantage.

Case 8.—I. F., aged 8, a brunette, of somewhat strumous organisation, but not unhealthy looking, though delicate, was affected with a dry, furfuraceous, hardened, and rather red condition of the palms and palmar surface of the fingers and thumb of both hands. They were chapped, and she complained that they were painful and itched. The complaint was of four months' standing. She was ordered to take five grains of the extractum urticæ dioicæ three times a day. In five weeks (after taking half an ounce of the extract), her hands were well. They were free from pain and itching; the colour was natural; and there was no dry scaling epidermis; but the skin was not so soft as it should be, though no harder or dryer than is natural to some hands. The general health was improved.

Case 9.—Ellen C., 11 years old, and of scrofulous habit, had been subject for eight years to unhealthy strumous sores on her hands. They commenced either as vesications or as flat livid tubercles, about the size of sixpence, and as much elevated above the skin; superficial ulcers formed, which healed with great difficulty, leaving white scars. Various means had been tried without success; and, as the complaint seemed to depend on a cachectic state of the system, I ordered, on April 5th, 1851, five grains of the extract of urtica twice a day. She rapidly improved; the sores

healed, and the tubercles diminished in a fortnight. On leaving off the medicine, a few fresh vesicles formed; but, on resuming it, these healed, and in a month her hands were well. At the end of three months, there had been no return.

Case 10.—Elizabeth C., aged 6, a fine, light-haired child, whose general health was usually good, but who was strumous looking, had been affected for some weeks by a dry papular eruption of the back, chest, arms, and legs, with some distinct pustules on the hands and arms. The papular eruption was lichen agrius; but the pustules had so much the appearance of pustular itch, and were attended by so much itching, that I ordered (October 18th) sulphur ointment to be freely applied, and diluted sulphuric acid with quinine twice a day. This was continued for twelve days, when the pustular eruption was quite well, but the lichen was not improved. She was then ordered to take five grains of the extractum urticæ dioicæ twice a day.

Nov. 8th. She had taken two drachms, and the eruption was much diminished.

Nov. 28th. She had taken three drachms more, and the eruption was almost well. After this she took three drachms more; and her skin was then free from any eruption, and her general health excellent.

Case 11.—S. P., aged 15, on October 28th, 1852, had large patches of lepra vulgaris on the legs, and smaller ones on the thighs, of two years' standing, and gradually getting worse. Menstruation was scanty; she complained of giddiness and aching in her limbs. I ordered her to take five grains of extract of urtica three times a day.

Nov. 10th. She had taken four drachms, and the eruption was declining. She felt better, and had less pain in her limbs.

Dec. 21st. She was very much better. She had taken one ounce. When giddy, I ordered her to take an aperient.

Jan. 27th. She was quite well, having taken another ounce. As she had not menstruated, she was ordered to take iron.

Case 12.—Mrs. W., a fat-florid woman, about 50, had had for six years lichen agrius, chiefly over the extensors of both arms, and of the skin covering the chest, back, and abdomen. She had been under treatment by various practitioners without benefit. At one time she was ten months continuously under one, and at another period six months under another, until she was "tired of taking medicine." The itching was considerable. In addition to getting the secretions right by alterative mercurials and aperients, she was directed to take a quarter of a pint daily of the decoction of nettle, and subsequently five grains of the extract twice a day. She began on May 18th, and on the 29th of August the eruption, which had gradually declined, was quite well, and remained so when I saw her five weeks afterwards.

Case 13.—Mrs. A., aged 43, July 9th, 1854, for six months had had lichen agrius of both arms. Both legs were affected with a diffused deep red eruption, with much itching, but without discharge. Sulphur ointment was applied to the legs; five grains of compound rhubarb pill were taken every night; and five grains of the extract of urtica twice a day.

July 31st. She had taken one ounce of the extract, and was much better.

Aug. 7th. The legs were nearly well, but there was rather more eruption of lichen. The extract was omitted.

Aug. 21st. The lichen was in the same state. She was ordered to resume the urtica.

Aug. 28th. The lichen was well, and the legs also.

In this case, as not unfrequently happens, the urtica seemed occasionally to aggravate the eruption; and, in the following, it seemed to bring it out, tending to show its real efficacy on the system.

Case 14.—A. T., aged 44, a tailor, had inveterate psoriasis palmaria, of four months' standing; both palms were covered with dry hard epidermis, and tender from numerous chaps. He was directed to apply glycerine, and to take a quarter of a pint of the decoction of urtica daily. After taking the decoction for a week, he became almost covered with a papular eruption of lichen, which itched considerably. His hands felt better. On omitting the nettle, the papular eruption declined and desquamated. He resumed it, and, with the constant use of glycerine, his hands, when he last called on me, were almost well, and quite comfortable.

These are a few out of a much larger number of cases in which I have tried the urtica during the last two or three years, as extract and decoction. From this

experience, it seems to be particularly indicated in inveterate chronic papular eruptions (lichen), and in various forms of vesicular and scaly diseases (eczema, lepra, and psoriasis), especially when occurring in cachectic states of the system. By those engaged in the country as parochial surgeons, who have numerous cases of chronic skin diseases in unhealthy children, and who have not already employed this remedy, it is worth a trial, from the facility with which it can be procured by the poor.

It is hardly necessary to add that, if the tongue is loaded, it is advisable to commence with mild mercurials and aperients, before the urtica is used; and that, in all cases, a plain wholesome diet, and frequent washing of the whole skin, are to be recommended. Indeed, to prevent the return of skin diseases which have been of long standing, it is of primary importance that the whole surface of the body should be washed daily with soap and water; for, as eruptions are often the means which nature sets up for relieving a faulty state of the system through the skin, it is rational to suppose that a free and perspirable condition of that extensive surface will assist nature in removing her waste matter, and thus will prevent the necessity of this scaly, or papular, or vesicular action. What the older practitioners attempted by means of issues may be more naturally effected by the daily use of soap and water to the whole cutaneous surface. In one case, this practice alone, steadily persevered in for several months, and continued as a habit, permanently cured psoriasis palmaria of more than twenty years' duration.

Washing the whole surface daily with soap and water will be found to be a very serviceable recommendation, not as a mere matter of cleanliness, but as one of the directions for the general treatment of many chronic diseases, and for the preservation of health in advancing life. Its effect is very different to that of merely washing the body with cold water, or using shower baths, or bathing followed by dry friction. It does not give the same shock as cold water alone; and it is thus much more easily borne by the delicate, who find that the reaction after cold sponging is exhausting. It more effectually removes the waste epidermis than flesh-brushes or hair-gloves; and suits those whose delicate skins are often irritated into eruptions by these rough appliances. For those who are very subject to colds, and are yet too delicate to bear cold sponging, the addition of soap, by its stimulating property, acts as a tonic to the skin, and thus guards it against the effects of sudden changes of temperature, besides aiding in getting rid of the waste and superfluous matters, which often require the actions which constitute a cold for their discharge from the overloaded system. The yellow soap, from the resin it contains, is an excellent stimulant as well as detergent; and a better still, in many cases, is a soap containing a small quantity of Barbadoes tar, called Hendrie's petroline soap, the daily use of which is peculiarly serviceable in keeping up a smooth and perspirable state of the skin when it is naturally dry, and especially in declining life, when the loss of muscular power prevents exercise carried to perspiration; for, as man is organized to earn his bread by the sweat of his brow, if he cannot do this, he must use some artificial means to produce the same state of his perspiring organ, or he will suffer. A physician, distinguished in his day, in a neighbouring county, who now, though verging on 90, retains a freshness and vigour of intellect, as well as a sound condition of health, which enable him to enjoy his existence physically and socially, and to devote a portion of each day to the cultivation of science, informed me that he attributed much of his health and activity to this daily soaping of his skin, which he had persevered in since he was a very young man. And the elderly, who usually have much spare time at their disposal, will find that the employment of a portion of it daily in this way will repay them for their pains. In the management of the disorders of the general health which so often attend the cessation of menstruation, this practice is, for obvious reasons, to be recommended.—*Association Med. Journal, Nov.* 10, 1854, *p.* 1910.

88.—ON DISEASES OF THE SKIN.

By M. Bourguignon.

[M. Bourguignon has communicated to the Society of Medicine of Paris, a report on M. Devergie's "Practical Treatise on Diseases of the Skin," from which the following extract is taken. M. Devergie divides skin diseases into twelve groups, viz.:]

1st, exanthematous affections; 2nd, vesiculæ and bullæ; 3rd, pustular diseases; 4th, cachectic; 5th, papular; 6th, scaly; 7th, exotic; 8th, scrofulous; 9th, those of the scalp; 10th, those of the nails; 11th, accidental productions; and 12th, syphilitic.

This classification is in truth as good as any other, although it makes it necessary to treat many times of the same disease; thus, for example, eczema, impetigo, ecthyma, studied amongst the groups of vesicular and pustular diseases, are a second time found among the affections of the scalp; this, however, we repeat, is a detail of minor importance.

It does not displease us to see the itch driven from the group of vesicular diseases to take its place amongst the papular. At last, no doubt, it will be ranged amongst the number of parasitic diseases.

M. Devergie excels in describing the various pathological forms that the anatomical elements of the skin assume. We would mention, among others, as having particularly attracted our attention, his description, amongst the exanthemata, of pellagra; amongst the vesicular diseases, of eczema, which sometimes assumes a hybrid form; of herpes zoster; and of chronic pemphigus, a complaint that never quits those who are unfortunate enough to be attacked by it.

The author has treated with particular care of rupia among the cachectic affections, and of the itch among the papular; he exposes, at great length, the reasons which induce him to believe in the *spontaneous generation* of acari; he believes, in short, the presence of the insect to be the result of disease, and that we have, up to the present day, mistaken the cause for the effect.

This pathogenic doctrine is, in our eyes, of very great importance; it is my intention to treat of this subject more fully afterwards.

In the sixth group we shall mention the description of psoriasis; where there will be found a notice of the different modes of treatment, compared with that by means of arsenic; in the eighth group we find the scrofulous diseases which the author attributes to a degenerated and transformed syphilitic taint, and which, although somewhat removed from the special field of dermatology, will be read with veritable interest. Among the affections of the ninth group I will quote tinea or favus, which has been abandoned almost exclusively to the medication of empirics, inasmuch as, in July, 1853, a peasant, who devoted himself, in his native village, to the cure of this disease, showed to M. Bazin the means of treating it by depilation. The twelfth group contains the chapter on syphilitic diseases, in which the author expresses his opinion on a subject much disputed, viz., regarding the characteristic form of the primary, and the contagiousness of the secondary accidents.

Such is a brief sketch of the descriptive part of the work. I would gladly dwell longer upon it, but I prefer fixing your attention on the chapter which treats of the general pathology, which seems to me to be the most excellent part of the "Practical Treatise on Diseases of the Skin."

M. Devergie expresses his astonishment at the isolated position which the diseases of the skin still hold in reference to affections of other organs.

He observes, that the diseases commonly known under the name of herpetic affections are, in every respect, identical with similar inflammations of other tissues. He attributes their origin to the same cause, and assigns to them the same progress and termination; he does not seek for an explanation of the disease in any morbid element peculiar to itself, nor in any specific virus, but in the temperament of the subject, his constitution, hereditary tendency, climate, moral causes, privations, and former diseases; in a word, M. Devergie conceives that the pathology of the skin does not differ from the pathology of other tissues and organs, except in the varied form of its morbid productions. According to him, the morbid elements and causes are identical, and consequently, there ought to be a close similarity in the therapeutics of these and other diseases.

In order to prove that his doctrines are well founded, he lays down the following general principles:—

1st. That the generality of diseases of the skin have a certain morbid inflammatory character, such as is seen in the greater number of the diseases affecting the various tissues of other organs in the economy.

2nd. That the diseases of the skin may be produced by all the causes which give rise to diseases of other tissues or organs, as physical, moral, and hereditary causes, climate, temperament, constitution, &c.

3rd. That the diseases of the skin follow in their progress the same course, and present the same termination, as the diseases of other organs.

4th. That if the morbid forms met with in cutaneous diseases are very varied, it is because the tissue of the skin is the most complex of all the tissues in the economy.

Then there follows the enumeration of some diseases due to each of these orders of causes.

The treatment is dwelt upon with particular care, and is inferior to nothing which has ever been written on the subject of therapeutics. In the treatment of cutaneous affections the author wishes to keep in view the disease itself as well as the phenomena that it developes, their cause, and, above all, the temperament of the patient; for certain morbid forms are always connected with certain temperaments, and each temperament demands a peculiar plan of treatment.

On the subject of therapeutics he successively treats of the following:—

A. Of the antiphlogistic treatment.

B. Of the antilymphatic treatment.

C. Of the antipapular treatment.

D. Of the antisquamous treatment.

E. Of the mixed or compound treatment.

These considerations on the subject of therapeutics are concluded by an excellent article on baths in general.

The antiphlogistic treatment comprises bleeding, emollients, and purgatives, from which it is necessary to abstain in the treatment of rupia and pemphigus; resolvents, in solution, as acetate of lead, nitrate of silver, alum and corrosive sublimate; and in pommades, as oxides of zinc, tannin, camphor, chloroform, &c.

The antilymphatic treatment comprises bitter preparations, and such as are of an antiscorbutic nature, as the chlorides of barium, of gold, or of calcium, preparations of iron, and cod-liver oil.

The antipapular, or antinervous treatment, comprises alkaline preparations in baths, drinks, and pommades; the doses of the subcarbonate of soda and of potash are remarkably diminished in comparison with the formulæ generally adopted. Amongst the alkaline waters those of Loueches, Vichy, Neris, Bourbon-Lancy, deserve a preference, especially in reference to chronic lichen. This mode of treatment also comprises the local use of chloroform, camphor, &c., so efficacious in complaints attended with much itching, and also tincture of cantharides, given in doses of one drop night and morning, gradually increased to ten drops.

The antisquamous treatment consists of the arsenical preparations so useful against psoriasis. M. Devergie lays much weight upon their administration, not only in this variety of diseases of the skin, but in a number of other affections, equally chronic and rebellious to treatment. In theory, and according to the idea that we have ourselves formed of the primary causes which remotely produce diseases of the skin, the use of arsenic seems to us to be valuable in exactly the same way that the preparations of iodine and mercury are of use against the syphilitic affections of the skin. Certain cases in which the rebellious eruptions of the skin alternate by metastasis or by repercussion with diseases of the stomach and of the womb, have very much arrested our attention, whilst the curious articles published by all the press concerning the arsenic eaters have caused us to make some reflections on the administration of such preparations. In fact it is with the most lively interest that we treat of the therapeutical and physiological effects of arsenic. And if it be true, as the assertions of Dr. Tschudi do not permit us to doubt, that the inhabitants of Southern Austria, Styria, and the Tyrol, often make use of arsenic in comparatively large doses, in order to produce an appearance of freshness and *embonpoint*, and to facilitate the functions of respiration; and if it is equally incontestible that the breeders of horses and oxen give it to these animals in order to excite in them the power of getting fat, and to give fineness to their coats, we can better understand the good effects of arsenical preparations in the scaly affections. Arsenic will, no doubt, after these recent publications, have its period of fashion, but assuredly it will always remain as a valuable remedy against a great number of cutaneous diseases hitherto considered incurable. M. Devergie refers to the action of arsenious acid, which has been found much less beneficial than that of the arsenites and arseniates; and it is important to bear this in mind in the administration of so poisonous an agent.

He was the first to observe a brown discoloration of the skin, named *arsenical spots*, when this drug is pushed to saturation.

The mixed or compound treatment is chiefly recommended in syphilitic affections; it principally consists in a solution of two and a half drachms of iodide of potassium in fifteen ounces of water, of which a teaspoonful is to be taken night and morning in a cup of sudorific infusion. An hour after, M. Devergie administers corrosive sublimate in the form of a pill, with extract of opium.

He sometimes orders a syrup, containing—

Iodide of potassium, 3ijss.; corrosive sublimate, gr. i. to iss.; thebaic extract, gr. iij.; syrup, ʒxv. M.

This mixture he prefers to the association of iodide of mercury with iodide of potassium, so as to form hydriodate of mercury, for in this case it is an error to suppose that any notable dose of iodine is administered.

In the scrofulous syphilis he gives the cod-liver oil, the syrup of iodide of mercury, and afterwards iron. For tertiary symptoms, he adds an arsenical preparation, Fowler's solution, in doses of from one to ten drops. In those skin diseases which are essentially scrofulous, he recommends cod-liver oil, wine of gentian, syrup of iodide of iron, all mixed together. In lupus, he gives cod-liver oil in doses of eight to ten teaspoonfuls every day.

I wish to draw attention to this plan of associating together these medicaments, which are in general administered separately, because this fact shows the peculiar tendency of our author's views; we know that he believes in the compound forms of diseases of the skin; he continues faithful to this principle in their treatment.

A chapter upon baths, and that by no means the least instructive of the work, completes the part devoted to treatment. Baths act in various ways, owing to their composition, their temperature, and the length of time the patient is subject to their influence; and in the douche bath by the violence of the jet; thus the douche with a single jet is best suited for neuralgia, that with many jets for paralysis.

The vapour baths are made the subject of a minute and interesting description; they act by the production of pulmonary and cutaneous perspiration.

They ought to be, in cases attended with secretion, of a temperature of about 35 degrees Cent. when the patient enters; afterwards they should be raised to 38, 40, or 42 degrees. In rheumatism the temperature ought, on the contrary, to be carried from 50 to 55 degrees, because in this case it is necessary to excite the skin more powerfully. A vapour bath should not last more than from 20 to 25 minutes; when it causes palpitations, it is because the temperature is too high. In certain cases, two or three shower baths of comparatively cold water may be given with excellent effect, especially in the cutaneous affections of the face. When a Russian bath is administered, the vapour is raised to 55 degrees, and cold douches are administered every five minutes.

The vapour baths do not disturb digestion; it is preferable that they should not be given when fasting.—*Rev. Med.—Dublin Hospital Gazette, Nov* 1, 1854, p. 296.

89.—ON THE TREATMENT OF THE MORE COMMON FORMS OF SKIN DISEASES.

By Dr. J. H. Bennett, Professor of Medicine in the University of Edinburgh.

Eczema is by far the most common disease met with, both in its acute and chronic forms. The local treatment I have found most efficacious, is that which I recommended in the August number of the Journal for 1849. It consists in keeping the affected part moist, with lint or linen saturated in a very weak alkaline solution, consisting of soda subcarb. 3ss. to a pint of water. For this purpose it is necessary to cover the moistened lint with oil silk, or gutta percha sheeting, which should well overlap the lint below, so as to prevent evaporation. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean, and prevent the accumulation of those scabs and crusts, which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their

fiery red colour, and merge into that of the healthy skin, and, finally, the whole surface assumes its normal character.

Herpes.—This disease generally runs its course in about fourteen days, and requires no treatment whatever, further than an acetate of lead lotion to allay the smarting. It is not very common.

Scabies occurs very frequently, and is cured by a host of remedies. A strong lather, of common soft soap and warm water, twice a day answers very well. The question with scabies, is not what remedy is useful, but which will cure it in the shortest period. The most extensive experience at St. Louis has shown, that the sulphur and alkaline, or Helmerinch's ointment cures itch, on an average, in seven days. That sulphur, however, is not the active remedy, I have satisfied myself of by experiment. Soft soap, as we have seen, which contains alkali, and even simple lard, if pains be taken to keep the parts constantly covered with it, will cure the disease as soon as sulphur ointment. I have tried the Stavesacre ointment, recommended by M. Bourguignon, in only a few cases, but found it to answer very well. Its superiority, however, over other applications, I am not as yet prepared to admit.

Pemphigus.—This is rather a rare disease, and when chronic, coming out in successive crops, is very rebellious. Two cases which entered the infirmary last winter were cured in a few weeks by the weak alkaline wash, applied as in the case of eczema, combined with generous diet.

Impetigo.—This affection in all its forms is very common, and is best treated by the weak alkaline wash, exactly the same as in eczema. In the chronic forms which attack the chin of men, constituting one of the varieties of mentagra, the same treatment cures the most rebellious cases if the moisture be constantly preserved. For this purpose the hair must be cautiously cut short with sharp scissors, and the razor carefully avoided. If the side of the cheek covered with the whisker is attacked removal of the hair from thence also is essential to the treatment. A bag or covering accurately adapted to the part affected must be made of gutta percha sheeting, and tied on with strings. This may be covered with black silk, to allow the individual to go about and carry on his usual occupations. In this way I have frequently seen chronic impetigo of the chin, of from eight to ten years' standing, completely removed in a few weeks. But then the surface must be kept *constantly* moist, a circumstance requiring very great care and determination on the part of the patient. When it becomes necessary to shave, flour and warm water, or paste should be used, and not soap. Alkalies applied from time to time only, as in the form of wash or soap, always irritate, although, when employed continuously, they are soothing.

Ecthyma is not a common disease, and usually presents itself, as the *E. cacheticum*, requiring in addition to the alkaline wash locally, a generous diet.

Acne is a disease always requiring constitutional rather than local remedies. Although not uncommon in private, it is rare in hospital practice. Careful regulation of the diet, abstinence from wine and stimulating articles of food, watering-places, baths, &c., &c., constitute the appropriate treatment.

Rupia.—This disease I have never seen occur but in individuals who have been subjected to the influence of mercurial poisoning. Hydriodate of potassium and tonic remedies, with careful avoidance of mercury in all its forms, is the treatment I have found most successful.

Lichen and Prurigo.—In both these affections constant inunction with lard is as beneficial as constant moisture in the eczematous and impetiginous disorders. In the prurigo of aged persons, the Ung. Hyd. Precip. Alb. is a useful application, although the disease is not unfrequently so rebellious as only to admit of palliation. The chronic papular diseases often constitute the despair of the physician.

Psoriasis, and that modification of it known as *lepra*, are very common diseases, and are uniformly treated by me externally with pitch ointment. I have satisfied myself, by careful trials, that it is the pitch applied to the part that is the beneficial agent, as I have given pitch pills, and infusion of pitch, largely internally without benefit. With the hope of obtaining a less disagreeable remedy, I have frequently tried creosote, and naphtha ointment and washes, but also without benefit. Lastly, I have caused simple lard to be rubbed in for a lengthened time, but without doing the slightest good. The oil of cade is also very useful, especially in psoriasis of the scalp. Internally, I give five drops each of Fowler's solution, and of the tr. cantharidis

It is rare that the internal treatment alone produces any effect on a case of psoriasis of any standing. If a case resists this conjoined external and internal treatment, I have always found it incurable. About a year ago I carefully treated a series of cases internally with Donovan's solution, without producing the slightest benefit.

Lupus is a constitutional disease, and must be treated by cod-liver oil, and all those remedies useful in scrofula, of which it is a local manifestation. The external treatment is surgical, consisting of the occasional application of caustics, red lotion, ointments, &c., according to the appearance of the sore.

Favus is a very common disease in Edinburgh, and is most readily removed, first, by poulticing the crusts till they fall off, and the skin presents a smooth, clean, surface; secondly, by shaving the hair; and, thirdly, by keeping the scalp continually covered with oil, so as to exclude the atmosphere, and prevent the growth of the parasitic fungi, which constitutes the disease. For this purpose, a gutta percha or oil silk cap must be constantly worn. A continuance of this treatment for six weeks produces a cure in young persons if combined with cod-liver oil, generous diet, and anti-scrofulous remedies internally. I have tried the lotion of sulphurous acid, recommended by Dr. Jenner, and found it successful in a few cases, but the treatment by oil is so easy as to be far preferable to it. Very chronic cases are cured with difficulty, but so long as the oil is applied the disease never returns, and mere freedom from the disgusting crusts is a great gain.

Scalp diseases must be treated according as it depends on eczema, impetigo, psoriasis, or favus, in all cases first removing the crusts with poultices, then keeping the head shaved, and, lastly, applying alkaline washes, pitch ointment, or oil, according to the directions formerly given. Ringworm is a disease I have never seen in Edinburgh, and of what it consists I am ignorant. Some writers apparently consider it to be favus, and others a form of herpes. On two or three occasions I have seen a scaly disease of the scalp, in the form of a ring—that is lepra, which I have cured by pitch ointment, or oil of cade. My friend, Dr. Andrew Wood, informed me some time ago, that he banished it from the Heriot's Hospital school by condensing on the eruption the fumes of coarse brown paper, and thus causing an empyreumatic oil, or kind of tar, to fall upon the part. This has led me to suppose that it is a scaly disease, and a form of lepra or psoriasis.

So-called *syphilitic diseases* of the skin are, in my opinion, the various disorders already alluded to, modified by occurring in individuals who have suffered for periods more or less long from the poisonous action of mercury. A longer time will be required for their cure, but the same remedies locally, conjoined with hydriodate of potassium, in smaller doses, with bitter infusions, tonics, and a regulated diet, offer the best chance of success.

The great difficulty in the treatment of skin diseases generally consists in their having been mismanaged in the early stages—a circumstance I attribute to their not having, until a recent period, been much studied by clinical students. Many chronic cases of eczema are continually coming under my notice, which, in their acute forms, have been treated by citrine ointment, or other irritating applications, which invariably exasperate the disorder. I shall not easily forget the case of one gentleman, covered all over with acute eczema, who had suffered excessive torture from its having been mistaken for psoriasis, and rubbed for some time with pitch ointment. In the same way I have seen a simple herpes, which would have readily got well if left to itself, converted into an ulcerative sore, by the use of mercurial ointment. Nothing is more common than to confound chronic eczema of the scalp with favus, although the microscope furnishes us with the most exact means of diagnosis. I need scarcely say that the correct application of the remedies I have spoken of can only be secured by an accurate discrimination, in the first instance, of the diseases to which they are applicable.

The general constitutional treatment in all these cases seldom demands aperient or lowering remedies except in young and robust individuals with febrile symptoms. In the great majority of cases, cod-liver oil, good diet, and tonics are required. In a few instances, sedatives, both locally and internally, are necessary to overcome excessive itching and irritation. These the judicious practitioner will readily understand how to apply according to circumstances.—*Monthly Journal of Medicine, Jan.* 1855, *p.* 19.

90.—ON PLASTIC SURGERY.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

When a part covered by cicatrix has to be restored, the cicatrix should be entirely removed. Suppose, for example, the point of the nose to have been destroyed; it is not sufficient to pare the edges of the defect and annex the flap to the denuded edges; it is absolutely necessary to remove very carefully the whole cicatrised surface of the nasal stump which is to be covered by the engrafted flap. Of course this denudation of cicatrix is to be effected with the greatest forbearance towards any remaining sound portions of the defective part. Langenbeck has insisted very strongly upon these points, and with great reason, especially in cases where the free borders of the facial integument, the lips, the edges of the eyelids, and the alæ nasi are concerned; for, as these parts can never be restored in their original perfection and delicacy, any portions of them which remain must be most carefully preserved. In two cases, where the whole of the cartilaginous portion of the nose was destroyed up to the very narrow remains of the free edges of the alæ nasi, he pared these edges, and united them by means of fine sutures with the flap transplanted from the forehead, so that the natural contour of the nostrils was most perfectly restored. In other cases he has preserved the tarsal border of an eyelid otherwise destroyed, and to this annexed the flap, so that the new eyelid, after healing, has been furnished with eyelashes along its whole border. I have followed this proceeding in one case with complete success.

If a transplanted flap were simply united with the denuded edges of a defective part, and the raw surface of the flap were applied upon the old surface of cicatrix covered by a more or less normal epidermis, the two surfaces would never unite, the flap would become raised, thickened, and undergo considerable alteration in form and appearance. The whole of the epidermoid surface of the cicatrix should therefore be pared away, so that two raw surfaces may be brought into apposition.

In order to bring the edges of the flap and the pared edges of the defect into perfect acquisition, the latter must be loosened from their attachment to the subjacent parts, to the extent of two, three, or four lines, by flat strokes of the knife. This is too often neglected, and the consequence is, that it is found difficult to bring both edges to the same level. One projects beyond the other, and the flap remains raised even after union by first intention. But if the edges of the defect be so loosened that they can be raised from their attachment, both the edges are easily brought into close apposition, and no difference of level is observed after union takes place.

As very small defects can always be restored by some of the simpler methods, no very small flap can ever be required, certainly not less than half an inch in breadth. The largest flaps ever employed have been about six inches by five, and have been removed from the shoulder to fill up gaps left in the neck after the division of cicatrices.

The size of the nourishing peduncle varies very much with the nature of the operation. In forming a nose from the arm, it is left as broad as the flap itself. It is also left very broad when large flaps are transplanted from the shoulder or arm to the neck or thorax. When the flap is taken from any part of the face, a much narrower peduncle will suffice. The minimum of safety has not yet been determined. The medium of Langenbeck's operations, when the nose has been restored from the forehead, is 6 lines for the breadth of the peduncle; the narrowest was 4½, the broadest 7. Probably it might be reduced to 4, if thought necessary or desirable. But it must be remembered, as Langenbeck observes, that the skin near the root of the nose is very firm and full of vitality, and that there is scarcely any other part where the formation of so narrow a nourishing peduncle could be safely practised.

The supply of blood would be insufficient if the peduncle were either too narrow or too tightly stretched. The reflux of blood would also be prevented by too great twisting, or by the pressure of the twisted sutures.

If either the flap or peduncle be too much stretched, the skin assumes a marbly hue, and it is only by loosening the sutures, lengthening the peduncle, and freeing the surrounding parts, that gangrene can be prevented. Too great twisting of the peduncle can be obviated by giving an oblique direction to the primary incisions.

When annexing the peduncle in its new situation, all stretching and compression must be avoided. The sutures on each side should not be opposite to each other, or the narrow strip between them would inevitably be compressed.

The effect of the best-executed operations is often marred by the contraction of the free edges of a flap. This is to be avoided either by uniting the free edge of the flap with neighbouring mucous membrane; by turning the free edge inwards, and so fixing the two raw surfaces of the flap together by suture, that the edge consists of a round duplicature of the flap; or, thirdly, by reckoning for the degree of cicatrix contraction, and making the flap too long.

The fourth method according to which plastic operations are performed, I have described as follows:—"By using some part of the body of another person, or some distant part of the body of the same person, which has been entirely separated, and retains no connection with the person or part from which it has been removed."

Very wonderful accounts are to be met with in many books, of successful reunion of parts of the body which had been entirely separated. One man cut one of his fingers off; and, after a long search, found it among some saw-dust. It was then cleansed, replaced, and union took place. Another fished his finger out of a gutter, and the same fortunate result was obtained. A man bit off another man's nose, bruised it with his teeth, and spat it into the dirt. This also was successfully replaced. Another nose, thrown into the dirt, or, rather, the separated point of it, was eaten by a dog, out of whose stomach it was afterwards cut; and this also united, after some hours' separation. Many such stories as these might be collected from the writings of men of good character and reputation. They are told sometimes with exceeding gravity, and with an amount of concurrent testimony which leads to a belief in the truth of some of the least marvellous, and suggests the inquiry whether success in such cases is to be considered as a lucky accident, or whether the retarded replacement, the apparently unfortunate delay, by which the wonder of the story is kept up, may not have been perhaps the cause of the fortunate result—whether the surgeon proceeds upon the best principles who at once sews on and binds up a separated part at the hazard of interposing a layer of coagulated blood between the two surfaces, and thus preventing their reciprocal contact; or how long a part, perfectly separated, retains its vitality sufficiently to become again a part of a living body, and at what period after the entire separation of a part of the body, or at what stage of the changes which take place in it, does reunion most readily take place?

Dieffenbach describes the change which takes place in a part which has been entirely separated as follows:—"If we cut away a piece of skin, which we have frequent opportunities of doing after surgical operations when parts are extirpated or amputated, we may notice a series of interesting phenomena, which show themselves still more clearly on the immediate separation of a flap of skin from the body. At the moment when the flap of skin is surrounded by the knife it becomes pale, not in consequence of loss of blood, but, when this is very small indeed, from a pure nervous affection, a spasmodic condition in the capillaries of the corium, which throws the blood out of it into the deeper parts. Very soon this pallor becomes less, even when the flap is perfectly separated. After the first bleeding from the edges, fresh blood flows, and this is repeated several times after its separation, until there appears a yellowish thin blood, and at last lymph. The edges of the corium are drawn together as if by a slight spasm, and the whole flap is slightly arched. These phenomena are most striking when the skin is from the scrotum; and, as I once took away a large portion of the scrotum, and applied it spread out, I found it afterwards wrinkled up together. Gradually life in the flap expires, and with its death all the above phenomena cease."—*Med. Tim. and Gazette, Dec.* 30, 1854, *p.* 661.

91.—ON DEEP-SEATED WHITLOW.

By JOHN HAMILTON, Esq., Surgeon to Richmond Hospital, Dublin.

[The following case illustrates an affection which has lately been very prevalent all over the country, and if the same deep incisions were made oftener, more fingers would be saved. This man was 40 years old, and was seized with a deep-seated

whitlow. Deep incisions were made in the front of the first phalanx, out of which matter gushed, to the man's great relief. Incisions were afterwards made in the palm and dorsum of the hand, but still the finger was obliged to be removed at the metacarpal articulation.]

The removed finger was carefully examined. The two last joints were black and shrivelled, and a faint line of separation was commencing between the dead and living parts. The first phalanx offered a good example of deep-seated paronychia in the advanced stage. The fibrous sheath of the tendon was very thick, the section white and firm, the lining membrane of a pink colour, and here and there bloodshot, with some deposits of flaky lymph. The flexor-profundus tendon was loose and soddened, and surrounded by pus. The periosteum separated from the bone by pus, and the bone dead. You will see here the contract between this natural thin white sheath, with its smooth, pearly white lining membrane and glistening tendon, and the thick sheath of the whitlow finger with its rough, lymphy red inside, and its dull tendon smeared with pus.

Many of you will think this subject scarcely worth your attention; but you are wrong; for you will find deep-seated whitlow a very serious and painful disease, by which the use of the finger or hand may be destroyed, and which requires prompt treatment; and nothing but a thorough knowledge of its nature and progress will lead you to insist early on those measures, which, though apparently severe, will alone arrest its destructive progress. As you have just seen, deep-seated paronychia is an inflammation of the inside of the sheath of the flexor tendons of the fingers, rapidly running into suppuration. The symptoms are usually as follows: the affected finger becomes painful, the pain increasing to the greatest degree, so as to prevent sleep, and often to cause the patient to spend the night walking about his room in torture. This will be at first accompanied by little swelling, and little redness, more especially in front, although the greatest suffering is complained of there. The finger will be kept bent, any attempt to straighten it increasing the pain, as does also pressure over the affected part. By keeping the finger flexed, the skin in front is relaxed, as well as the tendon, relieving the painful tension, and the pressure of the tendon on the inside of the inflamed sheath. The back of the finger next becomes red, swollen, and shining, pitting on pressure. You might be misled by this, and think it was the place to make your incisions in search of matter, but it will be found that pressure on this part can be borne, whereas in front, where Boyer says this whitlow always occurs, it cannot; and when the finger is attempted to be straightened, the suffering is at once referred to the front. After the third or fourth day, this part will be found swollen and prominent, particularly on a side view, but no fluctuation is discernible. If no treatment is adopted, the disease will often proceed from the finger to the palm, which becomes red, swollen, and tender, the back of the hand particularly so, of a deep shining red, pitting on pressure, and fluctuation is soon apparent either over the knuckle of the affected finger, or in the web between the fingers. In the palm of the hand we often observe that the matter, after having made its way out through a small opening in the skin, does not pierce through the cuticle, which in the labouring classes is excessively thick, but separates it extensively, and you have a considerable collection of pus only covered by cuticle, before you come to the real abscess; as the palm becomes engaged other fingers get flexed, and cannot bear extension, and the pain shoots up the arm to the shoulder; in a woman, I knew it extend to the breast of the affected side. In most instances the inflammation with suppuration stops at the finger and palm, particularly when treatment has been resorted to; but if not, it may go up the front of the wrist to the forearm, in which case the patient recovers after weeks of suffering, worn out by sleepless nights and profuse suppuration, with bent, emaciated fingers, and a stiff wrist, and a nearly useless hand. Should the disease not extend so high, or not be so violent, it may end with a stiff bent finger. I should say when there has been suppuration the finger is invariably stiff, and usually bent, from adhesion of the integuments to the sheath of the tendons, and of the tendons to the inside of the sheath, while in the state of flexion. If the inflammation is very intense, the tendons die and protrude through the natural or artificial opening that has let out the matter, and throw off greyish white sloughs; where a portion of the flexor profundus is totally destroyed, the finger may be quite straight, being kept so by the extensor tendon. Such is the case in the man now in the house, who came to the hospital

after having suffered from whitlow six weeks. I took away a dead portion of the flexor tendon, one and a-half inch long, from the sheath. I am going to remove his finger, as he presents a further step in the disease, viz., the periosteum is stripped from the bone of the first phalanx by effusion of pus, and the bone is killed.

The death of the bone is most common in the last phalanx, particularly of the thumb. In such a case you will find, though the matter has been let out by a free opening, the disease lingers, the part continues red and swollen, the opening discharges abundance of thin matter, and the large flabby granulations around it, and if you feel the end of the finger, there is a pseudo-fluctuating feel, as if it was extensively undermined; a probe passed in removes every doubt, by grating against the rough bone. It is best not to wait till it separates of itself, but enlarge the opening, and seizing the dead bone with a forceps, divide any ligamentous connexions at the joint, while any are on the stretch. The parts soon heal, the finger shortened, bent forward, and clubbed at the end, the nail irregular, but still a useful finger. In one of the coachmen in Mr. Champion's establishment, in whom the disease had been very violent for three months before I saw him, I found through a long side incision which had been made by a surgeon, the first and second phalanges dead and bare of periosteum, and the third projected through a sloughy opening at the end of the finger; though the bone was dead, the soft parts were alive. In other cases both mortify, and the whole or part of the finger becomes cold, black, and shrivelled, as in Phelan's case, and also in a woman in Mr. Adams' ward, now in the hospital. Last season I had to remove a finger, for this cause, from a woman in whom it had mortified a few days after the disease began. Indeed I have had to perform the same operation frequently, the cases being those in which the inflammation had been most acute, and the early treatment neglected. The excessive and rapid effusion into the sheath, and the attendant sudden swelling before the parts can accommodate themselves to it, seems to produce a strangulation, the circulation is arrested in the vessels, and the parts die.

The fever, in deep-seated whitlow, often runs high, and I have known delirium at night, but I never met with a fatal termination, except in one case, a man who crushed his thumb while intoxicated; deep-seated paronychia followed, inflammation of the veins up the arm, and death.

On looking over the notes of many cases, I find the most frequent seat of deep-seated whitlows to be the middle finger, next the ring, and then the index finger, all before the thumb. This agrees with Boyes's experience; it differs from what we perceive in the superficial paronychia, in which the index finger and the thumb are so much the oftenest affected, and at their extremities. The part of the finger in the deep-seated paronychia most commonly engaged is the sheath of the flexor tendon over the first phalanx; this sheath is not only the longest, but the strongest, and most complete. Men are more frequently affected than women, and the disease is most prevalent in winter. A few cases, when suppuration was threatened, subside and terminate by resolution. The majority run rapidly into suppuration, which is more or less extensive, according to the intensity of the inflammation. Such is an outline of the symptoms and pathology of deep-seated whitlow. You see that it is an inflammation of the inside of the sheath of the tendons, that nature makes vain efforts to get out the matter confined in the strong fibrous envelope; you have witnessed the disastrous effects of the unrelieved diseased action, the periosteum, the tendon, the bone, all at last engaged, and the finger, or even the hand, permanently injured or even lost. You will at once naturally arrive at the conclusion, that a free incision into the sheath, early performed, is the only treatment. Yet how is it that so many come to the hospital, where this has never been done, and all the destructive consequences allowed without an incision ever having been made.

I believe it arises from timidity, a fear of wounding blood-vessels, particularly when the disease approaches the palm; there the palmar arch becomes a kind of bugbear. Some years ago, I made careful dissections of the hand, with a view of furnishing myself with precise rules for making incisions in deep-seated whitlows. Permit me to call your attention to a few practical conclusions I arrived at:—

"There are two principal wrinkles or lines running across the palm of the hand; one, nearly transverse, corresponds to the joints between the metacarpal bones and

first phalanges, and is situated about an inch behind the first transverse lines on the fingers. The second begins from the first, at the articulation of the index phalanx with its corresponding metacarpal bone, and goes in an oblique direction across the palm towards the pisiform bone at the inside of the wrist; *it answers pretty nearly to the course of the ulnar or superficial palmar arch.* These two lines, which are very regular, will be found to serve as excellent guides in making our incisions in paronychia with confidence.

On the fingers there are three bundles of transverse lines; the two from the tip correspond to the articulations, that next the palm does not correspond to any joint, but is situated about the centre of the first phalanx end of the sheath of the flexor tendon.

The palmar fascia goes about as far as the transverse palmar line before it sends off its four processes which go between the fingers, and are attached to the sides along with the tendinous insertions of the extensor muscles and the lumbricales.

The web between the fingers contains the digital artery from the palmar arch and its division, and is filled up with loose cellular tissue, allowing matter easily to go from the palm to the back of the hand. As the superficial palmar arch, the only one likely to be wounded, corresponds to the oblique line which is behind the transverse line, the distance increasing as it approaches the inside of the wrist, you can, in all the fingers, *safely* make your incision as far as the transverse line; nay, in all fingers but the middle, you can cut beyond this as far as the centre of the hand, and in the index finger even further. But it is worse than useless ever to carry your incision from the finger further than the transverse line, so as to endanger the arterial arch at all, as after going so far it is quite sufficient to pass a director under the palmar fascia, which alone confines the matter, if it has gone into the hand. On the director, you could, if desirable, cut out through the fascia, and even the annular ligament. Whereas if you cut down without a director to the palm, through all the parts to the bone (which in the palm is unnecessary, as there is nothing below the palmar fascia or annular ligament to confine matter), you could scarcely avoid wounding the arteries or large nerves. In the same way in the fingers after the sheaths of the tendons are divided, nothing remains to confine matter. These sheaths extend from the tip of the fingers to the transverse palmar line or metacarpal phalangeal articulation; they are thick and strong between the joints, thin opposite to them; you should avoid over the joints, and confine your incisions to the strong part of the sheaths between them, cutting to the bone if you choose, or what is generally enough, down to the tendon, taking care to open the sheath well, and always keeping in the centre to avoid the digital arteries running along the sides of the fingers. In the thumb it is particularly necessary to observe this last rule, as otherwise, after cutting beyond the first joints, you would be very likely to wound the superficialis volæ.

Bearing these facts in mind, you will have no hesitation in making sufficiently free and deep incisions. I repeat, take care that your cut is long enough to open the sheath freely; a mere deep puncture through it may let out matter, and give ease for the time; but not allowing free vent, will cause the pus to seek another outlet, and much mischief ensue."

Three weeks ago, the Rev. Mr. Crampton's coachman came to me, suffering dreadfully from deep-seated whitlow in the sheath of the tendon of the first phalanx of the middle finger of the left hand. I plunged in a sharp straight bistoury, and laid open, by continuing the incision, at least one-half of the sheath; a quantity of thick matter gushed out, and he never had any more trouble from the finger. You are not always so successful; in spite of free incisions the diseased action is not stopped. In such cases, though there is at first much relief, the unpleasant sensations return, the next night the patient will be kept awake by deep-seated throbbing pain, and you may be sure that the matter is working its way out in some other direction, and is not getting free exit, and you will probably discover some swollen tender spot, to repeat the incision. If the disease presents itself in a more advanced and aggravated form, not only engaging the finger, but extending to the palm, back of the hand, or even the wrist, which last is very rare, you will have to follow the matter, regulating your incisions by the rules I have mentioned. Some authors have made a separate division of paronychia, which they name paronychia periostei, from

supposing the disease in this form to commence in the periosteum covering some part of the phalanges, with suppuration between the periosteum and bone, death of the bone, &c. I regard the affection of the periosteum rather as a secondary effect of the deep-seated paronychia, whose seat we have seen to be the sheaths of the tendons, but when neglected, to engage finally all the tissues, the periosteum among the rest, and the bone. When the first or second phalanges are stripped of periosteum, and dead, it is best to remove the whole finger at the metacarpal articulation, as I am going to do with the man in the house, who has only the first phalanx bare and dead. If it is the last phalanx, it can be removed alone, and as I have already said, a useful finger will remain after the opening is healed.—*Dub. Hospital Gazette, Feb.* 15, 1855, *p.* 21.

92.—ON CARBUNCLE.

[A very valuable and lengthy report on this prevalent affection is published in the "Med. Times and Gazette," for Dec. 2, 1854. The increase, in late years, of this disease has been considerable, and out of proportion to the increase of population. Everybody, whether delicate or healthy, seems disposed to be attacked. Diet would seem to be connected in some way with the disease. It is known that John Hunter cured himself of an habitual disposition to boils by a two months' use of carbonate of potash. We make the following extracts from the report.]

Premonitory Symptoms.—In 8 of the cases reported, no note as to premonitory symptoms was preserved. Of the 27, there were 9 in which the patient could not remember having felt in the least out of health, until the carbuncle appeared, and in one of these he had been previously feeling unusually well; in 3, a feeling of general lassitude had been the only symptom; in 1, headache; in 1, back-ache; in 1, "aching pains in all the bones;" in 1, liability to cramp had been observed. In the remaining 12 the introductory stage had been better marked. Drowsiness, headache, giddiness, and a feeling of extreme lassitude, sometimes with a tendency to stupor, had been its chief characters. In several instances these symptoms had been present for two months or more; in others for several weeks and in one for eight days only. The urine had generally been observed to be high coloured, and to deposit sediment on standing, and in most the bowels had been costive or irregular. A gradual decrease of relish for food had been observed in most in which there were any premonitory symptoms; but in few had it amounted to absolute loss of appetite before the outbreak of the carbuncle.

For these symptoms the only authorized generalization perhaps is, that they concur to indicate much functional disorder, and in all probability a vitiated condition of the blood. Whether produced by something noxious taken from without, or by the retention of principles which should have been eliminated, the data are yet wanting to decide.

Part Affected.—No single part of the body could perhaps be named on which carbuncle may not occur. Instances have presented themselves of late years in the hospitals on almost every possible locality. The feet, as far as our personal observation has extended, have been exempt, but Mr. Coulson has mentioned to us a case in which he was recently consulted, where the disease in a severe form was located on the dorsum of the foot. Its great rarity on the scalp is still observed. On the face, however, of late, the frequency of the carbuncle has been far greater than appears formerly to have been the case. When seated on the face, there is often present a more severe form of inflammatory fever, and the inflammation is less asthenic than in other regions. A series of most severe cases of this description occurred about three years ago in St. Bartholomew's Hospital, and since then we have chanced on several other isolated examples of the same kind. A case in St. Mary's Hospital, for instance, recently mentioned in our "Statistics of Operations," ended fatally after herniotomy, from the outbreak of an acute carbuncle of the lip. We shall not enter at any further length on the consideration of this class of somewhat peculiar cases, since it has already been minutely investigated in our pages. It was an observation of Sir Astley Cooper's, that when carbuncle attacked the head, it generally caused death. Though it must undoubtedly be admitted that the disease is by far more dangerous in this locality than in any other, yet recent experience does not

support so sweeping a statement. In ten out of the thirty-five cases tabulated, the disease was seated either partially or wholly on the face, and the whole of them notwithstanding recovered. The favourite seats of carbuncle are those in which the skin is thickest; the nape of the neck, the back, and the nates, standing prominent in the list. Of the cases mentioned, in 12 it appeared in the nape, in 10 on the back, and 2 on the buttock. More extended, though less exact, observation would lead us to believe that the proportion of cases in which carbuncle attacks the face is not nearly so relatively great as would seem from our table. Probably some allowance must here be made for what is mere coincidence.

Constitutional Symptoms attending the Outbreak.—The facts recorded in the columns referring to this part of the subject are intended to apply to the symptoms present during the time that the carbuncular inflammation was spreading. In very exceptional cases the disease was unattended by general disturbance, but in the large majority inflammatory fever of a more or less severe kind was present. In these the patient lost appetite, became heavy and depressed, had a somewhat furred tongue, a quickened pulse, costive bowels, and high-coloured urine. In some of the more cachectic and elderly patients the depression was severe, and the fever tended to what is familiarly known as "typhoid" in type. Some authors have asserted that the urine in carbuncle is generally phosphatic, and others that it is often saccharine; but neither of these conditions were observed in any one of the cases mentioned. The urine was always, when it differed from that of health, characteristic of inflammatory disease, being deficient in quantity, of a high colour, and generally loaded with lithates. It is but fair to attribute part of the constitutional disturbance present during the progressive stage of carbuncle, to the extreme pain almost constantly present. This will not, however, account for the whole, and the greater part is doubtless symptomatic of the carbuncular dyscrasia.

Duration, &c.—The average duration of the disease, reckoned from the commencement of the inflammation to such time as the ulcer was either healed, or so nearly so that the patient was able to leave the Hospital, was, in the cases included in the table, nearly seven weeks. The greater part of that period was of course occupied in the healing of the sore. Carbuncle does not appear to observe any determinate course as to the length of its stages. The time occupied in attaining its maximum size varied in the cases before us from three to sixty days, its average being rather more than fifteen days.

Nature of the Slough.—Rokitanski has asserted, contrary to the general belief, that the core of boils and the shreddy slough of carbuncles do not consist of dead tissue, but are inflammatory exudations, like the false membrane of croup. Without at all denying that they may be in part so formed, we cannot avoid expressing an opinion that their bulk is really a slough. The circumstance that in carbuncle, after the removal of the core, the subjacent muscles, &c., are often exposed, their cellular tissues having evidently been destroyed, the toughness of the membrane, and its firm adhesion to the surrounding parts, all seem to support the old belief. Mr. Paget informs us that he has, under the microscope, seen areolar tissue in the core of carbuncle, and one or two examinations of the same kind made by the writer have convinced him of the same fact.

General Treatment.—Three or four different plans of treatment are in vogue in the Hospitals for the treatment of carbuncle. 1st. By stimulants, full diet, quinine, ammonia, &c. 2nd. By opium, freely used, and generally combined with tonics. 3rd. A more expectant plan, by salines, low diet, &c., in the first stage, and the moderate use of tonics afterwards. In all these, purgatives, of course, form a part. One of the largest carbuncles which we have ever seen occurred to a tailor, from Hampton Court, who was treated by Mr. De Morgan, in the Middlesex Hospital, about two years ago. The case was an extremely severe one, the depression of the vital powers being such that for more than a fortnight the result seemed very doubtful. The very free use of opium, in combination with liquor cinchonæ, appeared to answer an excellent end, and the man ultimately recovered. We have, however, not seen the opiate plan tried on a sufficiently large scale to justify an opinion as to its general merits, the method by stimulants being by far the most frequently adopted. Mr. Lloyd, at St. Bartholomew's, is the surgeon who is perhaps the most sparing of any in the use of stimulating remedies; and we certainly have not observed that his cases do any worse than those of others. In the early stages, Mr. Lloyd generally

prescribes salines and fever diet; unless, indeed, the patient be very old or infirm. In one case under his care, after the sloughs were separating, several attempts were made to give stimulants, but on each occasion, a fresh accession of inflammation being caused, they were at length desisted from, and the man recovered very satisfactorily without. In carbuncles, as in all other diseases, the surgeon should doubtless pay less regard to the name and reputed pathology of the affection than to the symptoms actually present. If the system be much depressed, if the disease appear to have resulted from bad living, and, above all, if the patient's instincts demand them, stimulants should be pushed. If there be extreme restlessness, pain, exhaustion, &c., and if, after a few doses, it appears to agree with the stomach, the sustaining properties of opium may probably be very valuable. Should, however, the patient be of fair strength, the disease in its stage of advance, and attended by sharp inflammatory fever, it may be doubted whether any empirical evidence as to the usefulness of stimuli under such circumstances has yet been obtained sufficiently strong to lead the surgeon to ignore the rational principles of treatment. In such a condition, the arrest of the disease would probably be much sooner effected by alteratives, purgatives, and salines, than by brandy or opium. It is even not improbable that the bleeding caused by incision is often of great advantage.

Local Treatment.—From the time of Wiseman, the practice has been general of making free crucial incisions into all carbuncles. It has not, however, been absolutely universal. A few surgeons, among whom may be mentioned John Pearson in our own country, Von Walther in Germany, and Perrez in France, have been sceptical as to its efficacy. There are also at the present day some surgeons who do not recommend it. Chelius, on the contrary, who appears to have paid some attention to this question, records thus, in strong terms, his conclusions:—"That cutting into the carbuncle is generally neither useful nor necessary, is the most dangerous statement that can be put forth respecting its treatment." Respecting this point, two propositions may, we think, be maintained: 1st. Carbuncles will very frequently get well without having been incised 2nd. The practice of incision, however free, does not by any means universally arrest the carbuncular inflammation. In proof of the former, the fact, that of the cases in our list eight were not treated by incisions, is conclusive. In these, for the most part, the patients came under care when the disease was evidently arrested, and when free openings had spontaneously formed; so that there was no object for resort to the knife. Many others besides these have been under our observation, and, as a general rule, it may be asserted that the destruction of the tissues is far less extensive when the incision has been avoided, than when practised. The consequent cicatrix is of course much less also. The two following cases are of interest, because in them, although under treatment in the stage of advance, yet incisions were purposely avoided.

Case.—A stout pale man, aged 50, by trade a baker, applied as an out patient, at the Hospital for Skin Diseases, with a carbuncle the size of an ordinary cheese-plate in his back. It had been a fortnight forming, and had been attended by great pain. In its centre, the depth of diseased tissues was probably about an inch and a half. Its edges were tense and tender, and the overlaying skin was livid and congested. Several small openings had already formed near its centre. The inflammatory fever, as may be judged from the fact of the man's applying as an out-patient, was not great; the disease indeed, although extensive, was not one of the most acute forms. For several nights the pain had been sufficient to quite deprive the man of rest. Mr. Startin ordered a mixture containing Epsom salts, colchicum, and sulphate of iron, and applied an acid solution of the nitrate of mercury very freely over the extent of about a shilling in the middle of the swelling. Two days afterwards, the man again applied. The disease was much as before, and had scarcely spread. The nights had been nearly sleepless with pain. The acid had merely caused a slough of the diseased skin to which it had been applied. A second application was made to the tissues beneath the eschar, and the general treatment was continued.

At his next visit the man was much better; the pain had all but ceased; the induration of the swelling had much diminished. From this time forwards the cure was rapid. Considering the size of the carbuncle, the amount of slough which came away was surprisingly small, and, as it escaped almost solely by the hole made by the caustic, the scar left was trifling.

The following case, which occurred under the treatment of Mr. M'Whinnie, in St. Bartholomew's, was of much more acute character than the preceding:—

Case.—A woman, aged 30, previously of good health, was admitted on July 15, suffering from a large carbuncle between the angles of the scapulæ. It was attended with severe throbbing pain and much fever. The integument was livid and brawny, and, in its most prominent part, several small ulcerations had already occurred. Mr. M'Whinnie ordered the free application of the acid nitrate over the whole inflamed surface, and prescribed a generous diet, with wine, and the exhibition, twice daily, of a draught containing the sulphate of iron and Epsom salts.

In the course of a few days, the skin, to which the caustic had been applied, sloughed and separated, together with a large mass of cellular tissue, and the base of the sore assumed a healthy appearance. A few days later, an extension of the disease, in a direction upwards, took place, and the woman's health, which had improved, again relapsed. The part now involved was very large, and the inflammation acute. Mr. M'Whinnie accordingly decided to employ the knife, and, at his request, free incision was practised. It must be borne in mind, that the part to which the acid had been put was now in a healthy and healing condition. After the incisions no further extension took place, and the woman made, under the use of tonics, &c., a rapid recovery. When the parts were healed, Mr. M'Whinnie directed our attention to the very great difference in the amount of scar where the caustic had been used, and where the knife. In the latter, the skin was puckered up in a way which, had the part been an exposed one, would have been most disfiguring; while, in the former, the cicatrix was smooth, and comparatively insignificant.

There can be little doubt but that in the first and more successful of these cases the man would have been saved several days' pain by the practice of incisions, but it would have been at the expense of much more sloughing and suppuration. As to the relief of pain resulting from incisions, a glance at the columns of the tabular report will afford conclusive evidence. Almost without exception a free cut across a carbuncle gives immediate relief to pain; and even when the disease spreads afterwards the pain never again becomes as severe as it was at first. The following, as to immediate and perfect relief, is only an example of the ordinary course. Neither it nor either of the above are included in the table.

Case.—William B., aged 67, admitted into St. Mary's Hospital, under the care of Mr. Coulson, on August 9th. He had a carbuncle, the size of the palm of a hand, over the sacrum, which had been three weeks forming. The swelling was tense and hard, and the skin round it of dusky red. He had suffered intense pain. Mr. Coulson made a free crucial incision into the swelling, from which a moderate amount of bleeding occurred. The patient was from that moment most completely relieved of pain, and his recovery was rapid.

The assertion that incisions, even of the freest kind, do not always arrest carbuncular inflammation, will scarcely be held to need proof by any one who has seen much of the disease. If the proof be required, however, we may refer to the fact, that in five out of the twenty-seven cases in which incisions were practised, their repetition was subsequently necessary. Yet granting the occasional spontaneous curability of carbuncle, and the occasional failure of treatment by incisions, the case still stands very triumphantly in favour of the latter. The only drawbacks to their use are the amount of sloughing and the large scar; but to set against these we have the complete relief of pain in almost every case, and the fact that of twenty-seven cases on our list, in no fewer than seventeen the inflammation never spread after the first incision. This latter fact is one of great importance, for it cannot for a moment be held that in so large a number the cessation of the diseased action was simultaneous with the incision treatment by accident. The relation between the two must, we think, be held proven; and the fact becomes more telling when we add to it, that of the six exceptions in which the disease spread, in none did the spreading continue more than five days, and in none were more than three incisions needed. Now, it happens, that a large majority of carbuncles occur in positions in which the avoidance of a scar is a matter of no moment; and in those where it is important—the face for example—the disease is so dangerous to life that in its treatment considerations of personal beauty cannot be allowed much weight.

As to the employment of escharotics, there is this great objection, that if used effectually they must cause at least as much sloughing as the knife, while they do not

much mitigate the pain. It cannot be assumed, that because nitric acid has been applied over a small portion in the centre of a carbuncle, and a cure results, that therefore the acid was the curative means. In all probability, the case referred to is an example of almost spontaneous recovery, as far as local means were concerned. In cases in which the spreading is progressing fast, and the danger to life is great, the treating of the cut surface with an escharotic would probably give much additional security of arrest. In the fearful cases of acute carbuncle affecting the face, such a plan would be well worth a trial. As to the form of caustic, the acid nitrate of mercury is a great favourite with Messrs. Startin and M'Whinnie, at the Hospital for Skin Diseases, and applied with a glass brush, is very convenient of use. At St. Mary's Hospital Mr. Ure has recently employed the potassa fusa for the same purpose.

Are Carbuncles eliminative?—It is well known, that after an outbreak of boils the patient's health often improves remarkably on what it was before. He gets rid of dyspeptic symptoms, of languor, of flying pains, &c., which had troubled him for months; regains a degree of appetite and of general vigour to which he had been long unaccustomed. The same often takes place after several other of the skin affections. They seem to be effective in eliminating something poisonous from the blood; at least such is the *primâ facie* explanation of the effect. Does the same kind of result at all generally follow carbuncle? The patients on whom the preceding observations have been formed, were all treated in Hospitals, and therefore ceased to come under notice as soon as convalescent; we can consequently add nothing as to their subsequent health. Speaking, however, from the experience of a few observed in private, and from the accounts given by patients who were admitted for other diseases, stating that they had formerly been the subjects of carbuncle, we should strongly incline to the belief that much improved health does usually follow that disease. The theory of elimination, however, must not be too hastily accepted. Is it not at least as probable that a carbuncle is simply an effect of a morbid condition, and is produced without reference to any consequent advantage? May not the improvement be fairly attributed to the medical care which a patient with carbuncle receives? In this way most severe diseases are, if the subject recover, actual benefits. A man who has been living inattentively, and in whose blood the elements of disease have been long accumulating, suddenly has an outbreak of erysipelas, of dyspepsia, of gout, or, perhaps, of carbuncle. He fasts, and is purged; his doctor gives him blue pill, and a good lecture on diet, and, when convalescent, sends him into the world again a wiser man, and with a degree of health to which he had long been a stranger. Unless something like proof of it were advanced, we need not here recur to any eliminative influence of the disease for explanation. Authors have mentioned cases in which carbuncle suddenly subsided, and at the same time the patient sank, as proofs that the local disease was exerting a good influence. This is manifestly of little value in proof of such a notion. Phlebitis and pyæmia are known to be occasionally the modes of death in carbuncle, and very probably the cases alluded to were instances of their occurrence. It is well known that the original inflammation often rapidly subsides when pyæmia is established.

Summary.—The following appear to be conclusions deducible from the preceding facts:—

1. The frequency of carbuncle has vastly increased of late years, and still continues to do so.
2. But little is known respecting its predisposing causes.
3. It may affect any age, excepting perhaps the very young.
4. Men are much more liable to it than women.
5. It occurs without distinction, at all periods of the year.
6. It occurs in almost equal proportions among the temperate and well-fed and the intemperate and ill-fed.
7. It has a premonitory stage in a considerable proportion of instances.
8. Its general treatment should be by purgatives and alteratives in all cases, and by stimulants or salines, according to the character of the constitutional disturbance.
9. Incisions are demanded when a carbuncle is spreading, or attended by much pain.
10. In a great majority of cases, free incisions relieve the pain, and in a considerable degree arrest also the spread of the disease.

11. If the spreading and the pain have already ceased, no benefit will be derived from incisions, but the sloughing and suppuration will be much increased.

12. The "core" which separates consists, for the most part, of dead areolar tissue.

13. No proof exists that carbuncle exerts any eliminative influence on the system. —*Med. Times and Gazette, Dec.* 2, 1854, *p.* 567.

93.—*Treatment of Sloughing Sores by Chlorine.*—By THOMAS S. FLETCHER, Esq., Bromsgrove.—[Mr. Fletcher calls our attention to the treatment of sloughing wounds by means of chlorine. The first case is related as follows:]

A man, aged 84, was in August last shot through the palm of the hand, and, in a few days, the wound becoming very offensive, each orifice was covered with large pads of tow, dipped in a weak solution of chloride of lime (two drachms of the powder to a pint of cold water), and the dressing renewed about twice a day. The offensive odour was immediately destroyed, the wound became clean and healthy, the granulations sprung up with great rapidity, soon covering the ends of the fractured bones, and healing up the wound, and this without the use of any other remedy.

I have trusted to the sole use of chlorine in other cases with equal success, applying it not only when the wounds are become offensive, but in the earlier stages, where there was reason to apprehend there would be sloughing or offensive discharges. Various preparations of chlorine have long been used to remove the odour from wounds, and for this purpose they are applied over other dressings; but what I here wish to recommend is that it should be used alone, and as a curative remedy. The *rationale* of its action may perhaps be explained in the following manner:—Portions of the wounded parts are preserved from sloughing by means of the antiseptic properties possessed by chlorine; granulations are to a very remarkable extent promoted—first, by the stimulating power of chlorine, from its increasing the activity of the small vessels on the surface of the wound, and secondly, by the peculiar property it has of keeping the wound clean, and thus enabling the plastic cells formed from the liquor sanguinis to have immediate contact with, and adhere to, active living substance, and so form new healthy tissue, instead of their being thrown off in the form of pus. If chlorine has the power of increasing the process of granulation, it must, as a natural consequence, have in an equal ratio the property of suppressing the suppuration. Lastly, the formation of those noxious gases and larvæ so detrimental to the parts themselves and injurious to the general health, is prevented by the chemical action of the chlorine.

Poultices, ointments, and hot and cold water applications, do not possess the above properties; and if they promote, as I fear they do, what is most favourable to sloughing, heat and moisture, and the confinement of the noxious gases and discharges to the parts, they must in these respects have a tendency to do harm.—*Lancet, Dec.* 9, 1854, *p.* 478.

94.—ON CHRONIC ULCERATION OF THE LEGS.

By FREDERICK C. SKEY, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[Perhaps there is not a more troublesome thing to manage than chronic ulceration of the leg in old people, especially among the poor and hospital out-patients. The following cases are interesting, as showing the wonderful effects of opium:]—

W. G., aged 41, admitted under Mr. Skey into Harley ward, on Nov. 9th, 1854. On the left leg, on its inner aspect, was a large indolent ulcer, with very prominent white edges. He had been troubled with this for several years, and been unimproved by treatment. The ulcer was about the size of a crown-piece. He took the common diet, with a pint of porter daily, and soap and opium pill, five grains, at night. The alteration which even two doses of the pill made in the appearance of the ulcer was as remarkable as it was beneficial. The granulations (before pale, flabby, and extremely unhealthy) were red, small, and covered with a thin layer of healthy pus; the edges, before unhealthy and white, were level with the granulations. This last change was, however, assisted by the application of strapping and

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employed in practice; while its properties, as a sedative, are well known, and are in daily requisition. Its property of mitigating pain and of promoting sleep, is that for which it is almost exclusively employed, and so completely is its action associated with this sedative principle, that its occasional influence as a stimulant is almost entirely lost sight of, and the stimulating property has merged in the supposed sedative. How otherwise can we explain the reasoning of Mr. Pott, who may almost be termed the father of modern British surgery? In speaking of the subject of opium in the treatment of gangrena senilis, he refers its undoubted efficacy to its property of soothing pain. He says, "I have always found that whatever tended to calm, to relax, and to appease, at least retarded mischief, *if it did no more.*"

But, in truth, pain, though common, is by no means an invariable concomitant of senile gangrene, and it is tolerably notorious that opium is a valuable remedy in all cases of this disease. How, then, on what principle, does opium act in those numerous cases of senile gangrene that are destitute of pain, and in which it is an efficacious remedy?

I believe that its sedative properties have little concern with the result. In truth, opium is a most valuable stimulant of the vital powers, and whether its action originate with the centre or the periphery of the circulation, whether primarily on the heart or on the capillary vessels, I do not pretend to know; but there is no drug, simple or composite, known to our pharmacologists that possesses an equal power with opium, of giving energy to the capillary system of arteries, of promoting animal warmth, and thus maintaining an equable balance of the circulation throughout the body. To maintain the balance of the circulation! How much of meaning is attached to these words! How many affections of the bodily frame may not be brought within the range of this definition! Take the common chilblain; what is it but a local congestion of blood caused by defective capillary power?—there is no better remedy than opium; cold feet, as characterizing a person or a constitution, equally relieved; senile gangrene, the result of arrest of the capillary circulation, or its apparent opposite, local hyperemia—these diseases, one and all, manifest a loss of local power, a failure in the balance of the circulation. The term "inflammation," a word formerly in the mouths of our professional brethren on all occasions, is now limited in its application, and should be yet more limited, and I believe, in a yet more advanced state of medical science, will be restricted to an actually rare condition of the system. The influence of opium in such conditions is that of promoting a genial warmth over the system, a glow exactly resembling, and in fact identical with, that produced by the reaction on the system which is caused by the cold-bath. *It is local health*, and the sensation is most agreeable. The benefit derived from opium, when administered for the purpose of arresting inflammatory action of the vessels, admits, I think, of much doubt, and should be resorted to with some hesitation as a remedial agent, though I am quite persuaded that the evil of its administration is greatly overrated. But who will profess ignorance in these days of the inestimable value of this agent when resorted to immediately after an attack of inflammation has been subdued by a local or general bleeding? Here we can imagine that, the activity of the disease being checked, the diffusing influence of opium on the circulation may act as a simple derivative, operating on the vessels at the moment they are not indisposed to yield up their blood, and to which indeed they are compelled by the diffusive power of the general stimulus.

Many years ago, and before the introduction of railway travelling, I was summoned late one afternoon to see a patient some eighty miles from London. I travelled outside the mail. This occurred in the month of December, and the night was extremely cold. By some mistake I omitted to bring my great-coat; and, for the first hour, I suffered a good deal. On reaching a town at some ten miles' distance from London, I took the opportunity, while changing horses, to run across the street to a druggist's shop, where I ordered a draught, containing twenty-five drops of tincture of opium. I believe I was the only person outside the coach that night who did not suffer the slightest sensation from cold.

But it will be urged by many, who have experimented on, and who have observed less than I have done, the medical properties of opium, the infinite importance of studying the reactive effects of this daily poison, and they would inquire into the condition of a person so treated on the following day. You may be assured that it amounts to *nil*. You will, I am sure, readily understand what I mean when I say

the cold and the opium mutually balanced and mutually neutralized each other. There could be no reaction, because the influence of the depressing agent—viz., the cold, rather than otherwise, exceeded in duration that of the stimulant. If the period of prostration were brief, and limited to one or two hours, the argument might hold; but it is but a sorry objection to be urged after all.

I wish I could impress on the minds of the medical authorities in the Crimea the real benefit that might be derived to our noble troops, beaten down by intense cold and suffering in its various forms, from the judicious administration of opium. If twenty-five or thirty drops of tincture of opium, in addition to his ordinary quantum of rum, were administered to each soldier whose nightly services are required in the trenches or on guard, you would hear little complaint of cold for that night, neither would it produce the smallest tendency to sleep. And what do you imagine would be the objection urged against the proposition? "You would destroy the efficiency of the entire army; you would corrupt their morals; you would engender the most enervating habits; they would all degenerate into professed opium-eaters; and, in fact," say the alarmists, "the idea is preposterous." Here again I assert that no possible evil could result; the only sensation, present and future, would be the absence of cold. If cold beget suffering, opium is the antidote to that suffering, and the one will assuredly neutralize the other.

Notwithstanding the prejudices and the bigotry that have long beset the public mind on this subject, and from which our profession is not totally exempt, there is no comparison to be drawn between the practice of dram-drinking and the excessive indulgence in the use of opium.

The man who indulges in spirituous liquors makes daily inroads on his digestive powers not less than on his brain. His appetite is destroyed, and the pabulum for his blood is withheld from his circulation. He is stamped for life, and his perfect health is irrecoverable. The influence of opium, when taken as a means of indulgence, though deleterious, is not permanently injurious. It exercises no serious influence on his digestion or on his cerebral organs, and, the practice once controlled, leaves him in a condition to regain, without difficulty, the fullest vigour of both bodily and mental health.

I have related to you the particulars of several cases of chronic ulcer in which recovery was attributable to the medicinal properties of opium, and almost to opium alone. The character of these ulcers strongly marks the inactivity of their nature, and hence the class of society to which they belong. They are marked by a flat base, which indicates, by its pale, flabby uniformity of surface, that no reparatory action has approached it. It is often surrounded by a thick, high ridge of lymph, covered by unhealthy integuments. The depth of the ulcer, which may be seven or eight lines, is caused partly by the ridge, and partly by the excavation of the ulcer below the natural level of the healthy integuments. So long as this ridge exists, although granulations may form, and will form, from the date of the employment of the opium, yet cicatrization will never complete the process of cure unless the wound or ridge be absorbed. Now the action of opium is not alone exhibited in the development of healthy granulations, but in the entire complement of such actions as are required by the sore, viz., the formation of new material, and the absorption of the old.

The influence of the stimulant is exhibited therefore not on one particular function. It does not merely promote secretion, but it stimulates the healthy vital actions in their entirety—viz., secretion, organization, and absorption contemporaneously; the granulations are secreted and organized, while the circumvallation of unsound material, the product of years of growth, is gradually absorbed and reduced to the level of the surrounding integument; for the removal of this wall is quite as indispensable to the ultimate result as the obliteration of the cavity by granulation. Without the two surfaces be brought to the same level, the process of cicatrization, or skinning over, will never be perfected.

If, therefore, we find that a disease like that I have described, and which exhibits so palpably a dormant condition of the remote capillaries, is amenable to this form of stimulant, which can only accomplish the cure by the substitution of healthy for morbid actions, why should we restrict its employment to this class of diseases? Why, as I have elsewhere inquired, may we not experiment with success on any

local disease dependent on the same cause—viz., an inert condition of the remote vascular system.

In claiming for opium the merit of rousing into healthy action the dormant capillary system, to the end of accomplishing the permanent cure of chronic ulcer of the legs in old persons, I by no means wish you to infer that I consider all other modes of treatment unworthy of trial. Indeed, I attach great value to that recommended by Mr. Baynton, of Bristol, and others; but, having tested their value, I have no hesitation in pronouncing that which I have recommended, so far as I am competent to judge, as by far the most certain and efficacious.—*Lancet, Feb.* 3, 1855, *p.* 117.

95.—TREATMENT OF ERYSIPELAS.

By M. Velpeau, Paris.

[M. Velpeau gives us the result of his treatment of 1000 cases of erysipelas. He seems to place the greatest faith in iron.]

Calling to mind the modifications which the preparations of iron produce in the blood, it seemed to him that a disease so superficially placed, and one in which the inflamed tissues are so imbibed with altered fluids, was well calculated to be influenced by ferruginous preparations. He employed the *protosulphate of iron* in the proportion of 30 *grammes* to the *litre* of water (℥ viiss. ad ℥ xxxv.), or 8 parts to 30 of lard. In forty cases in which this was tried the erysipelas yielded in from twenty-four to forty-eight hours. It is, however, remarkable, that when thus extinguished at its point of departure, it will still spread beyond this, along parts already imbibed with the iron. Whether the inflammation, in order to undergo modification, requires to become fully developed, and whether the remedy is merely curative without being preventive, further researches must show. More easily employed to some parts, the ointment would be preferable; but it is somewhat less efficacious than the lotion. When used, it should be applied three times a-day to the erysipelatous patch, and some way beyond its margin. The lotion should be applied by means of compresses, which are to be kept on with bandages, and wetted every few hours, so as to keep the skin always moistened. Thus far the remedy has never failed in cutting short the erysipelas: but it has a disadvantage in iron-moulding the linen.—*Bull. Thér.*

M. Débout, in allusion to the local application of sulphate of iron in erysipelas, recommended by MM. Velpeau and Devergie, states that the following formulæ are of approved value:—

The Ointment.

Sulphate of iron	5 to 10 parts.
Water	12½ " 25 "
Oil	12½ " 25 "
Lard	70 " 40 "
	100 . 100

The Solution.

Sulphate of iron	10 to 20 or 40 parts.
Water	120 " 110 " 90 "
Glycerine	70 " 70 " 70 "
	200 200 200

Bulletin de Thérap.—Med. Times and Gazette, March 10 *and* 24, 1855, *pp.* 239, 289.

96.—*Treatment of Acne Rosacea.*—[Sulphur has lately been very efficacious in this troublesome and ugly eruption. Mr. Erasmus Wilson's plan is perhaps the best.]

A drachm of camphor is pulverized with alcohol; twice as much of milk of sulphur is then added (Mr. Wilson recommends sulphur sublimatum); afterwards distilled water, to render it sufficiently liquid for use. This lotion is smeared with the finger over the face freely at night, and more sparingly in the morning; the effect is

generally very soon apparent, and is often most striking. The lac sulphuris often contains as much as sixty per cent. of sulphate of lime; this does not appear to interfere with the action of the remedy. This preparation is preferable, on account of its minute subdivision, which makes it less irritating, and also for its whiteness. The use of this lotion should be persisted in for a considerable period if the blotches return; and if the surface be thus preserved in a healthy state, Nature will gradually restore the deeper-seated structures to their normal condition. It does not appear to lose its efficacy by continual use.—*Lancet, March* 3, 1855, *p.* 237.

97.—*Treatment of Condylomata.*—The local remedy usually employed by Mr. LLOYD, in St. Bartholomew's Hospital, for the treatment of condylomata about the anus is calomel. Of this a pinch is taken in the fingers, and sprinkled over the diseased part, the application being repeated once in every two or three days. Mr. Wormald, we observe, uses the grey powder for the same purpose; and in the case of children, the latter is probably the safer application. For out-patients, whom it may not be deemed safe to trust with the calomel, Mr. Lloyd is accustomed to prescribe the following lotion:

℞. Hydrarg. chlorid. gr. viij.; zinci oxydi gr. viij.; mucilagin. ʒij.; aq. ℥vj. Ft. lotio.—*Med. Times and Gaz., April* 14, 1855, *p.* 363.

98.—*Benzin or Benzole, as a Remedy for Animal Parasites.*—Our readers are probably aware that benzin or benzole is a clear, colourless fluid possessed of a pungent etherial colour, which is produced by the decomposition of benzonic acid, or other organic substances, at a light temperature. It was long ago ascertained by Milne Edwards that its vapour was very fatal to insects. This property has led M. Reynal, of the veterinary school at Alfort, to employ it for the treatment of pedicular maladies among animals. He has found that it destroys the parasites in these diseases, more surely, and with more safety to the animal, than tobacco-juice, mercurial ointment, or any other of the many remedies used. It destroys the epizoa without at all injuring the skin.

It is proposed to use this fluid in the parasitical diseases of the human skin, especially in pityriasis or morbus pediculariis, and in scabies, &c.—*Bull. de Thérap. —Dub. Hospital Gaz., Feb.* 15, 1855, *p.* 32.

AFFECTIONS OF THE EYE.

99.—TREATMENT OF STRABISMUS.

By T. SPENCER WELLS, Esq., Surgeon to the Samaritan Hospital.

[A squinting patient goes to his surgeon and wishes to know if he can be cured of his defect. The surgeon answers that he can. But he too often forgets that *two* operations are required, and if he does not state this beforehand, the patient looks upon the first operation as a failure.]

How are you to know beforehand, whether a second operation will be necessary? Nothing can be more simple, if you bear in mind certain rules. You must regard the degree of mobility of the eyes, the difference of visual power, the amount of convergence, and the alternation of distortion on closing one eye and calling the other into action. In some cases you may have but slight distortion, yet the motions of the eyeball are very much impeded. Such a case is not likely to be perfectly cured by operating even on both eyes. In almost all cases you will find a great difference in the visual power of the two eyes. The distorted eye is the weaker. In all the cases we have lately seen, this has been very marked. In one woman, the distorted eye was perfectly useless; she could not read ordinary type at all with it. It only served to render vision indistinct, so that she always closed

it when threading a needle, or looking intently at an object. You take a book, and find that a patient can read with the straighter eye at a distance of two feet, while she cannot read with the other, the straighter one being closed, at a distance of six inches. In other cases the difference is not so great, but there is almost always more or less. Then, as to the amount of convergence: if the distortion be slight, the degree of mobility not diminished, and the visual power of the two eyes nearly equal, you need not operate at all. You can cure the case by making the patient wear prismatic spectacles. I may enlarge on this more hereafter, in the meantime referring to a paper of mine on the subject in the "Med. Times and Gazette" last year. (See "Med. Times and Gazette," vol. vii., p. 216.) If you find the vision of both eyes good, but the degree of convergence considerable, in all probability two operations will be required, and you had better prepare the patient for it. If, with a considerable degree of convergence, you have, as I have said you almost always will have, very unequal visual power in the two eyes, the general rule is, that one operation will be sufficient; but, to be on the safe side, in forming your prognosis as to a second operation, you must determine the power you have of producing an alternation of distortion. You know that cases of strabismus are distinguished as alternating and non-alternating. In the one case, when both eyes are open, the same eye is always distorted. In the other, the distortion is observed now in the right eye, and now in the left, although more frequently in one than the other. In the great majority of cases, by the closing the better eye, the distorted eye becomes straight, and reverts to its abnormal position as soon as its fellow is open again. If you raise the closed lid of the straightened eye suddenly, you will probably find that the eye is distorted, but it becomes straight as the opposite eye returns to its old position. You have produced an alternation of the strabismus by closing the better eye, and calling its fellow into action. You may do the same thing by curing one eye by operation, and, in that case, may have to perform a second operation; and, if the distortion you produced was very decided, you had better prepare the patient for the *probability*. I do not say more, because you will often find that, after a few days, a distortion of the straighter eye, which has supervened after operation, disappears spontaneously. You will find it laid down as a rule in some books, that when the distortion shifts to the better eye, after operation, both eyes should be operated on at once; and some surgeons go so far as to say that if, after having thoroughly divided the faulty muscle in one eye, any distortion is apparent *in either* in looking straight forward, the second eye should be operated on without delay. My experience has convinced me that these are serious practical errors. If you acted upon these principles, you would often perform a second operation quite unnecessarily, and would run into great danger of converting a convergent into a divergent squint, for which the patient would be the reverse of grateful. I have seen more than one case, in the practice of other surgeons, where the internal rectus of one eye has been divided; the eye not becoming straight, the corresponding muscle of the other eye has been cut at once, with the effect of causing divergent strabismus in one case immediately, and in others within two or three days. I would say, therefore, never perform your second operation until you have had time to observe the effect of the first. You would be *tolerably* safe if closing the sound eye still caused the eye operated on to become straight; but it is better to be *quite* safe, and I advise you, therefore, always to wait, rather than run the slightest danger of leaving your patient worse than you found him.

It is curious and interesting to observe how very rapidly the vision improves after division of one of the muscles of a squinting eye. In some cases the improvement is immediate. This was first pointed out to me by the late Mr. Adams, who wrote some papers on what he called muscular amaurosis. I never agreed in his opinion, that compression of the optic nerve by the recti muscles was the cause of impaired vision; for, looking to the anatomical arrangement of the nerve and muscles, I do not see how this compression could be exerted. I think it much more reasonable to believe that the muscle which produces distortion alters the form of the eye-ball, or makes such unequal pressure upon it as to alter the natural relations between the cornea, lens, vitreous humour, and retina, so that the rays of light are not refracted in the same degree or with the same regularity as they are in the healthy eye. Admitting this, we can understand the instantaneous improvement of vision we often observe as soon as a muscle has been divided, and the gradual improvement still

more frequently observed as the eye assumes and retains its normal position.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 460.

100.—*On a simple Method of performing the Operation for Fistula Lacrymalis.* By E. R. BICKERSTETH, Esq.—[It has hitherto been considered essential, that the point of the knife should pass into and be carried along the lacrymal canal, before attempting to introduce the style. Mr. Bickersteth has given this plan up. He says,]

I have ceased to operate as before, and now content myself by making a simple puncture into the abscess or lacrymal sac; and then introducing the style, guide it to the orifice of the canal, and with gentle pressure pass it along the tube. The ease and simplicity of this method of procedure is surprising as compared with the usual plan, and I believe it will be found very generally practicable, and equally efficacious in restoring the canal to a healthy condition. During the last five months, I have adopted this simple means in six consecutive cases of confirmed fistula lacrymalis, with results at least as satisfactory as by the former system. In one case the fistula was of eighteen months' duration, yet the style passed without difficulty, and the patient made a rapid and complete recovery. In another case, the patient—a nervous and excitable female—could not bear the idea of being cut. I therefore insinuated the style along a fistulous opening, which fortunately happened to be nearly over the duct, and succeeded in passing it into the canal without trouble and without pain. She also made an excellent recovery, but an ugly scar remained from ulceration caused by the pressure of the style against the fistulous orifice, which was not quite over the canal.—*Monthly Journal of Med., April*, 1855, *p.* 299.

101.—*Tarsal, or Meibomian Tumour.*—[This is one of the most common affections about the eyelids. Mr. WALTON points out that it is an affection of the meibomian gland.]

Instances of spontaneous cure, either with or without suppuration, are very rare, and an operation is the proper measure. Now, as the nature of the tumour has not been the guide to the operative measures generally adopted, it is not surprising that some of the methods devised are abortive, and some require to be repeated. Dr. Mackenzie, in his work on the Eye, 4th edition, p. 154, says: "As it is unencysted, it is needless to think of a regular operation." Unless, when the tumour is opened, such means are adopted as shall destroy the cyst, a cure is not generally effected; and hence it was, that Mr. Tyrrell, who well knew that there was a cyst, writes, "Unless the cyst be freely broken, the disease is apt to return; but, if well lacerated, adhesive inflammation soon follows." Mr. Walton operates differently, and endeavors to pull away, or dissect out, the cyst, as a more sure method still. The patient under consideration would allow but one of the tumours to be treated, and Mr. Walton passed the blade of a small scalpel through it, and divided the tumour and the integuments at the same time, and dissected away the cyst. The edges of the little wound were brought together with plaster. In reference to this treatment it may not be out of place here to quote a postscript which is appended to Mr. Walton's paper. It is as follows:—"I desire to say, that since I have adopted the pathological views above stated, I have, whenever surgical measures are required, ceased to employ in general the usual method of attacking the Meibomian tumour from the interior of the eye-lid; but, for the most part, that is, when the tumour takes an outward direction, I divide it on the outside, squeeze out the contents, and, when it can be accomplished, pull away the cyst with a pair of forceps, or, if necessary, remove it by dissection. I am enabled to assert, that this process is far superior to the other, insomuch as it is instantaneously effectual. It is not necessary for me to point out the tediousness and uncertainty of the older method. I must add, that there need be no fear respecting the formation of a scar on the eyelid; for, if the incision be made horizontally, and the edges be brought together by a strip of plaster, no trace of the operation is left."—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 593.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

102.—ON THE STATICS OF PREGNANCY.

By Dr. J. Matthews Duncan.

(Read before the Medico-Chirurgical Society of Edinburgh.)

In commencing, Dr. Duncan wished to guard the Society from supposing that in attaching numbers to angles, he wished to imply the existence of real mathematical exactness in the subject he discussed. These numbers merely expressed a near approximation to the actual condition. There were used in order to facilitate the application to the anatomical circumstances so expressed, of mechanical principles which can only be used in an exact way.

1. Position of the Uterus.—In the unimpregnated female, the uterus generally lies with its fundus near the centre of the imaginary plane of the brim of the pelvis, and having its long axis in the direction corresponding to the axis of the brim, that is, an imaginary line drawn at right angles to the plane of the brim from its centre.

The unimpregnated uterus, weighing about an ounce, is so unstably poised in its position, that the common description of it as *floating* there, is an extremely apt use of that term. In accordance with this, the first change produced by the increase of its weight and bulk in pregnancy is, that it sinks in the pelvis. Gradually increasing in size to dimensions greater than those of this bony ring, it rises out of it, and is described as coming to rest upon its brim. But the uterus cannot, without risk of misapprehension, be described as ever resting upon the brim of the pelvis.

It must be remembered that the brim of the pelvis is in the erect, the attitude understood in all such discussions, much nearer vertical than horizontal, being inclined to the horizon at an angle of above 60°, a position in which it is evident it can give no *support* in the ordinary acceptation of the term. If we turn to the body to be supported, we shall find its circumstances inconsistent with the above description. The long axis of the uterus is in all the earlier months of pregnancy inclined to the horizon at about the same angle as the axis of the brim, that is, an angle of about 30° to the horizon. In the latter months, the angle of inclination of the upper part at least, exceeds 30° somewhat. From its nearly oviform shape, with the small end lowest, and the nearly uniform destiny of its contents, it may be predicated that its centre of gravity is nearly the centre of the mass. A necessary condition for the support of any body is that its centre of gravity be supported. This necessary condition is not fulfilled to the pregnant uterus by the pelvis. Consequently the pelvis cannot be correctly described as *the* part supporting the uterus.

The pregnant uterus is supported on every side by the soft and elastic structures which everywhere surround it, as closely as if it were enveloped in a fluid. No single part can be described as *the* part supporting it. But with this qualification, the nearest approximation to a correct statement of the conditions of its equilibrium is the following:—It reposes upon an inclined plane formed by the interior abdominal wall, and is prevented from moving down the plane by the pelvis. To be at rest upon an inclined plane, a body must have a power exerted upon it to arrest its motion down the plane. In the pregnant female, this is the power exercised by

the brim of the pelvis. The power required to resist the pressure down the plane is to the vertical pressue on the inclined plane, as the height of the plane is to its length. Now, in an inclined plane, which rises from the horizon at about an angle of 30°, the pressure on the plane itself must be greater than the pressure in the direction down the plane in the proportion of 87 to 50. Thus it is found that the chief support of the uterus is the extensive, equable, soft, and elastic abdominal wall, not the comparatively limited, unequable, hard, and inelastic brim.

Dr. Duncan would not dwell at length on the application of this in explaining the production of pendulous belly, &c. He would allude only to the pains in the sides of pregnant women, which Montgomery has described as produced by the dragging upon the oblique muscles at their insertions. And the same circumstances he showed to afford an explanation why the gravid uterus had so little tendency to prolapse.

Dr. Duncan then pointed out that great misconceptions were entertained in regard to the position of the uterus in different positions of the female. For instance, in attempting to disprove the gravitation theory of the position of the fœtus *in utero*, it has been argued that that doctrine presupposes that the mother's body is in the vertical position, in order that the gravitation of the fœtal head may have the effect attributed to it, and that the horizontal position assumed during repose, or from other causes, ought to afford many more chances than the statistical results show, of the head assuming, by gravitation, other positions than the ordinary one. This argument, to have any force whatever, must imply that, in the vertical position of the female, the uterus is vertical, or nearly so; and in the horizontal or recumbent posture, horizontal, or nearly so. But this is very wide of the truth; nay, it is totally opposed to it. In the erect position of the female, the uterus is, in all the earlier months of pregnancy, not vertical, but inclined to the horizon at the moderate angle of about 30°. In the latter months of pregnancy, the uterus is generally, though not always, directed to an angle somewhat nearer half a right angle, by the resistance of the anterior abdominal wall to its growing in its earlier direction. In the supine position, we have found the uterus to be not horizontal, but inclined to the horizon at an angle greater than that in the erect attitude of the female. This angle is between 60° and 30°, probably much nearer 60° than 30°. In this way, it is evident that the uterus is more nearly vertical in the horizontal position of the woman, than in the erect. In both cases it is in an oblique position; and by this beautiful arrangement, the uterus is preserved in an uniform condition, free from many statical variations which might otherwise affect it suddenly and injuriously. If the uterus were at any time vertical, then the lower part of the organ, the part best supported and perforated by the os, would be liable to be subjected to the pressure of a column of liquor amnii of about twelve inches in height, or equal to the longest diameter of the uterus. But by inclining the organ from the vertical position, the vertical height of the column of water is diminished, and with it the amount of the fluid pressure on every part of the walls of the organ.

Before leaving this part of his subject, Dr. Duncan would point out one practical application of it. In the third quarter of pregnancy, when the fœtus has acquired considerable firmness and weight, and before it has become at all closely adapted in form and dimensions to the uterine cavity, it may be made, by a very small amount of force, to bob about in the liquor amnii. Various positions of the female have been recommended as best adapted for successful repercussion or *ballottement*, as this bobbing of the fœtus is called. But a little attention to the configuration and attitute of the uterus will show that, on mechanical grounds, there is one position decidedly preferable to the others. This position is that of course where the operation can be best accomplished; in other words, where there is most room and freedom for the motion. In regard to the uterus itself, this is evidently its long diameter, in a vertical direction. Its long diameter offers most space; and if in any other than a vertical position, motion in it would be impeded, not only by the liquor amnii, but also by the fœtus's own friction on the uterus to which it gravitated, and on which it would, to some extent, move. Now, in what position of the female is the uterus placed, with its long diameter vertical? He had already said that the organ developes itself forwards and upwards nearly in the direction of the axis of the pelvic brim in all the earlier months of pregnancy, at least. If, then, we place the brim, which, in the erect attitude, is inclined to the horizon at an angle of 60°, in a horizontal position, then the uterus will be vertical. For this purpose, the

axis of the female, or at least the trunk, must be deflected to the same amount from the vertical line; that is, so as to form an angle of 30° with the horizon. This can easily be effected in actual practice, by propping the female with pillows on a couch.

II. Position of the Fœtus in Utero.—To give point to the description of the position of the fœtus, he would first state that it is often described to carry its body vertically, but with the head placed undermost; in other words, it is said to stand upon its head. To show that these descriptions are entirely wrong, it is merely necessary to observe that the fœtus at the full time is closely adapted to the cavity of the uterus, and to recall to mind what has been already said in regard to that organ. It will thus be observed that, in the erect and supine position of the female, the fœtus, near the full time, lies obliquely to the horizon; that in neither attitude is it at all nearly vertical or horizontal, but in a position about midway; and that in the erect position of the female it is more nearly horizontal than in the supine. The mature fœtus is nearly horizontal in ordinary circumstances, only when the woman lies flat on the side. It is vertical, only when the trunk is inclined to the horizon at an angle of about 30°.

From this position of the adult fœtus in the axis of the brim of the pelvis, several minor advantages might be pointed out as accruing to itself. But Dr. Duncan would merely observe that this fact is an important element in the mechanism of parturition; the fœtus being thus placed in the direction most favourable for the uterine efforts to overcome the most important mechanical difficulty of labour, the passage of the brim.

In the same way, as the nearest correct explanation of the equilibrium of the gravid uterus lies in the mechanical principles of the inclined plane, so these are similarly applicable to the case of the fœtus. As the uterus derives support on all sides from the organs environing it, so the fœtus is much more perfectly supported by the bland and warm liquor amnii, in which it neither *sinks* nor *carries* itself, but reposes, floating in almost perfect equilibrium. It is, however, specifically heavier than the liquor amnii. It therefore sinks in this fluid, and it does so with an almost unappreciable force. In accordance with the doctrines of the inclined plane, this minute force is subdivided between the plane on which it lies, the anterior wall of the uterus, and the brim of the pelvis, the latter bearing the smaller share. The same hydrostatical circumstances of the floating fœtus which relieve the uterus almost entirely from the pressure at particular points of a body which might obstruct its circulation, or otherwise injure it, do also, in the same degree, relieve the child from pressure which might be painful or irritating to it. By this beautiful arrangement, also, the cord is safe from liability to pressure which might be fatal to it. Again, the protection (says Dr. Arnott) given to the tender fœtus by the liquor amnii is such, that a blow from without is expended on the surrounding water and cannot reach the fœtus. During labour these same waters perform important functions; which belong, however, to hydrodynamics, not to hydrostatics.

It is here necessary to consider the statements of M. Dubois and his numerous followers, in regard to the position of the fœtus *in utero*, when not at or near the full time. These statements are founded upon statistics of the presentations of premature children in the course of miscarriage or abortion. The statistics show that the earlier the fœtus is expelled from the uterus, it is the less likely to present the head as the first part born. But, by an unwarrantable and quite unsupported step, they have been represented as proving the position of the fœtus *in utero*. These statistics are valuable, simply as indicating the presentation not before but only during parturition of premature children; and if such is the case, they are utterly valueless in the discussion of the question of the position of the fœtus before labour, or of its causes. The presentation in early labour of the fœtus, at or near the full time, is a very exact and trustworthy indication of its position before labour; and in many cases we can easily verify the identity of the presentation in labour with the part lying lowest before labour. But we have no evidence whatever that the presentation in a miscarriage or abortion is identical with the part lying lowest before miscarriage; and we have no means whatever of reasoning from the one to the other. On the contrary, the more round and globular shape of the uterus in the middle months of pregnancy, the non-adaptation of the child to the uterus, the great mobility of the child, and the greater abundance of liquor amnii in these months, are some of the mechanical elements in these early stages of pregnancy, which render it

almost absurd to reason from the presentation of a premature child during birth to its position in the uterus, before the process of its expulsion commenced. There are no collections of ascertained facts in regard to the actual ordinary position of the fœtus *in utero* in the middle months of pregnancy. We can only reason in regard to it, from comparing the position of the fœtal heart with the shape and site of the uterus, from feeling in some cases the position of the limbs or of the head *in utero*, from the questionable results of *post mortem* examinations in these months, and from the statical knowledge which we can bring to bear upon it *à priori*.

Another specious argument, adduced against the gravitation theory, requires only a few words to show its imperfection. If gravitation (it has been said) were the cause of the normal position with the head lowest, then this position should be found with more certainty when the gravitation of the head, from any cause, was proportionally greater than natural. In cases of intra-uterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is excessively so. But this increased gravitation (it is added) does not render head presentations in these cases more common than usual, but the very reverse. Here the altered circumstances of a hydrocephalic fœtus have been altogether misapprehended. The hydrocephalic head, although truly much larger and heavier in air, is probably lighter and more buoyant in water. In this question we have to do with hydrocephalic children only while immersed in liquor amnii. The larger the head is in these circumstances, the lighter and more buoyant it is; and the other extremity of the child is proportionally heavier. The fluid effused in hydrocephalus is specifically lighter than brain, and therefore renders the head more buoyant than it is under natural circumstances. It is also in all probability specifically lighter than liquor amnii, and consequently its accumulation will have a more decided effect in elevating the head. In this way it is demonstrated that the four times greater frequency of preternatural presentations in cases of hydrocephalic children, proves rather than disproves the influence of gravitation in deciding the position of the fœtus.

Dubois and others have attempted to show that the position assumed by the fœtus when immersed in a fluid, or rather when allowed to fall through a mass of water, is inconsistent with the gravitation theory. It has been shown, in these ill-devised experiments, that the part of the fœtus which reaches the bottom of the vessel first is not the head, as these authors fancy should be the case if the gravitation theory were correct, but is the shoulder. But the fact is, that the line of the descent through a fluid of nearly equal specific gravity with the body descending is, in the case of the fœtus, as in all other bodies, not a vertical line in the long axis of the body, drawn from what is the absolutely heaviest part, but a vertical line from the centre of buoyancy to the centre of gravity. The same fact is expressed in the hydrostatical principle, that a body floating and immersed is in stablest equilibrium when the centre of gravity is vertically below the centre of buoyancy—*i. e.*, the centre of gravity of the fluid displaced. These very experiments, and numerous other facts, show that the centre of gravity is much nearer the cranium in the fœtus, and especially when in the fœtal attitude, than it is in the adult; that it is, in fact, near the upper part of the back, and nearer the posterior than the anterior surface of the child. The centre of buoyancy of the fœtus will be very nearly the centre of the oval figure which encircles the fœtus. Both these centres will be liable to constant variations with the varying dimensions and specific weights of different parts of different fœtuses, and with the varying attitudes of the parts of the same fœtus. But allowing for every variation, it is evident that if these two centres be in the case of the fœtus placed in the same vertical line, the fœtus will be inclined obliquely to the horizon, as we find it to be in its ordinary position. The experiments of Dubois, involving, as they do, rapid motion in falling through water, are fitted to afford results of a very small degree of reliability. So far as they go, they point to the fact of the descent of the fœtus, shoulder lowest; or, to speak more correctly, following the shoulder. In an experiment Dr. Duncan lately performed with a fresh still-born adult fœtus, retained by threads in the fœtal attitude, he found it to poise itself in an oblique direction, head lowest, in a solution of salt of nearly its own specific gravity. This position evidently corresponds closely to the ordinary uterine position.

This argument against the gravitation theory, it thus appears, has been quite misapprehended; and a more correct investigation of the circumstances reveals to us here, as everywhere else, the beautiful arrangements of nature, whereby the fœtus

reposes in its sojourn in the womb in that position which is easiest and securest to the mother and to itself—in a position, indeed, which requires no effort whatever on its part for its adoption and maintenance.

The only other objection to the gravitation theory, which is deserving of mention, is founded on the fact, that dead or putrid children present the head as the lowest part in labour less frequently than the living; and it must be admitted, that the death of the fœtus does not remove it from the agency of gravitation. From these propositions the conclusion is jumped at, that mere gravitation cannot account for this difference in the ordinary position of the putrid and the living, for it is evident that gravitation should affect live just as it does dead matter. But here it has only to be pointed out that the major proposition of the syllogism is an assumption, and that this argument is a reasoning in the dark. The assumption is one very highly improbable, namely, that a dead, or rather putrid child, is in the same condition, *quoad* gravity, as a living one. We know that in the adult, at least, the circumstances of death and putridity produce great changes *quoad* gravity. What the changes are in the fœtus in similar circumstances we do not know. Till we do know, we must be content without attempting to reason as if we did.

Before proceeding with correctness to argue from the fact of frequent malpresentations of dead children to the causes of the ordinary presentation of the head, we must take carefully into account not only the new circumstance of death in these cases, but also all other circumstances in pregnancies with dead children, which are, or may be, statically different from the circumstances of healthy pregnancy. Among these are prematurity, quantity of liquor amnii, the shape of the uterus, and the shape of the fœtus. Dr. Duncan had already said that those who use this argument have not directed their attention even to the altered statical relations produced by death and putridity of the fœtus.

All the knowledge we possess of the position of the fœtus, after it has entered the second half of pregnancy, leads us to believe that its head ordinarily lies lowest. Before the seventh month, it is still capable of having its position *in utero* changed, by changes merely in the attitude of the mother, and probably it possesses the power of effecting temporary changes, at least, by its own unaided movements. But the fœtus is generally in a state of repose, and not producing motions in its limbs or body. In this state of repose, in a fluid of nearly its own specific gravity, it is impossible to conceive its maintaining any position but under the influence of gravity. Its position must at all times be mainly, if not entirely, caused and determined by statical circumstances. It is quite conceivable, that while still comparatively free in the uterus it may, by virtue of its very easy mobility in the dense liquor amnii, change its position. If this occur at a time when its dimensions are beginning to approximate to those of the uterus, having overcome some resistance of the uterine walls by the force of its own muscular efforts, or otherwise, as by accidents to the mother, it may not gravitate back to its old and ordinary position; and thus a preternatural presentation may be produced. But the same circumstances which give great comparative force to the muscular efforts of the fœtus, render its position in the same degree difficult of retention, and will tend always to subject it to the influence of gravitation. The uterine walls are everywhere smooth and glabrous, and rounded; and the fœtus lies in this cavity with its legs elevated; circumstances which appear to render its maintenance of any position but that of gravitation a greater feat than ever was performed by a rope-dancer. With all the advantages of its new circumstances, the child after birth cannot assume or maintain any position. How much less could it be expected to do so in the uterus, and under circumstances so disadvantageous.

III. Position of the Pregnant Female.—The researches of Weber have shown, that in the erect attitude of both sexes in ordinary circumstances, the body is accurately balanced upon the two ilio-femoral articulations, the atlanto-occipital and sacro-lumbar articulations being in the same transverse-vertical plane with the ilio-femoral articulations; the knee-joint and ankles being also in it. This adjustment and balancing of the trunk upon the ilio-femoral articulations must, in the impregnated female, be gradually changed more and more till the full period of pregnancy arrives. At this time the weight of the entire gravid uterus and contents is superadded to that of the anterior parts of the body, which in the virgin state balanced the posterior parts upon the ilio-femoral articulations, the fulcrum of the pelvic lever.

To restore the balance, a corresponding amount of weight must be added to that upon the posterior arm of the pelvic lever. The weight added in front is probably about from twelve to fourteen pounds avoirdupois; and to restore the equipoise a corresponding amount of the upper part of the trunk and of the head must be moved backwards, so as to increase the weight behind. These theoretical observations must have been frequently confirmed by every observant eye in actual life. It is then to be remarked that the protuberant abdomen is rendered apparently more so by the attitude of the woman being as if excessively erect. To the necessity for this new adaptation, the increased anterior development and weight of the mammæ will contribute. In small, and especially short-bodied women, this change is most evident; a circumstance easily explained by the greater prominence forwards in such of the uterine tumour, and by their not being able otherwise to adapt the trunk to the new circumstances, as can be done with greatest facility in tall or long-bodied women. Some few women, especially those last alluded to, are enabled to conceal to a certain extent their gravid condition by not assuming this change of attitude, nature securing by a different mechanism the equipoise of the trunk upon the lower limbs. In the mechanism of the erect position, the centre of gravity must be vertically above the ilio-femoral articulations, or points of support. In the virgin, this centre resides in the lower part of the lumbar division of the spinal column. The addition of the weight of the gravid uterus and contents, in front, would move this centre forwards. This is prevented, however, in most cases by the corresponding motion backwards of the upper portion of the trunk. In some women, either from voluntary or involuntary causes, this last motion does not take place. The equipoise of the body upon the ilio femoral articulations, or restoration of the centre of gravity to its position vertically over these points of support, must be otherwise effected. This can evidently be done only by moving forward the supports, so that they may be vertically under the new site of the centre of gravity, in this class of pregnant females. Put as the acetabula are fixed bony sockets, this can be done only by diminishing the angle which the pelvis forms with the horizon. In this diminution of the obliquity of the pelvis, the forward movement of the ilio-femoral articulations takes place, and the equipoise is restored without any backward motion of the upper part of the trunk. This change in the pelvis is analogous to that taking place in old age, when the forward stoop is counterbalanced by it. The same is also observed in youth, when a forward stoop is produced, as in the course of some affections of the vertebræ. Dr. Duncan had already pointed out that the attitude of the gravid uterus is inconsiderably changed by the woman shifting from the erect to the supine posture, or *vice versâ*. But though this be the fact, as regards the mere attitude of the uterus, yet great changes take place in it, in regard to its liability to vascular congestion, and other phenomena connected therewith. When the woman is recumbent, the blood is easily returned to the heart from the uterine sinuses, and there is little danger of vascular engorgement taking place. When the woman stands the return of blood is not so easily effected, it having now to overcome the increased resistance involved in the change of the position of the vena cava from being nearly horizontal to being nearly vertical. Of this circumstance clinical illustrations abound in the practice of obstetrics. A woman of delicate fibre, and easily made to miscarry, may require to maintain the horizontal position for the whole, or a part only of the period of pregnancy. In some such cases the assumption of the erect attitude for the shortest time induces engorgement of the uterine sinuses, separation of the placenta, hæmorrhage, and abortion, or miscarriage. Hemorrhage, again, is induced in some cases of ulceration and polypus, only by continuation in the erect attitude; in the same way colour often reappears in the discharges after delivery, on the first getting up; and all uterine hemorrhages are aggravated by the erect attitude. To the same cause also are to be attributed many inflammatory affections of the uterus, and many of the uterine pains attending disease in this region.

Dr. Simpson had some years ago taken pains to refute the theory brought forward by Dr. Duncan, and which, notwithstanding the very interesting and able paper just read, he held to be incapable of meeting the many objections which might be urged against it. As the subject was a very wide one, he (Dr. Simpson) would begin with the last statements in Dr. Duncan's paper, viz., those with reference to the influence of position in restraining uterine congestion and hemorrhage, obviating their effects. In his opinion, Dr. Duncan had overlooked one very

important circumstance, viz., the absence of valves in the vena cava and uterine veins, a most important item, as it was this that allowed the blood to gravitate without opposition. In lecturing on these effects of position and gravitation, such as Dr. Duncan had given them in his paper, he (Dr. Simpson) had always taught that this element, the absence of valves, was the primary and essential element in the consideration of the whole subject. As to the position of the fœtus, in a paper which he (Dr. Simpson) had laid before this society some years ago, he had advocated the influence of excito-motory or reflex action as the kind of vital mechanism by which the fœtus assumed and maintained its position. At that time he felt himself at a loss to explain some of the *residuary* phenomena, he believed that perhaps the old doctrine of gravitation might so far explain them; but as yet he was not convinced that the gravitation theory was the only key to the solution of the whole question. For example, in every 100 cases of head-presentation we had twenty-eight or twenty-nine in the third position of Naegelé, where the back and occiput of the child, instead of lying to the front of the uterus, lay to the back of it, and the position thus assumed was the very opposite to what we should expect on the gravitation principles laid down by Dr. Duncan. And that this position was not assumed by the child only a few days before delivery we had the surest evidence, as auscultation showed that it was maintained for weeks before. In Hunter's celebrated plate of his supposed natural position of the fœtus, which Dr. Duncan had exhibited to the society, we had the long axis of the head of the child lying in the right diagonal with the occiput anteriorly. This was a very rare position of the head, as Naegelé had only met with it in one in 1200 cases; but if gravitation were the sole determining cause of the child's position, we ought to meet with it as frequently as we meet with the head in the opposite diagonal diameter. The true reason for its rarity, Dr. Simpson believed, lay in the fact of the rectum occupying that diagonal, and the child in consequence was compelled by reflex action, to displace itself and take up its position in the other diagonal of the brim; the position assumed by the child being always dependent on the freedom with which it can place itself so as to be least incommoded by the irritation of such pressure and agency as were calculated to excite reflex motions in it. Again, we must remember, that the child swims in a fluid of nearly equal specific gravity with itself, and in which it moves with the facility of a fish in the water. Continuous motion never tires it, in accordance with the proposition laid down by Marshall Hall, that for voluntary muscles rest is required, but that the "spinal system never sleeps." Dr. Duncan's argument as to the death of the child not influencing its position, was founded in his (Dr. Simpson's) opinion on incorrect data. With living children the proportion of breech presentations was about one in sixty; but when the child had been dead for some days before birth, the proportion of these presentations was one in six. Hence the position assumed by the child was not a physical act, but one connected with life; it was lost with the loss of life, and was hence a vital act to that extent. But all the vital acts of motion are dependent on muscular contractions, and muscular motions in the fœtus are entirely reflex and spinal, not volitional and cerebral. Dr. Duncan had attempted to account for this frequency of breech presentations by stating that the statics of the child had been altered by the decomposition which had taken place after death. We must remember, however, that in the decomposition of the child the first change observed was an enlargement of the abdomen, which ought rather to maintain the head downwards than force the pelvic extremity upwards. In fact after death it was subject to statical laws alone, and hence its new position. As to the argument from hydrocephalic fœtuses, Dr. Simpson still regarded it as a strong one in favour of the true doctrine not being gravitation. We had two fluids, the hydrocephalic and the liquor amnii, of nearly similar specific gravity, but Dr. Duncan forgot that the weight of the cranial bones from the enlargement of the skull had greatly increased; and yet, notwithstanding this increase in the weight of the cephalic extremity, instead of the proportion of presentations of the head being ninety-six in every hundred cases and the pelvic presentations one in sixty, we had the pelvic presentations occurring one in every six cases of hydrocephalus, or ten times more frequent; and the cephalic presentations correspondingly reduced in number. Dr. Simpson could not regard Dr. Duncan's observations on Dubois' experiments as very accurate. At the sixth month, Dubois found that the child presented equally

often with its pelvic and cephalic extremities; and he (Dr. Simpson) held that the presentation shown by a fœtus when the os uteri opened was the best existing test of the former position which it had occupied in the uterine cavity before the os did dilate. There were certain cases, however, which in his (Dr. Simpson's) opinion decided the question. He referred specially to cases where there were twins, and those in which there was a recurrence of malpresentations in single births, and where the uterus, instead of its usual ovoid, was of a more or less irregular form. Were the position of the child solely dependent on gravitation, this irregularity of form should not interfere with the physical action of gravitation, and yet we found the frequent recurrence of preternatural positions in such cases, conclusively showing that gravitation could not account for them. The child resembled a decapitated frog, which to escape irksome pressure shifts its position independently of any act of gravitation, and adapts itself to the figure of the containing cavity. Lastly, Dr. Duncan had misstated the argument from the natural position of the woman and from the effect of the reclining posture. Velpeau had, many years before, used the same arguments; we know, however, that women have been confined to the horizonal posture during their entire pregnancy, and yet the head presented. In such cases the child must have inclined to one side or the other as the uterus shifted, and the fundus uteri thus being lower than the os, and, accordingly to the reasoning adopted by Dr. Duncan, we should have had the pelvis and not the head the presenting part. Granting that in the erect position and in the reclining position, when the mother lay on her back, the uterine walls formed an inclined plane, with the os as the lowest point and the one towards which the head would gravitate, yet we know equally well that when the pregnant woman lies during sleep on one or other side, as she usually does, the fundus uteri is considerably lower than the os uteri, the inclined plane of the uterine cavity is reversed, and the child's position, if dependent on gravitation alone, would be nightly liable to change. The subject of position of the fœtus was, however, a very extended one, and would require much more time than could be given to it at such a meeting. He was not aware from the title of Dr. Duncan's paper, that it was to treat of fœtal positions, and what he (Dr. Simpson) had now stated was, perhaps, not so relevant to the point as a little consideration of the subject might easily make it.

Dr. DUNCAN, after referring to the additional explanation of the increased liability to vascular uterine congestion in the erect attitude, from the absence of valves in the veins between the heart and the uterus, said he had only a few words to say in answer to the points taken up by Dr. Simpson. The latter had referred to the researches of Naegelé, which showed that the fœtal head entered the pelvis in a considerable proportion of cases with the face lying forward, or anteriorly, instead of the occiput as was most frequent. Dr. Duncan could not see that this affected any position taken up by him. For he did not think that the position of the head in early labour was any certain indication of its position before labour began. The position of the head in the pelvis, in early labour, was determined, as Dr. Simpson said, by the shape of that bony cavity. Now, Dr. Duncan's researches had reference exclusively to the position of the fœtus before labour, and before its head entered the pelvis at all. Moreover, all he wished to show was, that the position of equilibrium of the floating fœtus was an oblique one as in the human female. His researches did not enable him to say whether in this oblique position the child should float with back lowest or one of the sides. Peculiar attitudes of the uterus or fœtus, or the position of the liver, might explain the positions of the fœtus *in utero* with its sides or front anteriorly. These questions had to be decided before any argument against their production by gravitation could be admitted. Dr. Simpson had said that the spinal system never sleeps, and that the reflex nervous actions and resulting motions might go on so as to cause positions of the child. But Dr. Simpson forgot that he knew that the child was generally in a state of repose, not moving, and consequently that any argument about the possibility of constant, unwearied movements in the fœtus, was a vain argument, seeing we knew that such motions do not exist. Moreover, the fact of the fœtus being in a fluid, while it gave the child great power of motion, gave it also great instability. A man has great power of movement when immersed in water. But he was also excessively unstable and had little power to resist movement. In regard to the frequency of malpresentations of dead children, Dr. Duncan again pointed

out that we did not know the altered statical circumstances of dead children. We must wait to ascertain these before we can decide whether their frequent malpresentation is the result of loss of life and power of motion, or of such possible altered statical circumstances. One fact was worth a cartload of arguments. Dr. Duncan had one to adduce. He had flôated in a saline solution of its own density, a fœtus of the sixth month, which was born dead, and with the skin peeling. It floated head highest. In labour it presented the breech. This was the only fact bearing on the question which he or Dr. Simpson had to adduce, and it certainly told against Dr. Simpson, and in favour of gravitation. The death and putridity had so altered it, that it floated in the opposite way from a fresh child. Dr. Simpson spoke of the probable effects of swollen abdomen in putrid children. But this kind of hypothetical and assumptive reasoning Dr. Duncan thought unworthy of even getting any place in the question. Again, in reasoning about the uterine attitude of hydrocephalic children, Dr. Duncan did not wish to force any conclusion from the circumstances he stated. He merely destroyed the argument of Dr. Simpson and others, derived from the frequent malpresentation of hydrocephalic children. We must await the results of actual experiments to decide the position of equilibrium of floating hydrocephalic children. Till such experiments were made, neither he nor Dr. Simpson had any right to argue from suppositions as to what such experiments would show. The increased weight of skull in these cases would, in Dr. Duncan's opinion, be probably counterbalanced by the increased density of the rest of the child's body, the lighter parts being removed by emaciation or atrophy. Dr. Duncan referred to the cases of recurring malpresentations as being exactly the kind of cases from which no arguments could be drawn, till we knew the whole circumstances of such cases. The one circumstance of deformed uterus producing these results, Dr. Duncan demonstrated, could be more easily explained by gravitation than by motions of the child. In alluding to the position of the pregnant female on her side, as making the uterus horizontal, Dr. Simpson forgot that then the fœtus was on its side as well, and *de facto* in a position of stable equilibrium, not liable to move. Finally, Dr. Duncan wished it to be understood, that he was not discussing the whole subject of the position of the *fœtus in utero*, but only its statical relations, leaving its own motions out of view. These statical relations, however, threw light upon, and explained in an easy manner many phenomena which Dr. Simpson and others had referred to reflex actions in the fœtus. Dr. Duncan was sure the Society could not fail to see that if the Newtonian law was followed, of choosing always the simplest explanation of phenomena, the gravitation theory would, so far as he had carried it, have an easy superiority over any other that could be adduced.—*Association Med. Journal, Jan. 5*, 1855, *p.* 16.

103.—*Use of Uva Ursi instead of Ergot of Rye.*—Mr. HARRIS relates cases in which he has employed a strong decoction of uva ursi in accouchements, where the ergot of rye would ordinarily have been employed, and found its employment followed by vigorous pains, which soon caused the expulsion of both fœtus and placenta. Mr. Harris prefers this medicine to ergot of rye, inasmuch as it does not cause such strong contractions as the latter, which are so very painful to the mother and dangerous to the child.—*Virginia Med. Jour.—Monthly Journal of Medicine, April*, 1855, *p.* 346.

104.—*Uterine Douches in Tedious Labour.*—Dr. BOURGEOIS believes that the tepid douche, so successfully employed by Kibisch and others, in the induction of premature labour, is susceptible of beneficial application in a much wider field. Thus, he has used it with great advantage in simple inertia of the uterus; a single douche, of a quarter of an hour's duration, often sufficing to arouse the pains immediately and effectually. Cases of rigidity of the uterus, met with in too young or too old women, delivered for the first time, or produced by a premature rupture of the membranes, are treated in the same manner with success. It is also advantageously employed, but with less certainty of success, in spasmodic action of the organ, which is so distressing a cause of delay.—*Gaz. des Hôp.—Med. Times and Gazette, March* 31, 1855, *p.* 317.

105.—*On Injection into the Uterus in cases of Flooding.* By Dr. Septimus Wray, Brixton.—It is now many years since I first made trial of a practice to which the present communication refers, and as I have every reason to believe that it has been the means of preserving life in several instances, I now feel justified in recommending it most strongly to the profession. Many are the plans that have been proposed for the arrest of hemorrhage after delivery. In these fearful cases we have been advised to use cold affusions, to inject cold water into the vagina, to compress the uterus externally, to introduce the hand, to give astringents, stimulants, &c. Amongst all these recommendations, the plan, to my mind, the most simple and direct, and, in my practice, by far the most effectual, has been omitted. I allude to the injection of cold water *into the cavity of the uterus itself.* By so doing a stream of cold is secured to the open mouths of the vessels themselves; all clots, &c., which prevent closure are washed away, and the organ, to a matter of almost absolute certainty, is stimulated to contract. There is neither difficulty nor danger in the procedure. I use a Weiss's syringe, and introduce the nozzle an inch or two fairly into the os uteri, holding it in position whilst an assistant works the piston. A common stomach-pump, or enema syringe, will serve the purpose well; and with those fitted according to the new plan with an elastic propeller, the accoucheur might manage the whole without assistance. If, however, his syringe require two hands to work it, he will require help, as it is absolutely necessary that one hand be employed within the vagina in keeping the nozzle of the tube in place. The stream may be kept up as long as deemed necessary; and, if the case be very urgent, it might be well to use vinegar and water in equal parts, instead of water only. In one protracted case of hemorrhage I used a solution of lunar caustic, in the proportion of three grains to an ounce of water, with perfect success. The old plan of injecting into the vagina is often ineffectual, there being, indeed, no security that any of the water ever enters the uterus itself; whilst that of cold affusion to the abdomen is yet more remote in its mode of action. Having been for nearly forty years in extensive practice, I speak from considerable experience in these matters. I am very sanguine that, were the expedient I recommend in general employment, life need almost never be lost from the distressing accident referred to.—*Med. Times and Gazette, April* 14, 1855, *p.* 361.

106.—*Fecundation.*—Dr. Martin Barry has repeated all his former experiments as to the ovum, and though denied by Bischoff, Wagner, and various other Germans, the English physiologist proves to have been true from the beginning. He has recently shown spermatozoa in large numbers in the body of the ovum.—*Lancet, Jan.* 6, 1855, *p.* 25.

107.—BLENORRHAGIA OF THE EXCRETING DUCT OF THE VULVO-VAGINAL GLAND.

By Dr. Alph. Salmon.

The author directs attention to the affection very common among prostitutes, yet little known to the generality of practitioners, namely, purulent hyper-secretion of the excretory duct of the vulvo-vaginal gland, a disease first pointed out by M. Huguier, and described by him as the occasional source of blenorrhagia in the male. It is now universally known, that there exists on each side of the vagina, at the orifice, and imbedded in the labia, a glandular body, the duct of which, half an inch long, opens just at the base of the hymen, or by the caruncule myrtiformes. During erotic excitement, a viscid fluid, similar to the prostatic fluid in the male, is abundantly poured forth to lubricate the female external organs. That the duct of this gland may become the seat of one variety of blenorrhagia is shown by the following cases:—

A girl, aged 18, named Heloise, came from Paris for the purpose of prostitution, and was immediately subjected to medical inspection. There was nothing morbid in the urethra, the vagina, the excretory duct of the gland, nor in the anus, but there was a slight ulceration of the neck of the uterus, which the author cauterised; he then kept the girl in the Hospital some days. She was subsequently re-examined,

pronounced sound, and discharged. There was the same freedom from disease upon two successive examinations, at intervals of ten days; but shortly after the last visit she was marked as having infected a young man of respectability with a profuse blenorrhagia. Dr. Salmon examined her with great care, but found no disease in the urethra, vagina, or anus. Upon investigating the condition of the vulvo-vaginal gland, he noticed, upon pressing it with his finger from behind forwards, from the ischium towards the caruncule myrtiformes, that there flowed from the orifice of the duct a moderate quantity of thick yellow pus; there was also a well-marked increase of volume in the part. This condition remaining unchanged for several days, the author injected a solution of nitrate of silver into the gland by means of one of Anel's syringes. No improvement ensuing after a fair trial, the orifice of the duct was cauterised by the tincture of iodine, applied by means of a fine bougie. This plan was continued for four days; the discharge became gradually less, and the patient was discharged cured 25 days after her admission.

A girl, named D., had been living in a reception-house at Chartres 15 days. She came from the environs of Rambouillet. A soldier pointed her out shortly as having infected him with blenorrhagia, when she was subjected to examination, and found to be suffering from this inflammatory condition of the vulvo-vaginal gland. After a visit of a fortnight to the Hospital, during which time she was subjected to proper treatment, she was dismissed cured. Several other similar cases are recorded, from the consideration of which the author arrives at the following conclusions:—

1. Blenorrhagia of the duct of the vulvo-vaginal gland is more common among young prostitutes who have just commenced their mode of life, than among those who have been long accustomed to it. All the cases recorded by the author were those of young women who had recently been admitted into the receiving-houses.

2. This form of blenorrhagia mostly affects the left side. Out of eight patients, the left gland was affected in six cases. The orifice of the duct is more easily observed on this side. The reason for this preference is not clear; for one cannot accept the explanation of Huguier, that it is due to pressure upon the left iliac vessels by the sigmoid flexure of the colon distended by fæces.

3. It is written by authors who have treated of abscess of the labia majora, that excess of coitus is often a cause of the malady. This statement is verified by the fact, that the house which rendered the greater number of the cases related in Dr. Salmon's paper was much frequented, and its inmates, mostly young, subjected to frequent and daily intercourse.

4. Blenorrhagia of the duct of the vulvo-vaginal gland is very common. It may exist without the coincidence of blenorrhagia of the urethra and vagina; it becomes the cause of an analogous affection in men. Eight clear cases in verification of this statement have been witnessed in the town of Chartres. In all, this form of blenorrhagia of the female existed alone; in two cases, attention was first directed to the girl by the complaint of the men who had become infected.

5. It is most important that medical men should turn their attention to this point in the examination of females.

6. The signs by which the disease may be known are derived from examination only. The woman experiences no inconvenience, nor does she think that she needs medical aid. Moreover, she may, if she pleases, conceal her disease, by making water, or by using as an injection a strong solution of alum, shortly before the visit of the Inspector. This discharge of pus may be easily overlooked, owing to the narrowness of the duct. It is necessary that the surgeon should first make moderate pressure of the labia against the rami of the ischia, by the thumb applied in front of the anus, that it may be ascertained whether the gland be tumefied. In the natural state, it cannot be detected by the touch; if swollen, it feels like a rounded body, the size of a nut, or larger. Firmer pressure made against the ischium from within will cause the contents to escape. The normally constituted fluid is thick and clear, and appears at the extremity of the duct in not larger quantity than a drop or two. The fluid from the inflamed gland is either thick, more abundant, and mixed with mucus, when the girl should be put under surveillance; or yellow and puriform, when she should be removed to an Hospital.

7. The treatment resembles that of blenorrhagia in other situations. The result is much more rapid. Absolute rest; injections of nitrate of silver, with Anel's syringe; cauterisation with the tincture of iodine; baths. The duration is about

twenty days.—*L'Union Médicale.—Med. Times and Gazette, Dec.* 23, 1854, *p.* 646.

108.—ON THE EXCISION OF LARGE PEDUNCULATED UTERINE POLYPI, AND ITS ADVANTAGES OVER DELIGATION.

By Dr. J. Y. Simpson, Professor of Midwifery in the University of Edinburgh.

[As soon as polypi have passed the os uteri, they easily come within reach of surgical interference. Some surgeons prefer deligation and others excision. Dr. Simpson says:]—

The removal, however, of large uterine polypi by excision, has long appeared to me to be in many respects a simpler and a safer operation than their removal by ligature. It is, on the whole, more easily accomplished; the cure of the disease by it is infinitely quicker; it is accomplished with far less restraint and annoyance to the patient; with less risk of local irritation; and, as I believe, with less ultimate chance of actual peril to health and life.

Two objections have usually been urged by accoucheurs against the removal of large uterine polypi by excision. The first of these objections, viz., the danger of hemorrhage, has been particularly insisted on by those practitioners who have written in favour of deligation. But the fact is, that excess of hemorrhage is not common after the division of the peduncle of the polypus; and however great that or any other traumatic hemorrhage from the impregnated uterus may be, it can always be arrested by properly filling the vagina for a few hours with pieces of sponge or other appropriate plug. Dupuytren, though using no plug or other means to prevent bleeding, only met with two cases of severe hemorrhage out of some 200 instances in which he removed uterine polypi by excision; and Lisfranc only observed it twice out of 165 similar operations practised by himself. I believe, however, from what I have myself seen, that the practitioner, in employing excision, must expect considerable hemorrhage in a proportion of cases much greater than this; but at the same time he can, I repeat, always readily *arrest* it when it does occur, with proper plugging; and it is perhaps best—as a general rule—to *prevent* and forestall its occurrence by introducing, for ten or twelve hours after every case of resection, a proper tampon of fine sponge into the vagina. Let me add, that it is well to have each piece of sponge which is used previously transfixed by a strong thread, the end of which should be left out of the vagina, in order to permit of the more easy withdrawal of the plug.

The second objection usually urged against excision is of greater weight, viz., that more or less injury is always liable to occur to the pelvic attachments of the uterus, if it and the polypus are forcibly dragged down by Museux's forceps, or by hooked vulsella, so as to bring the peduncle of the tumour into view before dividing it, a plan followed by most operators; or, on the other hand, if the peduncle is divided while the polypus and uterus are *in situ*, the scissors or knife employed, are, it is averred, apt to injure and cut the vaginal walls and neighbouring tissues, while worked within the vaginal cavity.

Of late years, I have used a means of excising large uterine polypi, that seems to me to obviate entirely this last class of objections, and by which, as I have been led to believe, the whole operation is much simplified, and rendered both greatly more easy to the practitioner, and more safe to the patient. By the means or instrument in question, the peduncle of the polypus is divided *in situ*, and without any chance of its cutting portion injuring the structures of the vagina or vulva.

The instrument, or polypus knife, to which I refer, is sketched in fig. 1. It is of the form of the usual midwifery hook; with the concavity, however, of the hook not blunt, but turned into a cutting surface by the insertion of a small piece of well-tempered steel blade into it. A transverse section of the curved or cutting portion of the instrument, and of its included knife-blade, is shown in a small figure, No. 2. The entire length of the instrument which I have hitherto employed in my own practice is ten inches—the length of the wooden handle being four inches, and that of the metallic shaft six inches. A shorter instrument might, perhaps, suffice equally well. The cut represents the curved portion, or hook at the extremity, as somewhat wider and larger than in the polyptome made for me in the first instance,

and which I have generally employed in practice. Perhaps an increased or a diminished size and width, in the curved hook at the extremity of the instrument, would render the operation of division by it more easy, according as the stalk of the polypus was very thick, or comparatively slender. The extreme point of the instrument is blunt and rounded; and the cutting portion or blade is so protected and concealed by it, and by the back wall of the curve, that it can be introduced into and withdrawn from the vagina, without any chance of its edge injuring or dividing the vaginal structures themselves. To be always able to discover the direction to which its hooked extremity points after it is introduced into the vagina, the front aspect of the handle is distinguished by having a slight knob or other mark upon it.

In employing this polyptome, the stalk of the polypus is first to be reached by the apex of the first finger of the right hand, introduced along the short anterior or pubic surface of the vagina; the instrument is then pushed by the left hand along this finger as a guide, and passed over or above the peduncle of the tumour in such a direction that the concavity of the hook will come down upon and embrace this peduncle, as the instrument is pulled again downwards. The next step is to make the blade of the polypus-knife cut through the stalk of the tumour. For this purpose, a little simple traction, with a slight rolling or sawing motion, is all that is generally required. If the tissue of the peduncle is dense and strong, the dividing force of the instrument may be increased by the forefinger of one hand being applied with a tractive power to the blunt extremity of the instrument, while the handle is dragged down and moved in a sawing direction, by the other hand of the operator. Sometimes when the polypus is round and loose, after the curve or hook is applied to its pedicle, the cutting portion of the polyptome will divide this stalk most readily, by merely doubling backwards with the fingers the body of the polypus upon its own stalk, and pulling the knife against the bent peduncle. In such a case, the peduncle is divided as much by pressing it against the knife, as by pulling the knife through the peduncle.

During the last few years, I have removed a very considerable number of uterine polypi of different sizes, and some of them of large dimensions, with this curved polyptome; and I can now speak from somewhat extensive experience of the perfect facility and safety of its employment.

Sometimes soft and slender cellular and canaliculated polypi, usually of an elongated form, are met with in practice, which afford no sufficient resistance for a knife to divide them or their peduncles. In such cases, the peduncle of the polypus is, perhaps, most easily severed by the careful clip of a pair of blunt-pointed curved scissors. In all other forms of the disease, where the tumour was large and pedunculated, I have of late employed the polyptome. In using it, the patient is placed in bed, in the common position on the left side; and generally the whole operation is accomplished so readily and easily, that she is often not aware that more than a common digital examination has been made. I have several times seen some difficulty attend the removal of the amputated polypus itself from the vagina, after its stalk was divided, in consequence of the great size of the tumour; and in order to effect extraction, I have occasionally been obliged to transfix it with the teeth of a large vulsellum. But hitherto I have met with no special difficulty, in at once and easily dividing the pedunclees themselves of the polypi, with this polypus-knife.

The whole operation is simple and safe, expeditious and painless, and approaches, perhaps, more than any other in practice, to the Asclepedian character of the object of the physician; "*ut tuto, ut celeriter, ut jucunde curet.*"—*Monthly Journal of Medicine, Jan.*, 1855, *p.* 10.

109.—ON FIBROUS TUMOUR OF THE WOMB.

By Dr. Edward Rigby, Senior Physician to the General Lying-in Hospital, and Exam. in Mid. at the University of London.

[The following case is interesting. Mrs. C., aged 33, very young-looking, married thirteen years, seven children, the last seventeen months ago.]

Examinatio Abdominis.—A very hard, globular, moveable tumour is felt just behind the symphysis pubis, extending to the right side, as large as an orange.

Examinatio per Vaginam—Os uteri open; uterus enlarged, bulging anteriorly; it is evidently identical with the tumour above the pubes; uterine sound passes 2½ inches, and then passes forwards somewhat further.

℞. Liq. calcii chloridi 3ss., bis die ex infus. aurant. co.; applic. ung. hydr. ori uteri quâque septimanâ.

℞. Pil. hydr. extr. hyosc. aa. gr. v. tertiis noctibus; mistura ferri et magnesiæ sulphatis omni mane.

May 9th.—Very much better in every respect; has taken the liq. calcii chloridi till within a few weeks, and continues the morning laxative as she requires it. Says that the tumour is decidedly smaller. She walks better, is fatter and stronger; tongue natural. During the last three months the catamenia have been about every three weeks.

The abdominal examination shows that the tumour has certainly not increased, but on the contrary I think that it is smaller; it is softer, and cannot be felt above the symphysis pubis so distinctly as formerly.

In a few weeks let her resume the liq. calcii chloridi.

Not having had the opportunity of making an examination when I last saw her, I cannot speak with such certainty as to the precise amount of diminution which the uterus had undergone, beyond that, as felt with the hand above the symphysis pubis, it was softer and less distinct. The distance to which the uterine sound passed when I first saw her, was not more than might be expected in the mother of seven children. The globular shape of the tumour, as felt through the abdominal wall, showed that the increase of bulk which the uterus had undergone was more at its sides than as regards its length. The anterior wall was evidently much thickened, and by its pressing on the bladder, produced the frequent desire to empty it, as was described among her symptoms. The diminution of the pain and sensation of weight in the pelvis, which arose from the pressure of the enlarged uterus, also justified the inference that the mass was now smaller.

[In another case, the patient, aged 38, was haggard and feeble with severe neuralgic pain in the sacrum. Dr. Rigby gave the liq. cal. chlor. in doses of 25 minims twice a-day, and applied the mercurial ointment to the os uteri every week. The medicine was increased weekly till she took 50 minims twice a-day.]

The uterus having descended so very deeply into the pelvic cavity, was, in all probability, the cause of the neuralgic pain which she suffered about the sacral region; in other respects there was a considerable similarity between this and the preceding case; in neither had the disease attained a great size. Both patients were feeble, and broke down in health and strength; and, as their state forbade the use of leeches, I had merely the muriate of lime mixture, and the application of ung. hydr. to the os and cervix uteri, to rely upon.

I do not quote these two cases as remarkable for their success; but offer them as pointing out the value of this species of treatment, even where the health and strength have been much reduced. Although the uterine tumour has not so strikingly diminished as in the cases previously reported, yet it was sufficiently so to relieve the pelvic viscera from much injurious pressure, and the patient from constant suffering, which was gradually exhausting her. The uterine tumour had also become softer, the catamenia more regular, and the general health of the patient greatly improved.

It is now two or three years ago that my attention was called to a most interesting paper, by my friend Doctor Oscar Prieger, of Kreuznach, on the treatment of the fibrous tumour of the uterus by the well-known mineral waters of that place. From the practice of Doctor Prieger, sen., as well as his own, he has been enabled to give the results of about seventy such cases, a large proportion of which

have been eminently successful. Although he gives the water internally, he appears to produce his most remarkable effect from the use of baths and fomentations, the strength of which has been increased by the addition of the Kreuznach salts evaporated down. In two cases of great severity, (especially one of enormous size and hardness,) and which had set me entirely at defiance, he has produced very favourable changes, with justifiable hopes of still further improvement. For some time I added a solution of muriate of magnesia of the same strength as the liq. calcii chloridi, but not being satisfied with the results, Mr. Hooper has prepared for me an artificial Kreuznach water, so concentrated for easy carriage that a tea-spoonful (3 i.) in a glass of water is the ordinary dose. I commenced this in the case last recorded, and have found it of great value in many cases.

I have also given the bromide of potassium, in five-grain doses, during the last year, having reason to believe that the Kreuznach water owes a considerable part of its activity to the presence of this salt. I have given it alone, or with the combined muriates of lime and magnesia, or have added an entire dose of it to the artificial Kreuznach water. Good results have followed its exhibition; but whether they were owing to its effects or other causes, must be decided by further observation.—*Med. Times and Gazette, March* 3, 1855, *p.* 209.

110.—PROLONGED RETENTION OF LIFE BY INFANTS WHO HAVE NOT BREATHED.

By Dr. Maschka.

The long period during which life may, under certain circumstances, be retained by the infant who has never breathed, is a fact full of interest to the physiologist, the medical jurist, and the accoucheur. The experiments of Legallois show that, in the mammalia, the fœtus which has not breathed can resist death from submersion much longer than the fœtus in which respiration has been carried on. Puppies and kittens, immediately after birth, may be kept under water for twenty-eight minutes with impunity; when five days old, they perish after sixteen minutes' submersion; and, when fifteen days old, they die as rapidly as other warm-blooded animals of any age, from deprivation of air. The human still-born fœtus can probably live longer without respiration than any other mammalian fœtus. The following cases are collected in the Gazette Hebdomadaire for December 1st, 1854, from different sources. They are very striking, and very suggestive to the practical accoucheur.

Case I. A woman, aged 25, who had tried to conceal her pregnancy, was delivered when seated on a tub. The infant, born without any signs of life, was buried in a sand-pit, and, after remaining there for half an hour, was removed, and lived. This case is described by Dr. Weese in 1845, in Badisch. Ann. f. Staatsarz, x. 2.

Case II. In 1850, a young woman was tried by the tribunals of Berlin, who had buried her new-born male infant, believing it to be dead. After an hour, the infant was disinterred, and recalled to life.

Case III. T. P., a servant, aged 23, was delivered in a stable, when leaning against the wall, alone, and in a state of unconsciousness, about half-past four, A.M., on the 16th of October. When she came to herself, she found the infant on the ground, having a spade lying upon it, with its cutting edge turned to the body. She took the infant, which was perfectly cold, believing it to be dead, and, with the placenta attached, wrapped it up in her apron, and buried it in the garden. Suspicions arose that she had been confined: she confessed; and at half past nine the infant was dug up from a depth of thirty *centimètres*. It was found lying on its face, with the placenta under the abdomen. Though cold, apparently dead, and pulseless, the cord was tied. For two hours, P., a surgeon, used means to reanimate it, when at last it began to breathe feebly, gradual signs of life became more evident, and it cried. Some slight wounds were observed on its body: wounds in the neck, which did not bleed at first, bled when the infant was restored. It took the breast greedily. On the 17th and 18th, the wounds suppurated; and, on the 19th, it died of convulsions. The physicians entrusted with the judicial autopsy, reported that it had been inhumed before it had breathed; and that it had not breathed till after it was exhumed; and that the statement of the mother was possibly true. She was

therefore acquitted of the charge of infanticide, but was found guilty of concealment of pregnancy. This case is reported by Dr. Maschka.

CASE IV. A. gave birth about noon, in a private house, to an infant, who gave no signs of life. For an hour, different unsuccessful attempts were made to animate it. The skin became blue, and gradually warmth left it. It was considered dead, and in three hours it was removed to a cold room. [This occurred in January, and the weather was very cold.] Towards evening it was closed up in a coffin. During the night, the windows of the room remained open. At 11 A.M. on the following day—that is, twenty-three hours after the birth—Dr. Maschka, being accidentally at the house, was asked to look at the body. It was perfectly cold and blue; the eyes and mouth were shut; the joints and the extremities were flexible: there was neither rigidity nor cadaveric discoloration. Astonished at this latter circumstance, but not really doubting that the death of the infant was real, Dr. Maschka placed the stethoscope upon the region of the heart, when, to his amazement, he distinctly heard the sounds of the heart: they were feeble, and at long intervals. It was impossible to appreciate the impulse of the heart against the walls of the chest, or to perceive any movement in the corresponding intercostal space. Attempts at resuscitation were made, but without any effect. At the autopsy on the following day, cadaveric discolorations and rigidity were present. The lungs were of a deep red, and contained no air: they were heavier than water. There was blood in the left, but none in the right side of the heart. It must be admitted—provided the observation of the reporter be correct—that this infant lived twenty-three hours after birth, and never breathed.

Case 5.—The following case is extracted from the 'Gazette des Tribunaux' of Feb. 20th, 1850:—A woman was tried for attempting infanticide: she had buried her infant, but it was dug up, breathed, and lived. The following are the facts. Marie and Renée lived with their father at Vernantes, in the *arrondissement* of Baugé, (Maine-et-Loire.) On the 16th of May, 1849, Marie was alone in the house with her father. About half-past-six, P.M., Renée came home, found Marie in a swoon, and called the neighbours to her assistance. Marie soon regained consciousness. One of the female neighbours, having observed numerous spots of blood, asked Marie if she had been confined. She replied, "No, it is not yet time." Her father, however, having observed the earth disturbed in a place in the garden, asked her to explain it, when she replied, "I have been confined; but, as my child was still-born, I buried it in the garden." The child was then disinterred. It was found five *centimètres* (about two inches) below the surface, with its face downwards, and with the placenta attached. Means were used to restore the infant, and it was ere long recalled to life. It was calculated that the infant had been three-quarters of an hour under ground. The accused was acquitted.

The elaborate memoir of Dr. Maschka terminates with the following summary:—

1. New-born infants can live without breathing, not merely for half an hour or an hour, but for a very considerably longer period, even in circumstances the most unfavourable.

2. In such cases, there is obviously not only an arrest of blood in the capillaries of the skin, but the vessels of the different organs are either in a state of permanent contraction, or are filled with a column of stagnant blood.

3. The movements of the heart must gradually become very slow. Here, a very important question presents itself. How can there be circulation in the infant without respiration, and therefore without the production of arterial blood? This condition of passive life can only exist when assimilation is at a minimum, and when the oxygen of the maternal blood is consumed very slowly. The maternal blood must in such cases be sufficient to maintain life.

Dr. Maschka's essay is contained in the 'Vierteljahrschrift f. prakt. Heilkunde,' t. iii., 1854. It contains interesting physiological discussions, which will repay perusal.—*Association Med. Journal, Dec.* 8, 1854, *p.* 1104.

111.—ON THE ANÆMIA OF INFANCY.

By Professor RITTER VON MAUTHNER, Vienna.

The author remarks, that for many years the practice of venesection has been on the

decline, and he quotes the words of Professor Richter, of Dresden, "Poverty of blood is, next to tuberculosis and cancer, the increasing evil of our time, which will bring down a gradual deterioration of the race, and therefore merits our most earnest consideration." According to Valentiner, most neuralgic affections are caused by anæmia; and according to Trousseau, chlorosis now prevails in the general pathology of the female. An anæmic mother will produce anæmic children; anæmia may be congenital, or acquired from too rapid development and quick growth. It is difficult to believe in the disease as congenital, the quantity of blood in the infant's tissues being normal. Valentin has shown that the amount of blood in the newly born is proportionately greater than in later life; for a child of 5 to 6 pounds has nearly 2 pounds of blood; while at the age of 30, it barely attains one-fifth of the weight of the body. But, in infancy, as in old age, the watery constituent is more considerable than in middle life.

The cause of this often congenital anæmia lies in the general corporeal weakness of the mother, whence also it comes that there are so many abortions and early deaths. Want of proper food during pregnancy exerts a potent influence, too commonly at work among the lower orders, oppressed with want and care; and the younger children are more subject to the disease, because the exhausted mother loses in time the nourishing her children by her own milk, and the father has not the means of procuring a wet-nurse. A child born of a mother who has suckled another infant during her pregnancy, generally suffers from poverty of blood. Losses of blood, or profuse mucous discharges, by injuring the mother's health, are to be regarded as prejudicial to healthy fœtus development. An aged or diseased father commonly begets an anæmic child. Congenital syphilis must also be regarded as a cause. The morbid change in the blood being unknown, we must be content with the term, "anæmia syphilitica." It is not always recognised, but is of most momentous importance as regards the rising generation. Should an infant affected with this disease be vaccinated, a peculiar glandular disease is apt to ensue from this second poisoning of the blood. The author proceeds to enumerate the symptoms of congenital syphilis. The child suffers from excoriations about the mouth and anus, &c., from roseola, pemphigus, eczema, psoriasis, or from a peculiar tenuity, smoothness, and transparency of the epidermis. They do not possess power to resist external influences; they are long in teething; tuberculosis is apt to ensue in the course of time.

The anæmia of development comes on when growth at any period is very quick Hence we have the anæmia of dentition, the anæmia of puberty, or chlorosis.

From experiments upon animals, Nasse has shown that animal diet renders the blood more coagulable than vegetable diet, and increases the number of the blood-corpuscles. From sugar and starch-meal there is formed a glutinous lymph plasma, but no corpuscles. A purely vegetable diet, therefore, is not suited for infancy; the more so, from the anatomical fact, the cœcum, that part of the intestinal canal where vegetable digestion goes on, is but imperfectly developed at that period. Anæmic children are very apt to suffer from inflammations. Nature endeavours to excite a re-action from this depressing influence; and stasis of the blood commonly ensues in organs unfitted for active circulation; but the exudations tend only yet more to impoverish the blood; and venesections materially increase the evil. One of the evils of infantile anæmia is hemorrhage from the congested and delicate vessels of the large intestine. This occurrence is frequently overlooked, or confounded with other disorders, such as convulsions. The author has verified the fact by dissection.

When the plastic power of the organism, says Canstatt, is exhausted by rapid natural and morbid evolutions, then tuberculosis suddenly forms, and the latent material begins to be deposited upon any excitement in the different viscera. Thus, it is to be feared during the blood-impoverishment of dentition; and it attacks especially children subject to perspirations, and to excited pulsations of the heart.

The practitioner should never forget, in considering the diseases of childhood, that sudden attacks, even death itself, may occur just as easily in children from anæmia as from hyperæmia. Thus, cases of convulsions and other attacks, once regarded as inflammatory, require, in the present day, more careful examination and more accurate diagnosis.—*Jour. für Kind.—Med. Times and Gazette, Nov.* 4, 1854, *p.* 474.

112.—OVARIAN DROPSY TREATED BY INJECTION OF IODINE.

Dr. SIMPSON mentioned that he had injected a case in his wards a few days ago, from which he had removed 200 ounces of fluid, and that he had tested the urine for iodine, but without success. It was usually present, however, after the injection of a hydrocele. He (Dr. S.) had now injected some 10 or 12 cases of ovarian dropsy; the disease had occurred in a few of the cases, but in the others, the cure had as yet been permanent. The operation was unattended with any bad results. Indeed, the pulse never rose, and little or no uneasiness was expressed, and that only (if present) in drawing the fluid, apparently the result of adhesions. He employed the Edinburgh tincture of iodine, which had also been recommended for its strength by Mr. Martin, in his paper on the cure of hydrocele by injection.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 565.

[In a discussion in the Medical Society of London, Dr. TYLER SMITH referred to the case of Dr. Simpson of Edinburgh, in which this gentleman had injected iodine into ovarian cysts.]

Dr. Simpson had now performed the operation many times, and in one case, at the end of a year and a half, there had been no return of the disease. Dr. T. Smith described a case of ovarian dropsy, under his care, in which he had, on the previous Tuesday, with the assistance of Mr. Nunn, drawn off the fluid, and injected four ounces of the tincture of iodine into the cyst. As regards any danger from the operation, the patient had done remarkably well, quite as well as if only ordinary tapping had been performed. The pulse had not at any time exceeded 100. The smell of iodine had been very powerful in the secretions. Both the urine and saliva had darkened silver very readily, showing that iodine had been absorbed, and probably from the peritoneal surface, as it was difficult to understand that the cyst could absorb, or that its injection could be effected without the entrance of some portion of the tincture of iodine into the peritoneal cavity. The injection of the cyst certainly offered another method of dealing with this formidable affection.

Mr. I. B. BROWN remarked that the plan mentioned had been resorted to in Paris in several cases; in only one instance he believed had inflammation, followed by suppuration, supervened, proving fatal to the patient, showing the little liability of the living cyst to be affected by dangerous inflammatory action. Cases so operated upon should be of the simple kind, such as were generally amenable to treatment by tapping, pressure, &c.

Mr. HANCOCK had known of one case, in which a French surgeon had resorted to the operation in question, the pain resulting from which was so agonizing, that for fourteen hours the patient had to be held by four men; but the patient was ultimately cured of the disease.—*Lancet, Dec.* 2, 1854, *p.* 459.

[Mr. WINDSOR'S successful case of injection of iodine into the pleural cavity, mentioned at page 85, of this volume, is another confirmation of the comparative safety of this practice. If we may inject iodine into the pleural cavity, surely we may proceed to the same practice in an ovarian cyst, and even to other cavities.]

113.—ON THE TREATMENT OF OVARIAN DROPSY BY THE INJECTION OF IODINE.

By I. BAKER BROWN, Esq.

[Mr. Brown alludes to six modes of treating this disease: 1. Tapping. 2. Tapping, with pressure. 3. Tapping, and injection of jodine. 4. Artificial oviduct. 5. Excision of a portion of the cyst. 6. Extirpation.]

It is to the third plan of treatment I now desire to direct especial attention, viz., tapping and injection of the tincture of iodine into the sac. This mode of treatment is almost new in London practice, not so, however, in Paris, nor in Edinburgh. The injection of the tincture of iodine into the tunica vaginalis, for the radical treatment by hydrocele, was prominently brought before the profession, many years since, by Mr. Ranald Martin, and is now the regular practice amongst English surgeons; but the French surgeons have carried their experimental inquiries much further in this direction. Velpeau first suggested its application for effusion into large

joints. Bonnet, of Lyons, however, made the first trial, and with perfect success. This success was soon followed by many others in the hands of Velpeau, Robert, and some others. Then Boinet of Paris took up the subject, and applied the injection of iodine into ovarian cysts, and published several successful cases in the 'Gazette Médicale de Paris.' Others followed his example, and amongst the number, Monod, and one of his successful cases fell under my own observation. The French surgeons have also used this injection of iodine for peritoneal dropsy, and it appears, from various discussions and trials, with undoubted success. Our intelligent neighbours have gone further, especially Velpeau, Jobert, Maisonneuve, and Ricord, and have injected the peritoneal sac of hernia, and have obtained by it the radical cure of ruptures; and lately, my colleague in St. Mary's, Mr. Coulson, has tried this plan.

The first real practical application of the injection of tincture of iodine into ovarian cysts, that I am aware of, in this country, is by Professor Simpson, of Edinburgh, who with his usual zeal and industry has pushed his inquiries and experiments very freely; the result of his investigations I shall allude to fully when giving my own practical deductions. I shall now relate a case which I have lately treated in St. Mary's Hospital, and then offer some practical remarks for the consideration of the Fellows.

Case.—Mary Baker, kindly sent to me by Mr. Coulson, was admitted into Boynton Ward on December 9th, 1854. She stated that she had had two children, and one miscarriage. She was twenty-seven at the birth of the first, and twenty-nine at the birth of the second child; from the latter period she has noticed herself getting much larger round the waist, and has been much troubled by flatulency; has always been regular in menstruation all through her illness; she suffers no pain, but considerable inconvenience from leucorrhœa.

Examination. December 12, 1854.—I discovered a well-marked ovarian cyst, apparently unilocular, and fluctuation distinct. The measurement around the abdomen was 29 inches below, and 28 above the umbilicus. I placed her under medical treatment, with a view to improve her general health, which was much impaired by secretion of the fluid into the cystic cavity, and on

The 20th, I proceeded to empty the cyst; first placing the patient on the table in the horizontal position, I introduced a large trocar through the semilunar line, and evacuated twenty pints of a turbid thin fluid, which was found to be strongly albuminous, becoming almost solidified by the joint application of heat and nitric acid; it also contained abundant crystals of cholesterine. I then introduced a long flexible catheter, and through it injected five ounces of the tincture of iodine, (Edinburgh Pharmacopœia,) which is about double the strength of the London Pharmacopœia—the pain experienced was very trifling, described by the patient merely as "smarting:" the wound having been closed by strapping, I applied appropriate pads, and one of my many-tailed bandages; the patient was then placed in bed, and two grains of opium given, and four ounces of port wine ordered for the next twenty-four hours.

In my case-book, I find Dr. Vernon, the resident medical officer in charge of the patients in the ward for diseases of women, has made the following notes:—

10 p.m. Pulse 76—expresses herself as being very comfortable, having neither pain nor tenderness in the abdomen; complains only of a bad taste in the mouth—her breath smells of iodine.

21st, 10 a.m. Feels comfortable,—has slept only two hours during the night,—has voided three and a-half pints of urine since the operation, the fluid injected having amounted only to two pints; says she has a nasty taste in her mouth, somewhat like sea-weed.

1 p.m. Complains of her cough being troublesome, and of pain in the abdomen on both sides when she coughs—the pulse has mounted up to 110—discontinue the wine and take a tincture for the cough—continue the opium pill every six hours.

22nd. Amount of fluid taken, 3 pints; voided, 3 pints; bowels relieved this day. Pulse 90.

23rd. Amount of fluid taken, 1½ pints; voided, 1 pint. Pulse 76. No pain or tenderness in abdomen.

24th. Amount of fluid taken, 1¼ pints; voided 1 pint. Pulse 74. Skin cool; and she complains only of a nasty taste in her mouth.

25th. Has taken 2 pints of fluid, and voided 1 pint.

26th, 27th, 28th, 29th. The same.

From this date to the 15th of January she took a diuretic mixture, and the effect on the secretions was, that the amount of fluid voided corresponded with the amount taken.

It was now apparent that there was some refilling of the cyst, but it proceeded very slowly, her appetite was good, she slept well, and suffered no pain.

20th Jan. There is apparently about two quarts of fluid in the cyst, but it does not appear to increase, and the patient is decidedly better in health. I now ordered her to wear one of my ovarian bandages to keep up gentle pressure over the whole abdomen, so as to give support to the parietes, and also to arrest the refilling of the cyst. In a few days she left the hospital in good health, considering herself much improved, and exhibiting no external signs of disease. I have lately seen and carefully examined her, and can find no increase of fluid, but great improvement in her general health, and she has resumed her laborious duties of laundress, and states she feels quite well and is in good spirits.

Before offering some practical observations I am desirous of drawing the attention of the society to the important fact of the iodine having been taken up speedily into the system, as evinced by the smell of the breath, and the taste in the mouth. I was anxious to ascertain whether it was to be found in the urine, and therefore requested our dispenser, Mr. Copney, to examine some for me on the day following the injection. He says:—

"The urine gave unmistakable evidence of the presence of iodine by the following tests:—

"1. The formation of the blue iodide of starch of considerable intensity.

"2. By its forming with the salts of lead and mercury the characteristic of iodides of those bodies.

"3. By the production of hydriodic acid.

"4. By the elimination of the iodine itself."

Three days later, I examined the sweat of the patient, but could then detect no iodine, although it is probable that, if I had made the examination sooner, I should have found evidence of its elimination.

Practical Remarks.—The first question in a practical point of view is, Has the treatment in this case been successful? The answer is, that it has been only partially so at present; for, although it has not prevented some return of the fluid still it has evidently controlled the refilling of the cyst, and the general health has much improved; further time is required to test the extent of benefit. My own impression is, that in such a case the injection of iodine is not likely to effect a complete cure, yet that it will arrest the refilling. I believe that those cases which are radically cured by this plan are the more simple form of unilocular ovarian dropsy, —*i. e.*, where the cyst is thin, and its contents non-albuminous, or slightly so. The dropsy of the broad ligament is especially favourable for this plan; but then, so it is for simple tapping and pressure, and the latter plan is a less hazardous one than the former. I apprehend we cannot expect to solve the whole question until we know more of the pathological structure of the different cysts, in each form of the disease. I am investigating this question by aid of the microscope, and I hope to arrive at some certain facts as to the peculiar condition of the secreting surfaces, and the peculiar fluid from those surfaces; so that, by drawing off a small portion of fluid, we may be so certain of our diagnosis, as to recommend with greater confidence the adoption of any mode of treatment best fitted for that especial case. It will be seen at once that this will prove of the greatest practical importance; and I do not at all despair of being able to form certain general rules from the results of these inquiries; and I shall feel particularly indebted to any Fellow of the Society who will favour me with a post-mortem specimen of this disease. Much has already been done during the last twenty years, and I trust to see much more done in future years.

In the "Monthly Journal of Medical Science" for May, 1854, is the following opinion of Dr. Simpson:—

"Dr. Simpson has, within the last year, injected into dropsical ovarian cysts, subsequent to tapping, the tincture of iodine of the 'Edinburgh Pharmacopœia,' undiluted, in seven or eight cases. He has usually thrown into the cyst two or three ounces of the tincture. In some cases, he has allowed a portion of the injected fluid

to escape; in others, has retained the whole of it. From these cases he drew the following conclusions:—

"1. In none of the cases of ovarian dropsy treated with iodine injections after tapping, has he yet seen any considerable amount of local pain follow the injection, with one exception; in most instances no pain at all is felt; and in none has constitutional irritation or fever ensued. In one exceptional case considerable local irritation followed, and the pulse rose to 110; but the same phenomena occurred in the same patient after previous tappings, without iodine being used.

"2. While the practice seems so far perfectly safe in itself, it has by no means proved successful, as in hydrocele, in preventing a reaccumulation of the dropsical fluid; for in several instances, the effusion into the sac seems to have gone on as rapidly as after a simple sapping without iodine injection.

"3. But, in two or three of the cases, the iodine injection appears to have quite arrested, for the time being, the progress of the disease, and to have produced obliteration of the tapped cyst, as there is no sign whatever of any reaccumulation, though several months have now elapsed since the date of the operation.

"Lastly. Accumulated experience will be required, to point out more precisely the special varieties of ovarian dropsy, most likely to benefit from iodine injections, the proper times of operating, the quantities of the tincture to be injected, and other correlative points. Perhaps the want of success, in some cases, has arisen from an insufficient quantity of iodine being used, and from the whole interior of the cyst not being touched by it. The greatest advantage would of course be expected from it in the rare form of unilocular cysts. In the common compound cyst, the largest or most preponderating cyst is usually alone opened in paracentesis; and though it were obliterated, it could not necessarily prevent some of the other smaller cysts from afterwards enlarging and developing into the usual aggravated form of the disease."

I have preferred giving the whole abstract, so that Dr. Simpson's opinion might be fully understood. It will be seen that he used only half the quantity of tincture that I have used; and I intend, in the next case, using 10 or 12 ounces, so as to insure, if possible, the complete covering of all the cystic surface.—*Med. Times and Gazette, April* 14, 1855, *p.* 359.

114.—TREATMENT OF HYDATID TUMOURS IN THE ABDOMEN.

[The Editor of the Medical Times and Gazette appends the following general remarks to an interesting series of reports of abdominal tumours.]

Operative Measures.—It has been attempted to show that operative interference should be reserved for cases in which urgent symptoms impend. An hydatid tumour in the abdomen may cause urgent symptoms, *first*, by acquiring a very large size, and distending the cavity; *secondly*, by interfering with the function of the viscera adjoining it, as the liver, kidney, &c.; *thirdly*, by occluding, by pressure on the intestines, uterus, &c., the passage of the excretions; *fourthly*, by exciting surrounding inflammation, and threatening to rupture. In any one of these conditions, surgical assistance may come to be demanded, but the measure required will differ considerably in each. Thus, when the cyst is so large as to have distended the abdomen, it will generally have acquired adhesions, so that its puncture will be comparatively without risk. The same will be probable if it have excited surrounding inflammation, under which condition also it may become necessary to make a permanent opening for the escape of pus, when otherwise a trocar puncture would have sufficed. The cases, however, in which urgency is caused by interference with the viscera, are by no means secure from the risk that the tumour may be quite unconnected with the parietes; and in the treatment of these the greatest judgment will be called for. If the tumour be in the lower half of the belly, examination *per rectum* and *per vaginam* if the patient be a woman, should never be omitted, as it might chance that a puncture would be best made into one or other of these canals. A case has been already mentioned, in which a rectal examination would, probably, have saved the life of the patient. Supposing, however, that it seems necessary to make an external opening, and that there is reason to believe that the cyst has no adhesions to the parietes, the surgeon has then the choice of two methods—the use of a very fine trocar, or the application of an escharotic. The latter means was

employed very successfully by Mr. Le Gros Clarke, in a case under his care, and the former may be confidently stated to be very safe. The selection will, therefore, depend chiefly upon whether it be wished to make a permanent opening or not. And this brings us to the consideration of the important question already mooted, as to whether it is absolutely necessary to obtain the evacuation of the contents of the cyst, or whether to kill it, and then trust to their absorption, will suffice. Hitherto the plan of simply destroying the life of the cyst, has, we believe, not been tried. In Mr. Le Gros Clarke's case, however, it probably occurred, and the cure resulted without the removal of the smaller cysts, as had been intended. This accidental result is very instructive. Might it not be worth while, in future cases, to take means to secure the death of the parasite, such, for instance, as the injection into it of a solution of salt, or of iodide of potassium? The evacuation plan necessarily involves long-continued and profuse suppuration, which, supposing the cyst large, may become very dangerous. At any rate, whichever plan be selected, there can be no doubt as to the propriety of conducting the first steps of the treatment on the plan exemplified in the case which did so well under the care of Mr. Cock, namely, of reducing the size of the tumour by repeated punctures with a very small trocar. Hydatid cysts re-secrete very slow, and by such means great diminution in size may be effected, almost without risk. The trocar should be an exploring one, and of very small bore.

We must not leave this part of the subject without venturing one further remark, to the effect that a patient, suffering from urgent symptoms, however produced, in consequence of an hydatid tumour in the abdomen, should never be allowed to die without the attempt to relieve by operation being made. The recorded cases have been quite sufficiently successful to indicate the propriety of making such attempt, even under very unhopeful circumstances.—*Med. Times and Gazette, March* 17, 1855, *p.* 260.

MISCELLANEOUS SUBJECTS.

115.—ON THE PROPERTIES OF THE ORDEAL-BEAN OF OLD CALABAR.

By Dr. Robert Christison, Professor of Materia Medica in the University of Edinburgh.

[Dr. Christison gives us a long and interesting account of this poisonous bean and the use made of it by the natives of old Calabar. The effects which he has himself observed are detailed at length in the Edinburgh Monthly Journal for March, and are accompanied by the following excellent remarks, on the effects of some other poisons on the system.]

When a poison impresses powerfully both the circulation and some function or functions of the nervous system, it is a matter of great nicety to eliminate the true phenomena, especially by observation upon the lower animals alone.

We know that some poisons, such as strychnia, and the various seeds and barks which contain it, cause, by direct irritation of the spinal chord, violent tetanic spasms of the voluntary and respiratory muscles, without impairing sensation, or enfeebling the heart, or clouding the mental faculties; and thus they occasion death by convulsive arrestment of respiration. Others, such as the urari poison, and conia, or hemlock from which it is derived, cause, by direct exhaustive action of the spinal chord, the opposite state of paralysis of the voluntary and respiratory muscles, but still without influencing the heart, or sensation, or the mental powers; and so death arises in their instance, from arrestment of respiration, by simple paralysis of the muscles which maintain it. Others again, such as atropia, or belladonna, the plant which yields it, principally assail the functions of the brain, at first combining stimulus of some with exhaustion of others in the most singular and often grotesque concatenation, but inducing at last a state of profound coma, and as the result of this a universal muscular paralysis; and thus death ensues, equally as before, from the arrestment of the breathing, not however by direct action on the origin of the nerves which govern the muscles of respiration, but indirectly, through an influence on the cerebral functions, exactly as in ordinary apoplexy. We can likewise conceive a poison to possess only a simple and direct action upon the heart, producing exhaustion of its irritability, paralysis, and consequently death, by the arrestment of the circulation; but no such poison is yet known.

These are all instances of simple action on a single vital function. But many poisons exert a more composite action. Some, such as nicotina, and its source, tobacco, produce paralysis of the heart, and also a narcotic action on the brain. Others, such as foxglove, and in all probability its active proximate principle, digitaline, not only possess this double action on the heart and brain, but likewise powerfully irritate the kidneys. Others, such as hydrocyanic acid and picrotoxa, the active constituent of cocculus-indicus, exhaust the functions of the brain, so as to induce coma, and at the same time irritate the spinal chord, so as to excite convulsions; and thus, here again we have death produced by arrestment of the breathing, indirectly through the brain, but concurrently with direct spinal irritation. In others, such as aconitina, and its source, monkshood, there is a singular combination of exhaustion of the heart's irritability, and of common sensation, but without any influence on the voluntary muscles, or on the mental faculties; and death arises by arrestment of the circulation.

It is easy to see,—on considering attentively what must be the manifestations of these various actions, both simple and compound, but especially the latter,—that extreme difficulty will often occur in seizing and rightly comprehending the facts, above all when the succession of phenomena is swift, and when the subject of observation is one of the lower animals, which cannot adequately express by external signs the varying influence of agents on sensation and the other cerebral functions.

Hence it arises that many erroneous conclusions have been come to regarding the action of our most potent and interesting poisons. Take for example hemlock. This formidable poison was long supposed to cause death by coma, that is, a narcotic action on the brain. But I have shown in a paper read before this Society in 1836, that the mode of death is really by paralysis of the muscles and arrestment of respiration, through an exhaustive influence on the spinal chord. And it is easy to see where the source of error lay. For, when the muscles are paralysed, sensation and the mental faculties will seem to a common observer to be paralysed also; because the animal mechanism for producing expression is at rest.—It appears that many persons think it an easy task to investigate experimentally the physiology of poisoning. But they are assuredly mistaken. A long apprenticeship must be passed before any one can observe with accuracy the phenomena of the action of poisons.—*Monthly Journal of Medicine, March* 1855, *p.* 198.

116.—*Note on the Therapeutic Action of the Salts of Cerium.* By PROF. SIMPSON, Edinburgh.—The salts of few metals were at present in our pharmacopœia, and Dr. S. saw no reason why many more might not be added to their number. He had recently drawn the attention of the profession to the salts of nickel, which, as far as his observation went, presented much similarity in action to those of iron and quinine, and seemed to be of use in cases of sick headache. He now proposed to read to the Society some very imperfect observations on the therapeutic action of some other metals: and first, as to cerium, which, given in the form of nitrate, and in one grain doses twice or thrice a-day, appeared to act as a sedative tonic of considerable value, strongly resembling bismuth, and the salts of silver. He had employed it in the first instance in cases of general chronic intestinal eruption—a peculiar and intractable form of disease, for which arsenic and nitrate of silver were generally prescribed, and where these remedies had failed, cerium had been tried with marked advantage. In irritable dyspepsia, with gastrodynia and pyrosis, and in chronic vomiting, its exhibition was attended with satisfactory results; and in the vomiting which occurs during pregnancy prompt relief was afforded. It was a good tonic, and a useful substitute for the salts of silver, bismuth, and hydrocyanic acid. Dr. S. had not employed it much in convulsive diseases, as chorea and epilepsy, in which nitrate of silver was used, but the exhibition of the salts of cerium was certainly attended with this advantage, that it could be persevered in without any fear of discolouration of the skin. As far as his experiments with cadmium went, it bore much resemblance to the preparations of antimony, and excited diaphoresis and vomiting. Tellurium, besides its expense, was precluded from being used in practice by its disagreeable effects. Dr. S. mentioned the particulars of a case where a dose had been inadvertently given to a student of divinity, and had been followed by the evolution of such a persistent odour, that for the remainder of the session the patient had to sit apart from his fellow-students.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 564.

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117. *Anaphrodisiac Properties of Bromide of Potassium.*—THIELMANN recommends this remedy as an excellent anaphrodisiac in satyriasis, in the frequent and painful erections during gonorrhœa, in spermatorrhœa, and in nymphomania. He administers it in doses of from 2 to 3 grains every two or three hours; and, at the same time, enjoins a mixed vegetable and milk diet, and forbids all acid substances.—*Med. Zeit. Russi.—Dub. Hosp. Gazette, Feb.* 15, 1855, *p.* 32.

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118. *Treatment of Simple Incised Wounds.* By PROF. SYME, Edinburgh.—[If we close a wound too soon it often refuses to unite, on account of the oozing of the blood upon the surfaces. It is often the best plan, therefore, to defer dressing the

wound for a time. This, however, occupies valuable time, and Mr. Syme therefore adopts the following plan. He says,]

After no long time, however, I found that it was unnecessary to keep the patient all this time in a state of suspense, when he ought to be at rest, and then subject him a second time to disturbance; for, still following out the same principle, I saw that, by placing pads of lint upon the sides of the wound, but leaving their edges free, the wound might be permanently dressed at the time of the operation so as to attain the same object—the pressure of the pads, like that of the pin in hare-lip, preventing the accumulation of blood in the deep parts, while sufficient interval was left between the stitches to allow its escape between the edges of the wound in the skin, which were preserved free from pressure. 'Accordingly from that time to the present—that is to say, for thirty years—I have followed this method, and I am happy to say, union by the first intention has been always obtained, unless there was some unusual circumstance to interfere with it. The dressing is not disturbed for three or four days after the operation, when the lymph between the raw surfaces is already of considerable firmness; and the same dressing is usually continued afterwards, as it requires to be changed only every other day, and involves less disturbance than any other method. In illustration of the same principle, I may remark, that, when the cavity of the wound is peculiarly irregular, or when the situation is such as to make it difficult to apply pads of lint effectually, I find a large sponge a very useful dressing. Thus, after the removal of tumours of the neck above the clavicle, or at the angle of the jaw, I have by this means obtained complete union by first intention.—*Lancet*, *March* 31, 1855, *p.* 334.

119.—ON POLYPUS OF THE NOSE.

By James Syme, Esq., Professor of Surgery in the University of Edinburgh.

[In a clinical lecture on this subject, Mr. Syme remarks, that the nose of the patient was small and the nostrils narrow, and that the growth evidently extended into the throat, which could be ascertained by the finger.]

In considering the means that should be employed for the removal of this polypus, I beg to remind you that non-malignant growths are very limited in regard to the seat of their origin from within the nose, as they never proceed from the floor, septum, or external wall, and always do so from the roof, or that part which is formed by the turbinated plates of the ethmoid bone. Further, I must remind you that each nostril affords very little space in its transverse direction, and that the narrow slit which constitutes it is divided longitudinally into two nearly equal portions by the inferior spongy bone. Now the mucous polypus, always originating from the small extent of surface that has been thus defined, whether it does so singly or, as more frequently happens, in groups, descends so as to occupy first the higher and then the lower channel of the nose, but cannot attain any great size so long as it is limited to these situations; and not possessing sufficient vigour of expansion to enlarge the osseous space, in order to attain any considerable bulk, must therefore proceed backwards towards the pharynx. Keeping these things in mind, you will at once perceive that the plan of removing polypus by means of a thread or wire, whether simply put up the nose, or passed through it into the throat, cannot possibly extirpate the growth, or do more than remove a portion of its substance. You will also require no arguments to prove the inutility and inexpediency of trying to accomplish the object in view by scissors, knives, or caustic; so that our choice is thus limited to evulsion by forceps.

This operation is generally regarded as very unsatisfactory, from the relief which it affords being seldom complete, and still less frequently permanent. But these imperfect results should in a great measure be ascribed to the faulty nature of the procedure usually employed, which is, aiming at the body of the growth to be removed instead of its narrow root, so as to detach merely the most projecting portions, instead of extirpating the whole mass entire. It is therefore no wonder that the operator fails to relieve the patient, or finds many "sittings," as he calls them, necessary, and nibbles away, in vain attempts to effect a radical cure. The forceps that were formerly employed on such occasions are indeed of themselves

sufficient to show the utter hopelessness of succeeding by their means, since the blades are so large that they could not be insinuated into the narrow channel where the root of the growth is attached. You should select for the purpose an instrument such as that in my hand, which is of the smallest possible size, consistent with the requisite degree of strength; introduce it gently along the upper passage of the nose, with the blades separated so far as the walls of the cavity permit, until its further progress is arrested by the neck of the polypus; then firmly compress the handles, and, by a combination of traction with torsion in one uniform direction, break through the connecting textures included. If the growth does not follow the instrument, it must be introduced again and again in the same way until the nostril is completely cleared, when a piece of lint may be introduced to prevent further bleeding.

[Mr. Syme then proceeded in the way that has been described, and after several introductions of the forceps, succeeded in removing a large round polypus quite entire, immediately after which the patient breathed with perfect freedom through both nostrils.]

It seems not unworthy of notice that in this case there are some points of resemblance to that remarkable tumour named the fibrous polypus, of which the characters are unity of growth, ligamentous toughness of consistence, extreme firmness of attachment, and disposition to bleed. Several cases have occurred here in which the patients had nearly died of hemorrhage, and all the strength that I could exert was hardly sufficient to effect extraction, while blood streamed in torrents from the nose and mouth. But as this disease is not malignant, the operation, however formidable, proves satisfactory in its result. Now, you may have remarked, that the tumour which has just been removed was solitary, and confined to one nostril, instead of occurring in both, as the mucous polypus almost always does; also that its connexions were very firm, and that the bleeding was considerable. It would seem as if there may be conditions intermediate between the two sorts of growth, in which the respective characters more or less closely approach those of each other. For instance, some years ago, a nobleman returned from public service in one of the colonies, on account of complete obstruction to breathing through his nose, with a constant discharge of watery fluid from it, and occasionally alarming hemorrhage, of which the amount was estimated not by ounces but by pints. He remained in London for two months under the care of several practitioners, who regarded the disease as incurable; and finding himself thus given up as hopeless, he applied to me, bringing with him a letter from the late Mr. Bransby Cooper, which stated that the result of repeated consultations with Sir B. Brodie, Mr. Travers, Mr. Cæsar Hawkins, and himself, was a decision against interfering with the disease, which they seemed to regard as a tumour of the throat. Seeing no sign of malignant disposition in the aspect of the patient, who was about thirty-four years of age, and recognising in the tumour the characters which were familiar to me as those of a nasal growth stretching backwards into the pharynx, I at once undertook to remove the source of complaint. The patient and his friends at a distance having given their consent, I introduced the forceps, and at the very first pull extracted the whole growth, with immediate, complete, and permanent relief. Four years have now elapsed, and there has not been the slightest symptom of relapse to interfere with the enjoyment of perfect health, or the vigorous exertion of an active life.—*Lancet*, *Jan.* 6, 1855, *p.* 2.

120.—ON A NEW METHOD OF TREATING BRONCHOCELE.

By William Turner, Esq., Kensington.

[Mr. Turner first relates the following interesting cases, and from them proceeds to detail his new method of treating bronchocele.]

Was called in 1845 to a Mrs. H., of Holland-street, Kensington, an aged female, suffering from phlegmonous erysipelas; which had extended from a wound in the hand, and now involved the whole arm, neck, and side face (the neck being enormously swollen, owing to a very large general bronchocele as well as the erysipelas). This large bronchocele had existed forty years, and on a former occasion I got to examine, (although the doctors had told her it would be the death of her, if inter-

fered with), and found a large fluctuating cyst imbedded in the mass of enlarged thyroid. This foreknowledge emboldened me now to make a free incision into the mass, with the hope of relieving the present distressing symptoms impending suffocation. There was most urgent dyspnœa, purely, I judged, from the great tension about the throat; countenance livid, the tongue protruded, and every evidence of such trouble to the respiration and circulation as must quickly terminate life if unrelieved. A free incision into the large cyst discharged about a pint of serous, fetid fluid, at the same time giving immediate relief to the most urgent symptoms. The cut edges of a thick yellowish cyst became evident about an inch from the surface of the skin. The opening continued to discharge, and, by aid of a female catheter, I withdrew from time to time the liquid fetid contents which accumulated in the lower part of the cyst, until by degrees, and at length after six weeks, the whole cyst came away, and the opening in the neck healed; and, to my surprise, after many months, the enlarged thyroid body gradually got less, so that after two years she had lost all her bronchocele.

In 1849, Mrs. R., of High-street, Kensington, appeared to be suffering much pain and distress in breathing from a suppurating cyst, developed in a large but unilateral bronchocele. This I perforated with a trocar and canula, and discharged fetid matter from it. The discharge in this case continued for months; no cyst came away; but yet the thyroid enlargement has so much decreased as to be scarcely visible; tells me she can now do needlework. Before the operation, the tumour prevented her looking down on her work.

From these and other cases it was imagined, if inflammatory action could be induced and maintained for a time, it might, as one observes sometimes in the testicle, and as these cases in the thyroid appeared to demonstrate, be followed by adhesive and constructive inflammation from effusion and infiltration ultimately giving rise to absorption from obliteration of nutrient vessels.

The first case operated on with this intention was in 1852. Mrs. C., of Vale-place, Hammersmith, aged 43, of spare, delicate habits, complaining much of the trouble and inconvenience to her breathing, produced by a unilateral enlargement of the thyroid body. This was about the size of a large lemon, and had resisted all the ordinary means employed without any good result; therefore, selecting the most prominent part of the tumour within the inner edge of the sterno-cleido mastoid, I applied the potassa fusa over a space the size of a florin. On the separation of the slough, produced after twelve days, the potass was again applied, and repeated at intervals, until the thyroid was exposed, by the gradual destruction of the superincumbent tissues. Next, a blunt-pointed grooved director was passed freely into the substance of the enlargement in various directions. This gave rise, after some days, to considerable constitutional irritation and feverish disturbance, with heat, pain, and some slight swelling in the thyroid, followed by a copious watery discharge from the opening; the patient imagined a pint and a-half came off daily for two weeks, and then gradually subsided, as did also the swelling and constitutional irritation. The patient's present condition, free from any enlargement and with the slight disfigurement produced by the eschar, is most satisfactory.

Mrs. M., of Warwick-square, Kensington, aged 36, a nervous, delicate lady, of thin, spare habit, mother of a large family, requested relief from a unilateral enlargement of the thyroid, giving rise to difficulty and a noise in respiration on any active exertion: complained of starting in alarm from sleep, imagining some one was strangling her; could not read aloud, as was her former custom; for many months had been subjected to various plans of treatment, without relief.

The same mode of treatment was adopted in this case as that last detailed, with the caustic potash. About the period when the gland was exposed, the patient's health became rather suddenly disturbed from uterine derangement, followed by general nervous excitement and irritation. I was obliged to desist from further measures as regarded the neck. The patient slowly recovered from this low nervous fever (*secundum* Sir James Clark, who visited her when in this state.) A portion of the thyroid, the size of a walnut, blackened and spongy, came away after a time without hemorrhage. The opening gradually healed, and this lady is now in robust health, and although relieved of the urgent symptoms complained of, the site of the former swelling, as well as the eschar, is now in a depression. Still, the posterior portion of the gland behind the stern-cleido mastoid has much increased since the

operation, now nearly two years; yet, as I had not the opportunity to pass the probe through and among this portion, I do not consider it as having been subjected to the proposed plan. The perfect freedom from hemorrhage, by the employment of the method proposed, has much surprised me. The caustic potash seems primarily to seal up the vessels by chemical action, and, secondarily, by inducing inflammatory action on the surrounding structures, the blood-vessels are sealed up prior to the separation of the slough produced at each step in the progress. I imagine vessels of some considerable size are thus quietly cut through without bleeding.

Although the thyroid body in its natural state is not contained in any distinct capsule or membrane, yet, by its enlargement, it appears to condense the cellular tissue and fascia about it into a dense membranous covering, which would well resist the passage of a blunt probe or grooved director, unless inconsiderate force was employed; and, as all the larger vessels are chiefly confined to the upper and lower margins of the lateral lobes, one does not risk them in perforating the most prominent portion of the tumour, which always presents itself, either in front or behind the sterno-cleido mastoid; in the former case, covered beneath the cervical fascia by the expanded fibres of the sterno-thyroid and hyoid; in the latter by fascia latissimus colli and omohyodei.

Although very many kinds of treatment have been directed to the relief of bronchocele, not a few have had the great disadvantage of being more desperate and dangerous than the disease; and some may imagine the one now proposed of the same class; yet, from my experience, I do not find it so, and consider it worthy of a more extended trial by those of my profession who can command beds in hospitals, and have a better opportunity of keeping close watch on such cases than one engaged in general practice can do. With this impression, I have been induced to bring these observations before them, and must crave all indulgence for this hastily-written paper. —*Med. Times and Gazette, Jan.* 20, 1855, *p.* 56.

121.—*Note on the Performance of Tracheotomy.*—A few words may perhaps be permitted us concerning the operation of tracheotomy, more especially as the point to be adverted to is mentioned in few if any books, and, though of manifest advantage, is rarely acted on. It consists in the introduction of a hook into one side of the trachea *before* opening it. By this means, the trachea is held steady, and the making of the incision much facilitated; and, what is of yet more importance, the wound made is afterwards easily kept open and prominent for the introduction of the canula. The many expedients which have, from time to time, been devised for the performance of tracheotomy, are evidences of the difficulties which attend it; but none have, as yet, been able to supersede the old method by the scalpel. We mentioned at page 267, in the 'Med. Times and Gazette' for Sept. 10, 1853, a case in which great disappointment attended an attempt to employ the trocar; and at page 418, for Oct. 21, 1854, is the narrative of a lamentable and fatal accident with Dr. Marshall Hall's tracheotome. The use of a hook, in the manner suggested, obviates to a very great extent the difficulties attending the scalpel method, and constitutes it by far the safest and most preferable. A common tenaculum will serve the purpose very well; but by some instrument makers a hook, especially adapted to it, is sold with the tracheotomy case. A case in which the operation was so performed will be found at page 136, in the 'Med. Times and Gazette' for August 5, 1854. We have since witnessed an operation, during which, for want of the hook, considerable difficulties were encountered.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 593.

122.—ON THE BRAIN.

By Joseph Swan, Esq.

It seems that all the fibres of the human brain must undergo some change for bringing them to a proper condition for activity and steadiness, and this is the reason why all manual arts should be begun early, and even intellectual pursuits, as they are with so much difficulty allowed to come to perfection in mature age. Every

fresh impulse requires one or more fresh fibres of the voluntary tract for its perfect reception, and thus so large a brain is necessary for the almost unbounded extent of the powers of the intellect of man; therefore, however hard he may work, and however much knowledge he may acquire, there is always room left for further stocks of information. He can occupy as much of his brain, however, as he pleases, and according to the degree of his industry, or idleness, will be his intellectual progress, so that the extent of his attainments rests with himself. By the necessity for such use and proceedings, he becomes possessed of a deliberative power of acting, and of using his muscles, and thus gains the form and character of his hand-writing, which he retains to the end of life, whether it be good or bad; the one has been perfected by care and attention, the other by slothfulness and negligence, unless bad instruction or some disorder has prevented the proper development of his powers. In the same way artists form and retain good or bad methods of drawing, and artificers excel or not, in their several branches.

A large brain is generally considered to be favourable to the extension of the intellectual faculties, but a small brain has also been found capable of promoting high attainments. The difference may depend on the quality of the fibres; but the small brain, without any difference of composition, may, through assiduous study, have been sufficient for allowing a great extent of knowledge, and as much as is usually reached by a larger brain, which is seldom exercised to its utmost limits. It is difficult to determine how small a brain is capable of as much intellectual power as will not be considered low enough for constituting an imbecile person or an idiot, until attempts at instruction have been prosecuted extensively enough for determining its capability of ministering to intellectual functions.

In early youth, the brain is not only the most susceptible of impression, but the most capable of retaining it. As life advances, the brain varies in respect of its powers, and loses much of its susceptibility of imprinting letters and words so that their memory may be permanent. It is then maturity of thought and judgment exercises the greatest power, and allows the mind to receive the highest qualities of information. If the two seasons are reversed, the advantages of either are gained with great difficulty and uncertainty. It is most probable that unless a person is gradually educated from his youth, he will not attain to excellence either in learning or the arts; and hence the great difficulty in changing his position and leaving off an accustomed business, which depended on manual dexterity.—*Monthly Journ. of Med., March*, 1855, *p.* 238.

123.—APPLICATION OF ELECTRO-CHEMISTRY TO THERAPEUTICS.

Chemistry is about to save from death, or a premature old age, those artizans whom the exercise of a cruel profession condemns to breathe metallic dust or vapours, who poison themselves daily for the sake of living, and acquire so many dreadful infirmities in the silvering of looking-glasses, the preparation of white-lead, or working in the mines. Science comes to the help of the victims of industry or pleasure, and extracts from their bodies, atom by atom, the devastating metal that had fastened on their tissues, and weighed on the springs of life. These hopes are drawn from a memoir presented to the Academy of Sciences by M. Dumas, and the authors of which—MM. A. Poey, of the Havana, and Maurice Vergnés—will hold a distinguished rank among the benefactors of mankind, if experience confirms their assertions.

The invention consists in the application of electro-chemistry to the cure of the diseases we have mentioned; and surely, of all its marvellous uses, this would be the most admirable.

M. Poey takes an unfortunate patient, corroded by lead, mercury, gold, silver, or any other metal, and places him in a metallic bathing-tub, isolated from the ground. The man sits down, his legs horizontally stretched out on a wooden bench, isolated from the tub, which is filled with water up to his neck. The water is slightly acidulated, to increase its conductibility; and the acid varies according to the cases. Nitric or hydrochloric acid is used for the extraction of mercury, silver, or gold; sulphuric acid for that of lead. This done, the negative pole of a pile is

brought into contact with the sides of the bathing-tub, and the positive pole placed in the hands of the patient.

The work of purification is now in full activity; the electrical current precipitates itself through the body of the sufferer, penetrates into the depth of his bones, pursues in all the tissues every particle of metal, seizes it, restores its primitive form, and, chasing it out of the organism, deposits it on the sides of the tub, where it becomes apparent to the naked eye.

In this great discovery, chance or accident has played a part. One of the inventors —M. Maurice Vergnés—occupied himself with galvanic gilding and silvering. His hands being in continued contact with solutions of nitrate and cyanuret of gold and silver, got covered with ulcers in consequence of the introduction of metallic particles. One day he plunged the diseased organs into the electro-chemical bath, at the positive pole of the pile; and, after a quarter of an hour, to the great surprise of the beholders, a small plate of metal brought into contact with the negative pole covered itself with a thin coating of gold and silver, extracted from the hands of the operator, whence the most powerful remedies had not been able to eliminate them. This discovery was made on the 16th of April, 1852.

The authors employ a pile of thirty pair of plates, approaching, at the same time, that of Bunsen and of Grove, as coke and platina enter into its composition, by which its action is rendered more energetic. Each pair has a diameter of 40 millimetres, and is 217 millimetres high. The number of the pairs to be used at the beginning of the operation depends upon the temperament of the patient and the nature of the malady. Thus a delicate and very nervous person is at first submitted to the action of ten or twelve pairs only, and every five minutes the number is increased. A person of a sanguine or lymphatic temperament is able to endure a greater number of elements. The same observation applies to the quantity of acid employed in the bath, less being required for a nervous than a lymphatic constitution.

The metallic atoms extracted from the body deposit themselves on the whole surface of the tub; but they are more abundant opposite to the part of the body where the metal was lodged. The size of the metallic spots varies considerably; some are microscopical; others have the dimensions of a pea; those of the size of a pin's head are very common. "I have seen," says M. Poey, "after the first bath of a person who complained of pains in the arms, from having taken mercury, the contours of the arm perfectly drawn upon the metallic plate by the deposit of metallic atoms that without doubt proceeded from the suffering member."

We shall terminate our article with an experiment made before the members of the Faculty of Medicine of the Havana.

A patient has undergone during a whole week an external mercurial treatment (frictions with mercurial ointment). He had then taken several lukewarm baths, and it could not be supposed that any mercury still remained attached to the skin.

He was put into a water-bath mixed with muriatic acid. After having remained in it for five minutes some of the water was taken out, and afterwards analysed by M. Baraceca, who found no traces of mercury in it.

The circuit was then closed; and, after the electric current had acted for about an hour, a new sample of the water was taken. Mixed with an alkaline sulphuret, the water became black; and a piece of copper having been dipped into it, gave sure signs of the existence of a small quantity of mercury. Thus the water of the bath now held mercury in solution.

During the experiment, a perfectly clean piece of copper had been placed at the negative pole. When it was taken out of the water, towards the end of the operation, its yellow-greenish colour not only testified an oxidation in which mercury had taken a part, but small white spots were scattered over the surface, one of which, of the size of a square line, was very brilliant, and of a mercurial whiteness. The plate having been heated underneath, the spot disappeared, and the original colour of the copper was restored, which proves that the spot was mercurial.—*La Presse.* —*Med. Times and Gazette, March* 3, 1855, *p.* 216.

124.—ON THE TREATMENT OF BUBO.

By Henry Thompson, Esq.

Counter-irritation over the swelling, whether threatening to suppurate, or in the act of doing so, has long been employed as a means of discussing the contents of bubo. Especially of late, however, new methods of accomplishing this desirable end have been tried, with variable success. Malplaquet's mode of applying a small blister to the inflamed surface, and dressing it afterwards with a strong solution of bichloride of mercury, for two or three hours, so as to induce a thin slough, has been attended apparently with benefit in some cases. Painting with strong tincture of iodine probably acts favourably, sometimes, on a similar principle, by the extent of inflammation of the skin which is set up, and not from any special power of inducing absorption peculiar to the iodine. The employment of a strong solution of nitrate of silver seems, however, to have afforded by far the best results of any of the methods we have yet had an opportunity of observing. The first systematic trial of this agent was made, we believe, at Guy's Hospital, and with very successful results. Mr. Henry Thompson has subsequently adopted it at the Marylebone Infirmary for a very considerable number of cases, in some of which very marked results have followed its application. In several of these, where fluctuation in the bubo has been so complete and unmistakable as almost to forbid any attempt to discuss it, on the ground of its apparent hopelessness, two or three applications of the solution, at two or three day intervals, have produced diminution in the size of the swelling, increased firmness to the touch, and evident consolidation; while a continuance of the treatment during another week or two has thoroughly succeeded in averting all fear of suppuration, and in dispersing the tumour. Mr. Thompson has applied the same treatment to abscesses of other kinds, when not depending on diseased bone, or in other obviously unsuitable cases, with a very considerable amount of success. In some instances where suppuration and discharge have occurred, there has been a manifest improvement in the condition of the abscess, as an apparent result of the application, and the subsequent progress of the sore has been much more satisfactory than that which these cases commonly present.

The solution which seems to answer best is, three drachms of nitrate of silver to the ounce of distilled water, with twenty minims of strong nitric acid. It may be applied with a glass rod or stick, although the best instrument is probably a glass brush. Freely rubbed over the entire surface of the swelling, and allowed to dry, it produces a thin, black eschar, which begins to peel off in a few days, when the application should be again made, and repeated as often as may be necessary. Where, however, it is more convenient to use the solid nitrate, an application of this to the affected part, previously wetted with a little water, answers every practical purpose. The principal advantage which the solution possesses, arises from the uniform strength which is thus ensured in the application.—*Lancet, March* 31, 1855, *p.* 340.

125.—ON PRESERVED MEAT-JUICE.

By Dr. Robert Christison, Professor of Materia Medica in the University of Edinburgh.

About eighteen months ago, when consulted in the case of a relative of Mr. Gillon, the extensive and skilful manufacturer of preserved meats at Leith, I found that the patient was entirely supported, in a severe illness, by the preserved juice of meat, which had been given at Mr. Gillon's suggestion. Observing the readiness with which it was taken when other food of every kind was refused, I was induced to try it in other instances, and eventually to employ it in various states of disease. The results led me to suggest the use of it to many professional friends, and to advise the druggists of Edinburgh to keep it; so that it is now much in request, and may be easily obtained.

This substance is the pure juice of beef, preserved in the way in which meats and vegetables are now so extensively preserved in the fresh state, for store provisions. The mode of preparation is as follows: Cylindrical cases of tinned iron are filled each with six pounds and a half of beef; and the lid is soldered on, but with a hole about half an inch in diameter in the middle of it. Two trays of such cases are

shoved into iron retorts, analogous in form to retorts for gas-making, but double-cased, so that steam may be introduced into the interstice around. They are thus subjected to a heat of 220° under steam pressure, for about three hours, by which the beef is partially cooked, and, being thus also made to contract strongly on itself, squeezes out a portion of its juice, amounting to a few ounces from each tin. The tins are then drawn, the juice is poured out, and the meat, with certain additions, is subjected to the preservative process. The juice, after being cooled, and entirely freed from fat, is put into small four-ounce tin cases. Each of these has a small aperture at one end, which is secured with solder, after the juice is poured in. The tins are then subjected, on trays, to a temperature of 220° in a muriate of lime bath. On being removed, the solderer rapidly touches with his iron the solder on the top, which giving way allows steam to rush out forcibly and carry with it the air in the upper part of the interior. By the time he has thus swiftly passed over sixteen or twenty tins, the first is ready for being well-soldered by a similar dexterous application of his iron, which then in succession as quickly secures the whole open and steaming apertures. The process of heating in the bath, tapping, and re-soldering, is then repeated a second time, to make sure of the thorough expulsion of every particle of air. The tins finally are painted to preserve them against rust.

The process is most perfect. I have repeatedly opened tins eighteen months in my possession, and stated to have been many months in store when I got them, and in every instance the contents had the rich delicate aroma and taste of fresh beef-juice. Sometimes the taste is slightly resinous or soapy, in consequence of a little resin having obtained admission in the operation of soldering. But as this does not occur often, the impurity may be avoided with due care. The juice may be taken with relish in small quantity, either cold or warm, in its concentrated shape; but it is rather strong to be used without dilution. When diluted with three times its volume of boiling water, and duly seasoned with salt and pepper, it makes a more palatable beef-tea than any which can be made in the usual way. Sometimes, indeed, a patient will be found to prefer the ordinary sort, either because the preserved juice has unluckily been resinous, or on the principle that leads some people from the plains of England to prefer hard water to the pure mountain springs of the primitive districts of Scotland, viz., because they are not accustomed to the finer sort. But this is not the general fact; and there can be no doubt that the preserved meat-juice makes a most palatable beef-tea, and an equally eligible basis for many soaps.

Until about ten years ago, in concurrence with general opinion, I used to regard beef-tea as a highly nutritive article, not to be rashly or freely given during disease. My sentiments in this respect were shaken, when I ascertained, in the course of some experiments for adjusting the dietaries of the General Prison and the Royal Infirmary, that a pint of the very finest beef-tea contained scarcely a quarter of an ounce of anything but water. Since that time I have much more readily listened to the cravings of patients for beef-tea in even many acute diseases, and above all in protracted subacute diseases, and in chronic diseases with fever; and I have thought I saw that it maintains the strength almost like wine, lessens emaciation and weakness in tedious diseases, and does not occasion any increase of reaction. There is no disease in which these properties are more remarkably shown than in protracted cases of gastric fever, of which, by the way, I have seen an unusual number both in town and country during the last three years. These cases have often lasted for six weeks, or—with a relapse, from too early indulgence or exposure,—for the long term of three months nearly; during which little, or absolutely nothing else, was taken, except beef-tea or diluted meat-juice; and without the attenuation and debility which so protracted a fever and want of appetite ought to have induced. In some instances I could scarcely doubt that life was preserved by this nutriment. It is unnecessary to particularise the various states of disease in which the same practice has been followed. It is peculiarly applicable to all subacute protracted diseases, whether febrile or otherwise; and in all such there is even no great reason to hesitate in resorting to it when local inflammation is present. Every one, I think, will be struck with the readiness with which such patients will often take diluted meat-juice or beef-tea repeatedly when they refuse all other kinds of food. It should be given in the quantity of a teacupful at a time, every four or

six hours; but it is well to alternate it with some other simple nourishment, when the patient will consent to do so.

What is its mode of action? Not simply nutrient. A quarter of an ounce of the most nutritive material cannot nearly replace the daily wear and tear of the tissues in any circumstances. Possibly it belongs to a new denomination of remedies, whose action never was even suspected to exist until recently—those which, by some peculiar influence, diminish the waste of the tissues under the exercise of their functions. Professor Lehmann has proved [Annalen der Chemie, 1853], that coffee possesses this singular property in so remarkable a degree, that in persons following an active occupation, an infusion of an ounce of roasted coffee daily will reduce the daily waste by a fourth part; and the same property seems likewise to belong to tea, and other restorative beverages. It is not improbable that the sapid and saline principles of meat, united in what is called ozmazome, and constituting the ingredients of beef-tea, and meat-juice, possess some such property. It is difficult otherwise to account for the interesting results obtained by the late Dr. Edwards in 1833, who, in his researches on nutrition,—strangely overlooked by the celebrated Gelatin Commission of the French Institute, in their condemnatory report on gelatin in 1841,—found that dogs die slowly if fed on bread and gelatin alone, but, when thus greatly reduced, quickly regain flesh and strength by the addition of two ounces of meat-tea, which cannot appreciably increase their textures by its own insignificant amount of solids. Either it acts as a digestive ferment, so to speak—promoting the assimilation of other nutriment—or, like coffee, it must lessen the waste of the tissues in the exercise of their functions.

Mr. Gillon's meat-juice contains only 6½ per cent of solids. As a mere nutrient, therefore, it is much in the same category with beef-tea. Sixteen ounces of beef-tea, made with the contents of one tin, yield only 114 grains of solid extract. It contains no fibrin, no albumen, no gelatin. It does not even gelatinize on exposure to the air for days: it is ozmazome, with the salts and sapid and odorous principles of meat, and materially different from all boiled extracts.

I should add, that no good beef-tea can be made so cheap as with this preserved meat-juice. A tin of four ounces makes sixteen of strong beef-tea. This much requires, in the ordinary way, a pound of the finest beef, which at present costs ninepence, and is scarcely ever so cheap as sixpence. The reason for the cheapness of Mr. Gillon's meat-juice is, that the residual meat is economised, while that of the ordinary cooking process is good for nothing.

It is a much more convenient article for use than any of the extracts made from meat by extemporary processes in the kitchen, or by certain very dubious chemical methods lately come into vogue. It differs materially from all meat-extracts prepared by boiling.—*Monthly Journal of Medicine, Jan.* 1855, *p.* 6.

126.—*On Ammonia evolved in Respiration.* By Professors VIALE and LATINI.—After a series of delicate experiments, conducted with the greatest care and minuteness, these eminent chemists have arrived at the following conclusions:—

1st. That ammonia is evolved in each act of expiration. They consider that a healthy man expires, in 24 hours, 0·763 grammes of ammonia; and, in a year, a quantity equal to 278·504 grammes of the gas. They calculate that, in a city containing 160,000 inhabitants, 44·56 kilogrammes of ammonia are evolved by respiration in the course of a year.

2d. That the ammonia is given off in the form of a sub-carbonate.

3d. That the lungs cannot expire *pure* carbonic acid gas.

4th. That the *azote*, pronounced by chemists to be a product of respiration, is merely ammonia.

5th. That this ammonia keeps up the supply of the gas existing in the atmosphere, which, dissolved in rain, falls upon and enriches the earth.

6th. That the active principle of contagion is probably merely an ammoniacal salt. Their reasoning on this point is rather curious:—The lungs, skin, and excretions all give off a considerable quantity of ammonia, and contagious epidemics are generally developed where a large number of people are crowded together within a small space, or where the emanations of putrifying animal substances are rife. The latter

emanations are always ammoniacal. Fumigations of chlorine, chloride of lime, nitric and acetic acids, are the antiseptics which decompose the ammoniacal salts, and probably also the contagious principle. Heavy rains and tempestuous winds have been known to arrest the course of epidemics of contagious diseases. Viale and Latini believe that the ammoniacal salts which constitute the principle of contagion may be united either to carbonic acid or to the metalloids (as sulphur, phosphorus, iodine, &c.), or to animal products, generating cyanogen, or to fatty bodies, fixed and volatile oils, or even to albumen, so as to act as a ferment.

7th. That during respiration a certain quantity of albumen is eliminated.

Professor F. Liebig, of Berlin, has published a letter in which he attaches great weight to the foregoing deductions, and considers the demonstration of ammonia in respired air as a fact of great scientific importance.—*L'Union Médicale.—Monthly Journal of Medicine, Jan.* 1855, *p.* 58.

127.—*Sulphuric Acid, versus Urine.*—Mr. James E. Huxley has pointed out that, in the wards of insane establishments which are wet and dirty, no scrubbing or scalding will effect more than a temporary improvement, as the floor becomes a reservoir for the perpetual exhalation of the volatile ammonia. But if dilute sulphuric acid be poured over such floors, in the proportion of one ounce of acid to twenty-four of water, and allowed to remain twenty-four hours, a white film (sulphate of ammonia) will form, which may be removed by washing, and will leave the room sweet. This principle is that of converting a volatile into a fixed salt that may easily be got rid of.—*Asylum Jour.—Monthly Jour. of Medicine, Jan.* 1855, *p.* 62.

128.—*Dropping Fluids from Vials.*—Dr. Coale has found a very simple and ready means of dropping fluids from vials. Unless the lip project very much, the fluid is apt to run back on the neck, and remain attached until several drops have accumulated. Greasing the neck of the vial (not the lip) will prevent this, and give a clean uniform drop.—*Proceedings of Boston Med. Society.—Monthly Jour. of Medicine, Jan.* 1855, *p.* 96.

129.—ON COD-LIVER OIL.

[In an interesting review of several little works on this subject in the "Edinburgh Monthly Journal" for April, the Editor remarks,]

Thus chyle is an emulsion formed by the union of oil and albumen derived from the food; and no one can doubt that out of that chyle or fatty emulsion, blood is formed. When, then, after giving the oil to emaciated persons, we observe that they become stronger, more muscular, and fatter, it requires little reasoning to prove that we must have added something to the chyle, through the chyle to the blood, and through the blood to the tissues. Again, when we examine chyle microscopically, we find it to be composed of exceedingly minute particles, or to consist of a molecular basis, which is soluble in ether, and therefore fatty; and that, as it flows from the intestines towards the blood, through the mesenteric glands, and, lastly, through the thoracic duct, blood corpuscles may be seen to form in it. Hence the molecular is a primary and evolving element, out of which the blood corpuscles and the blood itself are evidently formed. This view, modified as to one or more unimportant details, is the one which is in most accordance with physiology, and is most generally adopted.

This theory still finds many objectors, the latest of whom is Dr. Black, who says of it—

"It may be urged that, were fat a 'primary and evolving element,' as considered by Dr. Bennett, it would not be diminished in quantity as it flows along the lacteals; that nature would not thus all but destroy the very resources whence the nuclei of the tissues are to be drawn; that it would exist in greater proportion in the blood; that the quantity found in that fluid is manifestly insufficient to form with albumen the necessary nuclei for the proper nutrition of the tissues; that it would constitute a constituent part of healthy muscular fibre, and of other structures having a

cellular basis of formation; and that, when found as a constituent part of such tissues, it would not be, as it invariably is, associated with disease. It seems, therefore, more consonant with facts to regard fat, absolutely considered as such, as taking no part in the formation and nutrition of tissues, than to look upon it as a 'primary and evolving element' of such tissues."

Here Dr. Black has evidently misunderstood what is meant by "primary and evolving element." In a note he tells us that he supposes it to mean an element which enters directly into the formation of tissues. But this, so far from being the primary, is really the ultimate element of histogenesis. By the primary element, histologists comprehend the first morphological appearance which presents itself, and this is everywhere a mass of minute molecules and granules which, by uniting together, constitute primary nuclei, around which primary cells are formed. All these elements, however, are but temporary, and undergo successive elaborations, evolutions, and reformations, before they directly enter the tissues. The argument advanced by Dr. Black, namely, that fat is not a primary evolving element, because it diminishes as it flows along the lacteals, is exactly that which every histologist employs to show how, by its transformations, it becomes prepared to form tissue. That oil may serve to form albumen, and that albumen may be transformed into oil, nobody can have any doubt who watches in an ovum the development of an embryo. Indeed, although we are willing to pay every attention to Dr. Black's objections, and to give him credit for sincerity in expressing his opinions, it is impossible to overlook the fact, that he is not familiar with the great progress recently made in histology and embryology. He has been led away too far by the hypothesis of germinal centres, and, from what he has advanced, does not appear to us sufficiently to appreciate what may be called the molecular theory of morphology.

Passing, however, from these speculations, it may be asked, how is it that, if cod-liver oil operates in virtue of its oily or fatty matter, that any other oil or fat does not answer the same purpose? Now the fact is, under ordinary circumstances, the stomach and alimentary canal are capable of reducing all kinds of fat to the required fluidity and purity necessary for the formation of chyle. But there are a certain class of cases where the digestive powers are evidently impaired, and cannot do so. We shall not at present enter into the theoretical views by which some have endeavoured to account for the peculiar form of dyspepsia in these cases, because we should thereby be led away too far. We may safely assume that there is such indigestion, and in the vast majority of cases a remarkable antipathy to eating all kinds of fatty, or, as it is called, rich food. It has indeed been asserted—and we find it is so by Dr. Black—that tubercle frequently occurred without the slightest symptom of a dyspeptic character being present. If by this be understood that individuals who are not habitually underfed (as often occurs among the lower classes) become tubercular and waste away, whilst the appetite remains good and digestion is perfect, we will venture to say the assertion is altogether opposed to experience. The statements of patients, indeed, would often lead a superficial examiner to conclude that their appetite and digestive powers are every thing that could be wished; but we need scarcely say that the hopeful and flattering accounts given by consumptives of themselves are not likely to deceive the sagacious physician, still less to be received by him as an argument for or against any particular theory. Our own notion, therefore, is, that in giving animal oils, we introduce an essential element of nutrition, already so prepared that it is rendered easily assimilable. Of this we are convinced, that it is in proportion to its capacity of being digested, and of forming good chyle, that its good effects are rendered obvious.

But why should cod-liver oil be more assimilable than any other kind of oil? This is a difficult question to answer. The vegetable oils, indeed, are more or less purgative, and have been largely tried, without producing any benefit. Many other animal oils have been employed, and, it is said, with some effect—for instance, neat's foot oil. We have given scate, shark, and dog-fish oil—for samples of which we hereby thank the donors—all with good effect; and we believe that these oils are little, if at all, inferior to cod-liver oil. We have also given various kinds of genuine cod-oil, which have been kindly forwarded to us from various parts of this country, and from abroad, and have especially to thank Mr. Archibald of Newfoundland, Mr. Fox of Scarborough, and Mr. Hansard of London, for specimens recently forwarded to us.

We have no difficulty whatever in speaking of the rival claims of the different manufacturers of this substance, because we believe—in accordance with the views previously stated—that they are all good, and are medicinally of equal value. Yet these oils vary greatly in their physical properties. Here in cannot in justice forbear mentioning that the Messrs. Parker, oil manufacturers in Leith Walk, have for many years—indeed long before Dr. C. J. B. Williams spoke of the advantages of giving a pure oil—manufactured it as pure and tasteless as has been done by anybody. It is unnecessary for them to make their oil known by advertisements, for the simple reason that every drop they can produce is eagerly purchased by the druggists of Edinburgh. They were the first, as they are still among the best and cheapest producers of this substance. To meet the increased demand in England, however, an excellent, pure, and almost tasteless oil has been brought into the market, and that of Messrs. Archibald and Fox has in these respects certainly never been surpassed. Those persons, therefore, who find, from experience, that they can take with greater ease those light and comparatively tasteless oils, may with great confidence employ any of them.

The notion, however, that a pale oil is superior to the brown kind, is most erroneous. Throughout Germany and the northern kingdoms of Europe the brown oil is preferred, and the assertion so readily made, that its more nauseous smell and taste prevent its introduction into practice, or interfere with its assimilation, is entirely opposed to the most extended series of facts. We can declare, from the result of no small experience, that not unfrequently the brown oil is retained on the stomach when the light oil is not, and that children more especially, contrary to what may be supposed, in many cases prefer the brown to the more tasteless kinds. Many describe the brown oil as stimulating and acting like a cordial. The conclusion we have arrived at then, with regard to the superiority of one or other kind of oil, amounts to this, that that is the best which is most easily taken, retained on the stomach, and digested, and when we have to do with a capricious organ, experiment alone can decide which that shall be.

It is only right, however, to give Dr. de Jongh's view of the action of the oil, which is as follows:—

"Our analyses have shown that cod-liver oil is a very compound remedy. We find in it fatty matters, biliary principles, iodine, phosphoric acid, phosphorus, butyric acid, gaduine, and several inorganic salts. To which of these principles, many of which are recognised as very active remedies, does cod-liver oil owe its salutary action in rheumatismal and scrofulous affections? Is it to the iodine, to the fatty principle, to phosphorus, to other matters, or to the action of all these principles united? If we consider that, in the diseases in which cod-liver oil exhibits an incontestible value, the digestion is augmented, the nutrition ameliorated, the secretions excited, the function of the lymphatic system revived, and lastly, that the ganglionic system is affected, we shall be easily convinced that it is not the biliary principles, not the fatty matters, not iodine, or any other single one which can meet all these indications. Hence why we attribute the efficacy of cod-liver oil, as much to the combined action of all these principles as to that of many among them."

This is certainly an ingenious way of avoiding a difficulty. Dr. de Jongh avows his want of power to resolve the question of how one or more of these principles act, and evidently regards cod-liver oil as a mixture made up of certain precious ingredients, each of which has its destined, though undiscovered object, while the whole of them unite in producing great effect. Hence, only the oil that contains all these ingredients is a good oil, and as they are present in the brown kind as determined by analysis, this is the oil to be preferred.—*Monthly Journal of Medicine, April*, 1855, *p.* 317.

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130. *Phosphate of Lime.*—Dr. KUCHENMEISTER recommends the following formula in cases in which phosphate of lime is indicated:—Calcis phosphat. 3ij.; calcis carbon. 3j.; sacch. lactis, 3iij. M. 3ss. bis terve in die. Instead of the milk sugar, lactate of iron may be substituted, if iron be required. The especial use of the carbonate of lime appears to be that carbonic acid is liberated by the acid of the stomach, and dissolves the phosphate. Lactic acid also is formed from the sugar, or is set free from the lactate of iron, and dissolves the phosphate. The most ready way of absorption is, however, when the phosphate is given with food, especially with milk,

with which it forms a soluble combination.—*Schmidt's Jahrb.—Monthly Journal of Medicine, April*, 1855, *p.* 345.

131.—*On the addition of Tannate of Lead to Adhesive Plaster.*—M. Herpin states, that his own experience teaches him that Baynton's treatment, by strapping, may be extended to every branch of surface, whether resulting from wound or ulcer; and the only inconvenience he has found attending it is, the production of eczematous eruptions, or vesications in irritable skins. After having tried various means of obviating this, he remembered the great advantage that accrued from the treatment of bed-sores, by means of plasters powdered with the *tannate of lead.* He caused some of this substance to be combined with adhesive plaster, which henceforth produced no irritation. As the addition of the tannate diminishes the adhesiveness, the proportion may vary accordingly, as this quality is desired or not. It is retained when 1-20th of the tannate is added, and when not much required the proportion may be raised to 1-12.—*Bullet. de Thérap.—Med. Times and Gazette, April* 7, 1855, *p.* 346.

132.—*Gelatine Paper.*—Mr. Dobell has called the attention of the Royal Society to Gelatine Paper as a medium for colouring light, likely to be useful in many employments, and in cases of weak sight. This kind of paper, which was first invented at Rouen, in 1829, is now produced in great perfection; it is highly transparent, and in sheets measuring sixteen inches by twenty-two, but can be made, if required, of the dimensions of the largest plate glass. These sheets, moistened with a solution of gelatine, may be stuck on the panes of a window, and thus change the light admitted to any required colour. A green light, falling on the white silk made up by dressmakers, deprives it of all its painful glare; and in the same way, yellow silk is made to appear green by a blue light, as has been proved by actual experiment, and it is attended with the happiest results. Jewellers who have tried the green paper, say, that when once accustomed to working in a coloured light, they find it greatly relieves their eyes. In reading, too, a sheet of the green paper laid on the page preserves weak eyes from being injured by the strong contrast of black and white, and enables many to read with comfort who have been hitherto obliged by too susceptible vision to abstain from books. Other applications of gelatine paper naturally suggest themselves; it may be used as screens and shades for many purposes; the glasses of spectacles may be coated with it; gardeners may use it in their conservatories; and the yellow will probably be taken into their service by photographers. By the addition of a small quantity of acetate of alumina during the process of manufacture, the gelatine paper becomes waterproof, just as linen or woollen cloth is rendered waterproof by the same chemical substance.—*Chambers's Journal.—Med. Times and Gazette, Jan.* 6, 1855, *p.* 23.

133—*Chlorate of Potass in Mercurial Salivation.* By MM. Herpin and Blache.—Encouraged by the success of Hunt, and others, in the employment of the chlorate of potass in the treatment of gangrenous affections of the mouth, M. Herpin has repeatedly employed it with the best effect in mercurial salivation, giving it in the dose of 3ss. to 3i. per diem—the cure, when the case is taken early, being completed by the fourth day. He has long been accustomed to the employment of this substance, as recommended by Odier, in jaundice, whether simple or complicated, with engorgement of the liver. Odier carried the daily dose as high as 150 grains.

M. Blache also furnishes us with the results of his experience with this substance at the Hôpital des Enfans. He found that, while cases of *ulcero-membranous stomatitis*, treated by the fuming chlorhydric acid, or the chloride of lime, required a mean duration of treatment of twenty days, those treated by the chlorate of potass were quite cured in five or six days. In *mercurial stomatitis* he has found it highly serviceable. He gives from 30 to 60 grains, in a gummy julep, in the twenty-four hours—a small dose, indeed, compared to the 300 or 450 grains prescribed by M. Socquet, of Lyons, in acute rheumatism.—*Bulletin de Thérap.—Med. Times and Gazette, March* 24, 1855, *p.* 289.

134. *Etherial Solution of Nitrate of Silver.*—Every one who has tried it is aware that the application of a watery solution of the nitrate of silver over a large extent of skin is a troublesome and patience-requiring process. The fluid does not dry quickly, and it runs about, being prevented, by the greasiness of the skin, from being rapidly absorbed. A plan which we see adopted by Mr. WARD in the London Hospital obviates very completely these difficulties. It consists in making the solution with the common nitric ether, instead of water. The ether acts as a solvent of any sebaceous matter which may be on the skin, and, from its volatility, very quickly dries in, producing, at the same time, a sensation of coolness very agreeable to the patients. If wished, several coatings may be applied successively to the same part with the loss of but little time. The strength which Mr. Ward generally employs is eight grains to the ounce, but it may, of course, be modified according to the wishes of the surgeon. The use of nitrate of silver externally in erysipelas and other low forms of inflammation of the skin, is a very favourite practice in the London Hospitals,—either an aqueous solution, or the solid stick moistened, being usually employed. Mr. Ward informed us, that the use of the etherial menstruum was not original, but had been suggested to him by a gentleman by whom he had been consulted.—*Dublin Hospital Gazette, Nov.* 15, 1854, *p.* 320.

135.—ON A PRESERVATIVE MEANS AGAINST THE SYPHILITIC VIRUS.

By M. A. RODET.

[*Syphilization*, as it is called, has failed as an antidote to this poison, so also has *syphilitic vaccination*. M. Rodet thought that some neutralising liquid might be discovered.]

For some time I had been occupied in watching the effects produced by different chlorides employed in the dressing of chancres and ulcerated buboes, and I remarked, that, of all remedies, the chloride of zinc possessed the most remarkable properties. Dissolved in thirty to forty times of distilled water or alcohol, it powerfully modifies the surface of chancres, transforms them rapidly into simple sores, especially when elevated, and induces a cicatrisation in a very few days. It has no effect on the skin when the epithelium is intact, and it passes through the least fissures.

Dissolved in eight times its weight of distilled water, and applied to a puncture recently inoculated, it destroys the virus and prevents the formation of a chancre; but as it slightly cauterises the interior of the puncture, there forms around, at the end of two or three days, a slight inflammatory zone, whence results a simple pustule, which ordinarily lasts six to nine days.

The chloride of zinc, therefore, does not fulfil all the desirable conditions; it protects, but it cauterises.

The iodide of zinc, the chloride of cadmium and barium produced analogous effects; they protect, when the solution is sufficiently concentrated, but they give origin to simple pustule. The chloride of iron does not cauterise the punctures, but it does not protect, nor does it ever retard the progress of the virus. Whatever may be the amount of the concentration, no other result is obtained. The preparation, however, appeared to me to be valuable, and I therefore endeavoured to give it, by combination, those properties which it did not naturally possess. After some experiments, the problem seemed to be solved. Having applied to an inoculated puncture a solution of perchloride of iron and citric acid, protection was obtained in a most undeniable manner. I repeated the experiment a certain number of times, and always with the same result, when I was suddenly stopped by one of those unforeseen occurrences which disturb most scientific investigations.

The sample of perchloride of iron which I had hitherto used being exhausted, I procured another, which I found not to possess the same properties. No preservation was obtained, neither with the same nor with stronger doses. I tried in vain to procure a specimen similar to the first, and, from constant failure, was beginning to doubt the accuracy of my first observations, when I discovered two little vessels of the original perchloride in my cabinet. The results here obtained were precisely the same as at first.

I now proceeded to investigate the nature of the differences between the two sets of preparations. The perchloride first used was quite soluble in distilled water; the others threw down a deposit. By adding to the new samples a sufficient quantity of hydrochloric acid to render them completely soluble, a fluid was produced exactly similar to that which had in my early experiments proved so efficacious, and therefore it was necessary to determine—

1. What are the doses and the compositions which preserve the best without irritating the sound tissues?
2. What is the most simple and efficacious manner of employing the remedy?
3. What are the appreciable effects upon the punctures of inoculation?
4. What is the duration of time during which the remedy possesses a preservative power, and what are the modifications which its effects produce at different periods from the insertion of the virus?
5. What are the causes which make the preservative effects of the remedy vary?
6. What are the other applications of which the remedy is susceptible?

From the very numerous experiments which have been instituted, the following propositions are laid down. The best formula is,—

Distilled water . . . . 32 grammes = ℥j. + ℥iss.
Perchloride of iron, Citric acid . . . . , Hydrochloric acid } aa. 4 grammes = (approx.) ℥j.

A similar fluid may be made by omitting the citric acid and augmenting the dose of the hydrochloric acid one-third. The most simple method of employing the remedy consists in letting fall one drop upon the part inoculated by the virus, and in letting it remain there ten or fifteen minutes; or by applying to the part a piece of lint soaked in the fluid. If the contact of the liquid is of too short duration, the preservation is incomplete, and an ulcer forms, which advances slowly, and may be regarded as an imperfectly-formed chancre.

If the lint or charpie be applied for the space of one hour the preservation is complete. A shorter time might suffice, but no inconvenience would result if the fluid were allowed to remain twenty-four.

At the moment of application the patient feels a smarting sensation, which lasts but an instant; a minute afterwards the puncture becomes raised so as to form a pimple, and spreads from the centre to the circumference, so as to resemble a gnat bite. In about half an hour it ceases to extend. In two hours it begins to disappear, and shortly afterwards there is no trace left. This elevation is a certain proof that the fluid has penetrated the puncture, and permeated the subjacent tissues. Preservation may be obtained even when the virus has produced no appreciable effect upon the spot where it has been applied. At the end of two, four, or six hours, its effects are as complete and lasting as if it were applied immediately after inoculation. Should the inoculation have produced sensible effects, such as the formation of a pimple or of a pustule, the absorption of the liquid goes on badly; consequently, the patient is imperfectly protected. Cauterisation with a piece of nitrate of silver is then the only sure method. The effects of the preservative liquid may be modified, not only by the doses of the active substances which enter into its composition, and by the duration of its contact with the contaminated parts, but also by the degree of activity of the virus employed. Weak doses have preserved in some instances, and in others produced unsatisfactory results. Hence it may be inferred, that the syphilitic virus is the more energetic in proportion as the ulcer which it produces is more recent.

The liquid acts beneficially upon simple ulcers by causing a puriform secretion when deficient.

The vaccine virus is completely neutralised by this liquid, and perhaps its opportune application to those parts of the body where cicatrices are a source of disfigurement in small-pox, may be attended by a similar good effect; the poison may be neutralised and the progress of the pustule stopped.

Finally, can this liquid neutralise the poison constituting the disease known as hydrophobia as well as that of syphilis or small-pox? If experience should reply in the affirmative, science will have made an important step. As the remedy does not cauterise the tissues, it may be freely applied to the bitten parts, even in event of there being a shadow of suspicion that the animal was diseased. Cauterisation is

at the best a painful mode of treatment, from which people shrink in terror. To be effectual it must be very extensive, and it has failed even when applied by experienced hands.

In thus making public a mode of neutralising the syphilitic virus, I have performed a sacred duty. I trust that it may be permitted me to follow up its application, and to be the means of throwing a veil over one of the most hideous sores of society. Should it prove a successful plan, and become received with favour, I fancy that I hear already from afar the murmur of the reproach of immorality. Should such a cry be raised I shall rebut it with all the energy in my power. That which is immoral is the debauch, depraved habits, promiscuous intercourse; it is, in short, all that can render necessary the employment of prophylactic measures. It is an act of immorality on the part of the surgeon to hold in his hands the means of preventing a great evil, and from some motive to neglect to use it. Medicine resembles charity; it should do good with the head turned aside; its sacred duty is to cure evils, from whatever source they emanate, and to protect mankind as well from syphilis as from any other scourge.—*Gazette Hebdom., Jan.* 12, 1855.—*Med. Times and Gazette, Feb.* 10, 1855, *p.* 142.

136.—*New Broth for the Sick.*—[Dr. Thudicum exhibited before the Medical Society of London, a new broth for the sick.]

To prepare this broth, half a pound of the flesh of a recently-killed animal (beef, or the flesh of a fowl) is chopped fine, and well mixed with a pound and an eighth of distilled water, to which four drops of pure muriatic acid, and from half to a drachm of common salt, have been added. After an hour, the whole is thrown on a common hair sieve, and the fluid is allowed to run off without pressure. The first portion, which is turbid, is poured back, until the fluid runs off quite clear. On to the fleshy residue in the sieve half a pound of distilled water is thrown in small portions. In this way a pound of fluid (cold extract of meat) is obtained, of a red colour, and an agreeable taste of broth. The sick are allowed to drink a cupful cold at pleasure. It must not be heated, as it then becomes turbid, and deposits a thick coagulum of animal albumen and hæmatin. The broth possessed great advantages over other preparations of meat, from containing albumen, and being remarkably easy of digestion. Cases were referred to in which its efficacy in this respect had been tested. Some conversation took place on the advantages which this broth possessed over concentrated beef-tea, &c.—*Lancet, Dec.* 16, 1854, *p.* 508.

137.—*On Collodion.*—Different proceedings have been instituted to give to collodion a greater amount of elasticity, with or without increasing, by the addition of certain substances, its therapeutic properties. Although experience has not yet pronounced upon the value of these trials, we give the principal forms of preparation.

1. *Elastic Collodion.*—Three formulæ are indicated: the first is due to M. Robert Latour, who employs impermeable coverings for the cure of local inflammations.

Collodion ʒ iss., Venice turpentine gr. iii.; castor-oil ʒ ii.

The fluid must be spread in a tolerably thick layer, by means of a brush over the affected part.

A simpler but less solid form is the following:—

Collodion, 40 parts; castor-oil, 5 parts. Mix.

The castor-oil may be replaced by glycerine.

M. Semoine (Bulletin de Thérapeutique) advises the employment of collodion and bird-lime (*collodion à la glu*). The difficulty is, however, to obtain the lime in sufficient purity.

2. *Medicated Collodion.*—The good effects following the application of this substance to affected parts of the skin has induced M. Aran to recommend—collodion, 100 parts; tinct. of the perchloride of iron, 100 parts. Mix.

Dr. Hannon, of Brussels (Presse Médicale, 1850), recommends the combination of collodion with the acetate of lead, by pouring, drop by drop, an ounce and a-half of collodion into warm concentrated alcoholic solution of neutral acetate of lead, continually stirring up the mixture. The saturnine collodion is very supple; and,

according to the ideas of the author, participates in the properties of the two principal constituents. It seems doubtful whether these mixtures possess qualities superior to simple collodion.—*Archives Gén.—Med. Times and Gazette, Dec.* 9, 1854, *p.* 597.

138.—*External Employment of Aconitine.*—[In a case of painful stump, under the care of Mr. HILTON, in Guy's Hospital, this remedy was found useful.]

Ever since the amputation he has suffered most severely from neuralgia of the ulnar nerve, which has quite prevented him from using the stump. The pain was also increased by the least motion. Mr. Hilton ordered the application of an aconitine ointment over the course of the nerve. The ointment consisted in two grains of the alkaloid to an ounce of lard. Very great benefit speedily resulted, and when the man left the hospital a short time afterwards, the pain had been so completely relieved, that he could use his arm most actively without producing any uneasy sensation. The cure, however, was not permanent, and a month after his discharge the man was re-admitted. Mr. Hilton did not think it advisable to trust him to use the ointment continuously, and as nothing less seemed to keep the pain in abeyance, it was decided to excise a portion of the ulnar nerve. The operation was performed last Tuesday, a little below the elbow, an inch and a quarter of the nerve trunk being cut out. The case is of value, as showing what the effects of the ointment might be in cases in which the neuralgia was due to a transitory cause.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 614.

139.—*On the Immediate Cause of Coma and Insensibility.* By Dr. SNOW.—In a paper read before the Physiological Society, Dr. Snow said that these phenomena were met with under a variety of circumstances; but that they always, in his opinion, were caused by the interruption of the process of oxydation of the brain and nerves, which was necessary to consciousness, sensibility, and all the other animal functions. The effect of respiration, which was a process of oxydation, was very evident on the fœtus, the moment it began to breathe the air, in exchange for the imperfect oxygenation of the blood previously effected through the placenta. In asphyxia, the privation of oxygen caused coma and insensibility, whilst the heart still continued to beat for a time. Narcotics produced coma and insensibility by diminishing oxydation, through a counter affinity which they possessed for the oxygen, as he had endeavoured to show on a previous occasion. The following were the chief facts, which proved that narcotics acted by diminishing oxydation in the system. He had found that the quantity of carbonic acid given off from the lungs was diminished during the influence of chloroform and ether. Bocker had found that the quantity of all the constituents of the urine was diminished by alcohol and by some other narcotics. The temperature of the body, which always bore a direct relation to the consumption of oxygen in the system, was lowered during the insensibility caused by narcotics. Narcotics had most of them the power of preventing oxydation out of the body. Putrefaction was a process of oxydation in its commencement; and nearly all narcotics were antiseptics, their antiseptic power usually bearing a direct relation to their power as narcotics. The vapours of volatile narcotics also had the property of preventing that kind of oxydation which constitutes ordinary combustion. Some parts of the nervous system seemed to have a greater affinity for oxygen than others, as they were able to perform their functions with a less amount of that gas. It was on this account that patients were able to exist in a state of coma and insensibility without dying. In privation of air, and also under the influence of narcotics, the functions of the cerebral hemispheres, and of the nerves of common sensation, were first suspended, coma and insensibility being induced, whilst the respiratory movements and the action of the heart continued. By further privation of air the respiratory movements ceased, from the functions of the medulla oblongata and the nerves of respiration being suspended, whilst the heart continued to act; showing that the ganglionic system of nerves continued to perform their functions. Under the influence of narcotics, also, the action of the heart usually survived that of the muscles of respiration, when these agents were not very rapidly introduced into the circulation. The circulation of the blood was

of course necessary to convey oxygen to any part of the body; and it was by the interruption of the circulation in the brain that the diminution of oxydation was produced which caused the coma of apoplexy and epilepsy. The symptoms of apoplexy arose sometimes from extreme congestion, and sometimes from anæmia of the brain, but in either case the circulation was much interrupted: whilst in effusion the yielding coats of the vessel were the first to feel the pressure.

Mr. Headlam objected to Dr. Snow's views, as being more complicated than the simple view of a direct action on the nervous system. In some cases where narcotics were exhibited there was excitement and delirium, in lieu of coma. The diminished oxydation present during narcotism might only be coincident with, and not a cause of the comatose condition.—*Lancet, Dec.* 23, 1854, *p.* 530.

140.—EXTERNAL USES OF THE ACID NITRATE OF MERCURY.

A solution of the nitrate of mercury in strong nitric acid is in very common use at the Hospital for Cutaneous Diseases, and constitutes a very convenient form of caustic. Its formula is—℞ Hydrargyri ʒj., acidi nitrici (specific gravity 1·50) ʒij.; solve. The solution produced is a clear, colourless fluid. The following may be mentioned as some of its chief uses.

In Carbuncle.—Mr. Startin usually applies the caustic if the carbuncle be of not more than moderate size, to but one central spot, where it is freely painted for an extent of about a shilling in size. Its effect is to produce an eschar, from beneath which the core afterwards escapes.—[See page 189 of this vol.]

In Acne.—A very minute drop of the acid is placed, by means of a finely-pointed glass brush, on the apex of any indolent tubercles, whether suppurated or otherwise. It has the effect of opening the pustule, if matter have formed, and if not, induces the disappearance of the induration. The application is followed only by a little smarting pain, and if it have been carefully made leaves no scar. For an account of the constitutional and other measures which are adopted simultaneously in the treatment of this disease, the reader is referred to the 'Med. Times' for Oct. 24, p. 394.

In Boils.—There can, we think, be little doubt as to the superiority of the caustic treatment over that by the knife, even in the case of very large boils. The pain of the incision, the large sore caused, and the unsightly scar which follows, constitute very formidable drawbacks to a practice for which there is no real necessity. At this hospital, where cases of boils are very common, the knife is never resorted to. The general treatment consists in giving aperients and steel conjointly, and the local in applying to the apex of the furuncle a full-sized drop of the acid nitrate solution. The morbid action generally terminates coincidentally with the application, and the core is thrown off through a comparatively small opening, the resulting cicatrix being insignificant.

In Lupus.—The acid nitrate is one of the most efficient and convenient forms of caustic in this disease. Mr. Startin does not, however, employ it solely, but uses also the biniodide of mercury, and a paste of which arsenic is the principal ingredient.—(See page 11, for July 1). The acid nitrate is chiefly used in indolent tubercles, and to indurated patches not actually ulcerated. After ulceration has occurred the arsenical paste is preferred.

For Sloughing Ulcers.—The practice of treating unhealthy ulcerations wherever situated, by means of caustics, is much pursued at this hospital, and with excellent results. The pain attending the application of nitric acid has been much overrated by the profession generally, and its use has consequently been avoided in many instances in which it would have been efficient to completely change the course of the morbid action, and induce healthy processes. Its powers in cases of phagedæna are now widely recognised, and its use will probably soon extend to various other kinds of ulceration of somewhat similar nature, but much less severity. The pain spontaneously caused by an unhealthy sore during a single night is probably much more than that produced by an application of caustic. In most cases of sloughing or unhealthy ulcers, Mr. Startin employs either the solution of the acid nitrate or the arsenical paste just referred to. The rapidity with which the surface granulates afterwards is often surprising.

In Moles, Nævi, &c.—Small moles on the face, if superficial and not too thick, may be readily destroyed by the acid nitrate. A cicatrix of course results, but it is small, and far less unsightly than the original disease. Small cutaneous nævi are often treated both at this and the various other London hospitals, by means of the nitric acid. Unless the disease be of very small extent, the employment of a ligature appears to be a much more certain means of effecting the end desired. If there be a subcutaneous base to the morbid structure it often persists in growing, despite frequent applications of escharotics. There is a mild form of dilated cutaneous capillaries which produces the marks known as "port-wine stains," "spiders," &c., in the treatment of which much benefit may be obtained by the dexterous application of fluid caustics. With a finely-pointed glass brush, charged either with nitric acid or the acid nitrate of mercury, the tortuous vascular trunks should be severally painted, a minute streak of the caustic being thus left along the whole course. In this way, by repeated applications, the whole of the larger vessels may be destroyed, and the disfigurement, to a large extent, diminished. The "port-wine stain" is of course very much more difficult to remove than the less diffused forms of this condition, such, for instance, as are of frequent occurrence on the cheeks or nose; even in it, however, much benefit may, by patient treatment, be gained.—*Med. Times and Gazette, Jan.* 6, 1855, *p.* 9.

141.—*Therapeutic Action of Lupulin.* By ZAMBACO.—Debout's observation on the favourable influence of lupulinin quieting painful erections induced the author to institute some experiments with the medicine, which, to prevent mistakes, he administered to the patients with his own hands. The form was threefold; pure lupulin, the tincture, or mixed with an equal part of sugar. The doses were different: of pure lupulin, 1—16 scruple (these high doses caused no disturbance of the nervous system;) the tincture, 2—16; the mixture with sugar, 2—3. From eight well-marked cases of painful erection, consequent on gonorrhœa, out of a much larger number, the author was enabled to draw the following conclusions:—Lupulin possesses a remarkable effect upon the genital organs; the erethisms were quieted in four-fifths of the cases. It is of therapeutic utility when there exists the necessity for keeping the penis in a state of rest. Camphor given in large doses, with a similar object, both irritates the alimentary canal, and often fails in its end. The sedative and anti-blenorrhagic influence of lupulin depends upon a resinous, etherooily principle; while the bitter principle yields a real tonic. The author has seen lupulin given with good effect in scrofula.—*Med. Times and Gazette, Feb.* 3, 1855, *p.* 118.

142.—MOVABLE TUMOURS OF DOUBTFUL NATURE.

[We sometimes feel a kind of gratification when we find the eminent men in London puzzled with a case, knowing that we have so often found ourselves in the same predicament. It is very consolatory in misfortune to find ourselves in respectable company—with these feelings we give the following cases:]

Case 32.—*Hard, Movable Tumour the size of a Fist in the Right Hypochondrium, remaining without Alteration for Seven Years.*—A healthy-looking young woman, aged 28, unmarried, was under the care of Mr. PAGET, in St. Bartholomew's Hospital, in Sept. 1852. In the left side of her abdomen, immediately beneath the border of the ribs, was a hard lump about the size of a large fist. It was firm, obscurely lobulated, and not very accurately defined. By pressure, it was easily forced deep into the abdomen, or pushed either downwards quite into the pelvis, or upwards under the ribs, none of which movements occasioned the slightest pain. When the hand was removed, it always rose to its former situation, which was apparently in close apposition with the abdominal parietes. The patient had known of its existence for five years, during which period it had remained stationary as to size, and other characters. She suffered only from dyspeptic symptoms. The case is detailed in the Med. Times for December 25, 1852, page 642. Since that date, we have repeatedly seen the patient, the last time being a few months ago. The tumour has remained precisely *in statu quo*, and, excepting some chlorosis and palpi-

tation of the heart, the woman continues in good health. At St. Bartholomew's, the case has been examined by many surgeons; but no opinion as to the nature of the tumour has been hazarded.

Case 33.—*Hard Movable Tumour the size of a Fist in the Right Hypochondrium, remaining without Alteration for Seven Years.*—A delicate looking woman, aged 38, has attended the City Hospital for Diseases of the Chest, under the care of Dr. RISDON BENNETT and Dr. BIRKETT, at different periods for the last three years. She suffers from frequent attacks of bronchitis, but the main interest of her case consists in the presence of a tumour in the abdomen, very closely resembling in size, in shape, and hardness, a small orange. In September, 1852, it was so freely movable, that it could, without difficulty, be carried from the left side of the umbilicus into the right iliac region, thence upwards into the right hypochondrium, then across the hypogastrium, to be ultimately lost beneath the margin of the ribs on the left side. On being left for a few minutes, the mass always rose to the position it had at first occupied. It was not in the least tender. When examined in October, 1853, it was found to be movable in an area of about two inches around the umbilicus, but not further; its size, &c., were just as before. The woman is at present attending the Hospital, and during the last year no alteration in the condition of the tumour has taken place. She is not in materially worse health than she was three years ago. The history of the existence of the tumour now extends over a period of twelve years, during which it has not increased in size. For further details see the Med. Times for December 18, 1852, p. 621.

Case 34.—*Large, Firm, Movable Tumour in the Right Side of the Abdomen, with History of Existence for Ten Years, and much Increase of late—Patient in Perfect Health.*—In October, 1853, a girl, aged 20, stout, florid, and in apparently good health, was under the care of Dr. WALSHE, in University College Hospital, on account of a tumour in the right side of the abdomen, having the following characters. When placed on her back there rose to the right of the umbilicus a very visible prominence, the elevation of which extended transversely four inches, and from above downwards three and a half. The skin above it was flaccid, loose, and easily pinched up. On applying the hand, a circumscribed tumour was felt, obscurely divided into two large lobes. It seemed to lie close to the abdominal wall, to which, however, it did not adhere. In all parts it was equally firm and inelastic, and nowhere yielded any sense of fluctuation. Gentle handling gave no pain, and by pressure the mass might be carried either upwards or downwards, to the extent of two inches, and across the abdomen, to the extent of at least six. It could not, however, be pressed backwards. In the erect posture it descended considerably lower than in the recumbent. On percussion over it a dull sound was elicited, and its size so defined was six inches by four. Between the upper boundary and the liver was a space in which a finger might be laid, and where the percussion note was moderately clear. In the infra-axillary region no interval could be made out between it and the liver. The iliac fossa and hypogastrium were extra resonant; and posteriorly, over the region of the right kidney, the extent of dulness was not increased, being, if anything, rather less than on the left side. It did not appear that the functions of any part of the abdominal viscera were in any way interfered with. The history given was, that the tumour had existed for seven years, had been almost painless, but during the last ten months had rapidly increased in size. Dr. Walshe inclined to the diagnosis of a fibrous growth in the great omentum. The patient remained under care for about a month only, and then returned to her home in the country, no alteration in the disease having been perceptible. See 'Med. Times' for Dec., p. 621.

Case 35.—*Fatty Tumour in the Omentum the Size of a Fist.*—A middle-aged man, under the care of MR. LLOYD, in St. Bartholomew's Hospital, December, 1852, died after the operation for inguinal hernia. He was moderately stout, but not very fat. At the operation intestine only had been found in the sac. At the *post-mortem* there was found at the free border of the great omentum, near its middle, a well-circumscribed mass of consolidated and firm fat, about the size of a fist. Its neck was somewhat constricted. The explanation which seemed most probable was, that at some former period this portion of the omentum had been incarcerated in the hernial sac, and that subsequent to its return it had been the seat of fatty deposition. It had not any appearance of having been at all recently strangulated; and

localisation of the fat, which in other parts of the omentum was not very abundant, seemed to show that the form of the tumour did not result merely from compression, but that there had been growth of the fatty tissue. The tumour had not been sought for during life, but had it been discovered, it is evident that it would have afforded a very puzzling example of a freely movable mass in the abdomen. The Hunterian Museum contains specimens of fatty tumours in the omentum of a cat; but we are not aware of any example on record, excepting the above, of the like condition in the human subject. We narrate the case here, not because it is to be supposed that any of the three preceding ones resembles it in nature, but on account of its important relation to them in respect to its being seated in the omentum, and being freely movable.

[The next case was that of a woman, aged 22, under Dr. BUDD, at King's College Hospital.]

Jan. 21. She is a married woman, and was confined of her first child in Sept. last. She recovered fairly, and for the first two months suckled her infant. About seven weeks afterwards she had an illness which lasted a week, and of which the most prominent symptom was a tendency to rigors and irritability of the stomach, with occasional vomiting. On becoming convalescent from this, she noticed accidentally that there was in the right side, which had never been the seat of pain, a large hard lump. The tumour, soon after its discovery, became the seat of uneasy sensations, and, as she thought for the first few weeks, somewhat increased in size. More recently, however, she has not noticed any enlargement of it, but for three weeks past there has been more of pain than there was previously. There is at present, however, no pain, and but very little tenderness in it. It is situate in the right hypochondriac and upper part of the right iliac region, and feels as if about the size of an infant's head flattened and moulded against the abdominal wall. It is firm and resisting, without the least feeling of fluctuation. Its boundaries are well circumscribed, and its borders feel much thinner than its middle. Percussion over it is quite dull, unless made by a sharp tap, when a clear note from intestine beneath may be elicited. The mass descends and rises with the respiratory movements, and may by manipulation be made to change its position considerably from side to side. Its borders on the right side, as well as in other directions, can be plainly felt. The woman has a clean tongue and a quiet pulse; her appetite is good, but she complains of thirst, and of some pain in the right lumbar region. Slight rigors, followed by sweating, occur occasionally. The urine is very acid, and contains, as shown by the microscope, oxalate of lime crystals, and a few pus cells. There is some irritability of the stomach, and vomiting sometimes occurs.

This patient remained under Dr. Budd's care from Jan. 21st to March 2nd, during which time no change in the character of the tumour took place, nor did it perceptibly increase in size. The urine was frequently examined microscopically, and, in a general way, contained no pus, nor was it in the least albuminous. The treatment was directed to the relief of the tendency to vomiting, and to perspiration, which having been achieved, the patient was allowed to return home.

Comments.—The last case which we have mentioned very closely resembles Cases 32 and 34, excepting in that a much shorter history was assigned to it, and that there had been and were some constitutional symptoms which might seem to indicate the existance of local inflammation. The characters of the mass, however, were so unlike those of an accumulation of inflammatory product, that it would probably be safer to refer those symptoms to the irritation of some adjacent organ than to the tumour itself. Not improbably, the right kidney was pressed upon,—a conjecture that the presence of pus in the urine, which was at one observation thought to be the case, would go to confirm. As to the date assigned to the tumour, also, it is very possible that the woman was much mistaken, more especially as it had, when first discovered, very nearly, if not quite, attained its ultimate dimensions. If, then, we may venture to regard this as one of similar nature with the three preceding cases, Nos. 32, 33, and 34, we have a series of four cases in which movable tumours existed, superficially placed in the abdominal cavity. In all, the patients were women below middle life, and in three under the age of 28. In three the tumour lay to the right side. In none were there any indications of malignant cachexia; and in three the account of the existence of the tumour for many years quite negatived any supposition of its cancerous nature. In but one was there anything in the history

of the case suggestive of an inflammatory origin. That the tumours were not of an hydatid nature seemed probable from their not possessing a globular shape, and also from the absence of any degree of fluctuation. In regard to the influence of sex, it is probable the occurrence in females of the whole of those we have been able to adduce from recent hospital experience, is quite accidental. Mr. Paget has mentioned to us a case as being the precise counterpart of the one under his care, excepting that the tumour was not quite so large, and the patient was a man. It came under his notice in private practice, and has been repeatedly seen during several years; the patient, meanwhile, retaining perfect health, and the tumour not altering either in size or other character. Its history extends over a long period of years, during which no growth has been noticed.—*Med. Times and Gazette, Nov. 4, 1854, p. 469.*

143.—ON ANTISTASIS OR COUNTERACTION.

By Walter Cooper Dendy, Esq., Surgeon to the Royal Infirmary for Children.

If we analyze the physiology of the living world we shall find it displaying a series of antagonisms: the entire process of carrying on vitality indeed may be termed the resistance of one thing to the influence or agency of another. In pathology it is essentially so. We daily see one disorder disappearing as another is developed or established.

In therapeutics, above all, whether their action be similia or contraria, many of the most potent remedial agents effect their influence by some impregnation of the blood with their own especial quality; thus poison and antidote, disorder and remedy, are often but the relative terms for two exciting elements—the one, it may be, perilous, destructive, permanent; the other, salutary and transient. It is probable indeed, that something abnormal, some systemic change, is induced somewhere by every remedy in imparting its counteracting influence. The term antistasis indicates in its comprehensive sense this power, which we recognise as antagonistic force, vicarious action, metastasis, specific or special remedy, prophylaxis, vis medicatrix; nor is it confined to physics, for mental influence or derivation will often prove remedial after therapeutic agency has failed. Spontaneous or natural antistasis, the controlling or preventive force of one disease over another, is seen in the prophylaxis of measles by small-pox, of phthisis by ague, &c.

During September last I was called to an infant, a year old, who is now convalescent of pertussis. When the cough was at its height, it was at once and entirely checked by a sudden attack of spasmodic cholera. On its recovery from this, the hooping-cough re-assumed its severity. Had this been a case of common inflammation, the flux might probably have cut it short at once; the *special* or *periodical* disorders are only *suspended* by it.

The influence of specifics is on the principle of catalysis, and will consist of one of the following modes; the stimulation of special or particular glands to excrete or dislodge a deleterious something from the blood, by instilling their own poison, so to write, into the system, and thus expelling one more noxious or perilous, as iodine *versus* struma, arsenic *v.* lepriasis, and mercury *v.* syphilis; for sooner or later, if unduly exhibited, we must have iodism, arsenicism, and mercurialism; 2nd, the producing, on the principle of neutralization, some *tertium quid* as quiescent and innocent, as the product of an acid and an alkali. Such may be the effect of soda in excess of gastric acid, and the elimination of sulphuretted hydrogen by the combination of sulphur with hyper-hydrogenized blood; or a special controlling influence, as that of aconite and digitalis, on the pulse; or the subduing of hyperæsthesia or exalted sensibility, as the power of opium and prussic acid in neuralgia, or the imparting of the tone by bark in ague.

All this fully illustrates the truth of Hunter's axiom, and the two important practical secrets are probably these—to know when the disease is neutralised or dislodged, the moment for the suspension of our ministration, or the remedy will itself become disease—the antidote will prove a bane; and to apportion our doses to the idiosyncrasy or systemic power of the patient, or we may salivate with a grain of calomel, or suspend or destroy the heart's action by almost homœopathic doses of laurel water. As an example of casual prophylaxis, I may glance at the influence of

the chlorides on strumous diatheses. The factory girl, although hereditarily predisposed, when submitted to the inhalation of chloric atmosphere, will constantly resist the development of her taint of struma.

The object of this paper is of course limited, having reference chiefly to the lessons taught by the *vis medicatrix naturæ*—the removal of obstacles to her remedial progress, and the imitation of her salutary processes.

We see, then, that the two grand modes of antistasis which nature adopts to preserve and restore the health of the system are, elimination, and vicarious action. The former mode more especially prophylactic; the latter, curative. And I adopt this word antistasis, in contrast to that of metastasis, which merely implies the transmigration of the same especial action or quality from one locality or tissue to another, often of *more* importance, as the transference of rheumatism from the joints to the pericardium, of the fibrous tissues of the eye; whereas antistasis implies the development or spontaneous establishment of another action on a remote and usually *less* important tissue in which that action was indigenous and hitherto latent; metastasis, moreover, being usually aggravating; antistasis, remedial.

Of course natural prophylactic elimination is constantly going on in the body; its integrity could not for a moment be evinced without it. Let it be checked or thwarted, and disorder in varied degrees instantly ensues. May not the loads of grape sugar in diabetes be a depuration and the prophylactic of disorder?

The pathology of the kidneys, ere the elaborate researches of Prout, Bright, and Bird, was often ignored, not only because its investigation was troublesome, but that the prominent symptoms were *elsewhere* apparent. Thus constantly arose internal metastatic action, vicarious deposits in the shut sacs of the body, ever perilous, often destructive, and the reflex contamination of the blood by the absorption of urea. This poisoning of the brain is noticed not only in absolute suppression of urine, but where, although in large quantity, it is of low specific gravity.

If the skin were in every point vicarious of the kidneys, it were well; but the transference is unhappily misplaced, the brain being often the scapegoat. In the comatose disorders of children we constantly see the value not only of urinary tests, but of ascertaining the condition of the renal functions.

I may venture to adduce two or three cases as indirect illustrations.

The daughter of a barrister, in Eccleston-square, had been indisposed for many days, the prominent symptoms being moaning, lopping or tossing of the head, frowning, coated tongue, and remittent febricula, so constantly marking these infantile maladies. The routine of grey powders, antimonials, and even powerful aperients, failed in giving relief. In was then ascertained that the urine had been extremely deficient in quantity, and diuretics, which speedily brought the kidneys into play, and the warm bath, speedily relieved the head symptoms, and the child was convalescent. In retention of urine also I believe the coma and delirium so soon supervening arise from this poisoning of brain-blood, whether the mode be absorption or vicarious deposit, in consequence of non-separation by the renal gland.

I was called to Mr. P. His symptoms were headache, hot, dry skin, furred tongue, wandering, and clipping of the words. *Prima facie*, it seemed that plethora was the sole cause, and I was about to adopt several modes of depletion. I discovered, however, that he had stricture, and, by cross-examination, that he had not passed urine for twenty-four hours. The passage of this was peremptory, but myself and my accomplished friend, Mr. H., failed in effecting this; and, as he resisted all our entreaties for an operation, he became more and more delirious, and rapidly sunk, a dissolution too rapid to be explained by the common course of typhoid fever. The bladder remained entire.

In July last, I was telegraphed to the further part of Surrey, to visit a gentleman, whose perineum I was compelled to open very deeply, in consequence of an enormous gangrenous abscess, which was rapidly dissecting the perineal spaces. The symptom most distressing to the friends being muttering, acute delirium. The anatomy of the perineum was so completely deranged that it was with the utmost difficulty that I introduced a catheter into the bladder, and withdrew the urine of twenty-four hours' retention. In this case, within an hour the cerebral symptoms subsided —an amendment so rapid as to be inexplicable, except by the draining off of that which had by metastasis poisoned the circulating fluid, and restoring the function of subsequent elimination. On this principle, probably, we may explain the *tonic* effect

of diuretics where the apathetic granular kidney has induced extreme asthenia and debility. How important, then, to assist the suppressed eliminations of nature! The skin is constantly vicarious of the kidney in profuse diaphoresis or sudamina—*i. e.*, as regards the mere *serum* or water; but it is not specially so, as I have observed, urea is not *excreted*. The renal apparatus seems essential for this elimination, although some medicines, essentially purgative, will, I believe, when the kidney fails, pass off through the skin with the profuse sweating. The derivative influence of the skin is also beautifully seen where extensive cutaneous vesications have, by a system of *compensation*, rapidly induced the absorption of hydrops pericardii. But it seems that no antistatic action can be induced to excrete urea; the only remedy is mechanical. Even the fluid of cholera—that specific fever so often fatal in the first stage—may be often antistatic or eliminatory, being but a profuse critical diaphoresis of the mucous tissue, as the sweating sickness was of the cutaneous pores: and this may have perhaps been the gist of Abernethy's system, the sweating of the membrane doing more than the stimulation of the liver by his pill. The salutary sequelæ of intestinal antistasis are daily witnessed. The happy result of critical or spontaneous diarrhœa was probably highly suggestive to Hippocrates and Sydenham, and the two modern oracles of alimentary elimination, Hamilton and Abernethy.

I believe that deficient defecation, also, is a constant source of this poisoning of the blood. In children who die of marasmus, we constantly see a pulpy mass, like decayed brown paper, quickly prone to septic changes, or acetous fermentation, closely coating the mucous membrane of the alimentary canal. Hence two morbid effects, negative and positive—a closing of the mouth of the lacteals, and an indirect contamination of the blood.

Habitual constipation, of young girls especially, lasting, as I have known it, for even twelve and fourteen days, I believe often proves a fertile source of their multiform and eccentric maladies, by absorption. This mode of endosmose is proved by the feculent odour imparted to pus in the vicinity of the lower bowel. No plan is more prejudicial than to treat this habitual constipation by habitual and drastic purgation. This mode is certain to increase the malady, which depends on an atonic state of the canal. Very minute doses of aloes, with soap, combined with enemata of warm soap-water, are far preferable; and this should be quickly followed by decided tonics, which will prove antistatic to this debility, and thus ensure healthy action. These are not the only cases maltreated on the routine of purgation, because we are fearful of the censure of blowing hot and cold. I see no incompatibility in the simultaneous exhibition of bark and foxglove. In habitually depraved secretions we again adopt the same error. It is not enough to dislodge the bad; we should ensure an improvement, both by the antistatic process of cutaneous activity and the healthy stimulation of the alimentary glands, by alterative tonics.

The more prominent mode in which the *vis medicatrix* is displayed is local or *external* antistasis—the establishment or development of morbid action in a remote organ, to relieve one primarily affected. This, perhaps, induced Sydenham to affirm "disease to be no more than a vigorous effort of nature to throw off the morbific matter, and so recover the patient." Boerhaave even wished to produce petechial fever as a counteraction to internal disorder. This may, of course, be perilous and fatal when a more important organ is secondarily attacked, as, for instance, the occasional transference of ulceration from an external burn to the mucous membrane of the duodenum. But nature is usually wiser than this, throwing out on the periphery of the body that which might destroy in the centre.

What a volume of cases might be cited of the salutary result of this spontaneous antistasis, and of its converse spontaneous recession, or what I have been often jeered at for terming unfortunate cures! We constantly boast of a victory that ought in strict justice to ruin us. The instances I may refer to are, milk fever and insanity, from lacteal suppression; meningitis, from suppression of favus; hæmoptysis and phthisis, from the stoppage of hemorrhages and fluxes; the development of tubercle, from amenorrhœa, &c.; and, indeed, the termination of pregnancy in parturition, the vicarious uterine action counteracting or suspending for a time the phthisical malady. How often, also, has accident indicated the mode we ought intentionally to have adopted. We have often known epilepsy, chorea, and other convulsive or spasmodic maladies relieved, nay, removed entirely, by an extensive burn or suppurating wound.

In the pathology of mania, especially, most emphatic lessons are written for us by accident, if we would but learn them. The history of the ravages which overworking commits in the highly intellectual brain teems with illustrations. I need only point to the case of the statesman and the philosopher, who, in what is termed temporary derangement, cut their throats. In the one, the physician who came only to see his lordship die, found that he had stuffed the wound with a towel in a moment of reason—the lucid interval consequent on the flowing of the brain-blood. In the other, the surgeon who stanched the wound was reprimanded by the patient for his neglect of that depletion which he had urgently begged him to employ, and which he was certain would have entirely divested him of all attempt at suicide. The pathologist who reads the autobiography of Walford, in the July number of the Psychological Journal, will discover at once the source of the most distressing and awful train of monomaniacal illusions that ever excruciated an enlightened intellect. These are the progressive sequelæ of a *cranial contusion* in infancy; acute headache; coryza maligna; continued cerebral erethism; melancholy madness most protracted. And yet even Mr. Cline blinked the case by merely recommending fumigation; and Dr. Withering, yet more apathetic, adopted the *expectant* treatment. And what were the results of this sad trifling? Extensive dura matral adhesions, the membrane of *leathery* consistence; honeycomb of the inner table of the skull; cerebral *ramollissement* and *green* abscess, and extensive serous effusion. Which of us will not believe but that decided counteraction in this, as well as the other cases, might at once have relieved the symptoms, and snapped the chain of morbid changes that ended in so melancholy a catastrophe? Analogous cases might be cited: this one is sufficient for my purpose.

How beautifully does nature set up an antistatic action in cases of amenorrhœa! The sanguineous discharge from the ulcer, therefore termed the *menstrual sore*, is close on a compensation.

When a morbid idiosyncrasy or diathesis exists, we may convert it into a most valuable mode of counteraction, both curative and prophylactic. Often in a fit, or a threatening of apoplexy, in a gouty diathesis, we should not hesitate, if we read nature's lessons wisely, to blister the foot with liquor vesicatorius, in order to solicit or induce an attack of gout.

In cases, also, where a wound assumes an unhealthy aspect, we should beat about to discover some habitual or constitutional malady, by the bringing out of which we may, by antistasis, remotely influence the wound. A few days after Sir Astley Cooper had dissected the cyst from the head of George IV., the lips of the wound gaped, and an ichorous discharge ensued. The accomplished surgeon, I believe, trembled for his patient, and even for the continued splendour of his own fame, when happily a desperate fit of gout supervened, and the king was saved. If Long, the Sangrado of endermic surgery, had known when to stop, or how to heal, I believe he might have had his tether to this day, unless we ourselves were for once wise enough to take a lesson in prophylactic surgery from a quack.

Those who have watched the cerebral disorders of children will remember how constantly symptoms have been aggravated by arrest of spontaneous antistasis, and how immediate is the influence of surface counteraction in cases marked with imminent peril, where time is not allowed for the effect of internal remedy. Indeed, if there be a class of patients especially obnoxious to physic, and yet are constantly overdrenched, it is the batch of babies, for whom the endermic method of treating them is at our elbow. The infant of a well-known architect had taken a quarter of a grain of Dover's powder, which it is probable the chemist had carelessly triturated, when coma very speedily supervened. The pupil was contracted to a point, and the child seemed moribund. There was no time for drenching. The application of snow to the vertex caused the pupil instantly to expand, its contraction recurring as the snow was melting. Perseverance in this simple mode, however, ultimately relieved the brain of its congestion, and the infant recovered. I do not here enter on the jealousies between nerve and brain blood; I merely state the fact and the effect. If we do not all fully coincide in the aphorism of Hippocrates—"They in whom tumours and ulcers do appear are neither taken with convulsions nor driven into rage or madness,"—we must believe in the extreme value of superficial counteraction as a prophylaxis—a safety-valve in cases of tendency to, or even in, incipient disorders. Indeed, the latent or dormant germ about to be developed may be thus checked and blighted, or kept in

abeyance, until by successive generations the systemic taint or tendency may at length cease to be hereditary. An issue has often checked hæmoptysis and tubercular growth, which might, in its default, have speedily lighted up consumption.

For the sake of brevity, I have not here discussed the comparative influence and value of *especial* counter-agents, and, indeed, I have merely jumbled together a tithe of what I might wish to record.—*Lancet, Dec.* 23, 1854, *p.* 526.

144.—ON THE MODES OF DEATH BY HANGING.

By Dr. J. S. Hughes, Examiner in Surgery to the Queen's University in Ireland.

The modes of death by hanging may be thus summed up—not in the order of their frequency, but of their fatality:—First, Luxation of the odontoid process of the second cervical vertebra, or fracture of the spine, and consequent injury to the medulla spinalis; Second—Suffocation from pressure on the windpipe, producing complete obstruction to respiration; Third—Congestion of the brain, or apoplexy from arrest of the return of blood from the head to the heart, the result of compression of the great bloodvessels of the neck; Fourth—Injury inflicted by violent and prolonged pressure on the important nerves of the neck.

The first cause of death by hanging, or that by luxation or fracture of the cervical vertebræ, is rarely met with, even in those who have been criminally executed; thus, in four cases of this description, the late Dr. Houston found the "cervical vertebræ uninjured, and also the spinal marrow and the brain;" and in the case of Hurley, who was hanged in August, 1853, for the murder of a young girl at Dunsandle wood, although the body had fallen from the drop, "with a tremendous jerk, and oscillated for a few minutes," yet we are informed by Dr. Croker King, who made a minute examination of the culprit's body eighteen hours after the execution, that "there was no dislocation or fracture of the vertebral column, or injury of the ligaments or spinal cord." Sir Benjamin Brodie states, that he has examined the bodies of several persons who had been hanged, and never found the spine to have been injured in a single instance.

It is now generally admitted that the most frequent immediate cause of death by hanging is that by suffocation, or exclusion of air from the lungs, the result of pressure of the rope or ligature on the windpipe, but in many cases death would appear to be produced by the compound influences of suffocation and apoplexy; indeed, according to Remer and Caspar, asphyxia and apoplexy operate in the greater number of cases of hanging as mixed causes of death. The rapidity of death by suffocation will depend on a variety of circumstances, which may be thus enumerated, viz.: the exact position of the rope or ligature round the neck, the weight of the body, the height of the fall, the age of the patient, and the quantity of air in the lungs at the moment the ligature was applied to the neck. The following are the physiological results which ensue soon after the respiration has been completely stopped. The heart, after a few beats, propels impure blood, that is, blood no longer oxygenated, to the brain, and, as the action of the heart and diaphragm depend on nervous energy, and as nervous energy requires a supply of pure or oxygenated blood, it follows that when the supply of pure arterial blood is cut off, the heart and diaphragm soon cease to act.

The heart continues to act for a brief period after respiration has completely ceased. When speaking of the rapidity of death in these cases, Dr. Taylor says:—"Supposing the hanging to be unattended with violence to the parts about the neck, it is possible that some individuals might be resuscitated after five minutes' suspension, or longer; others again may not be recovered when cut down immediately after suspension—a fact which depends probably on the different degrees to which asphyxia or apoplexy has extended." Sir B. Brodie is of opinion that, under ordinary circumstances, it rarely happens that the heart continues to act for more than four minutes after the trachea has been completely obstructed; and he thinks, in weak and exhausted animals, the period to be shorter than in the strong and vigorous.

The great majority of physiologists are of opinion that if once the heart's action has positively ceased, in consequence of the suspension of respiration, it cannot be renewed. Dr. Carpenter, however, says: "The cessation of the heart's action is

due to two distinct causes, acting on the two sides; for on the right side it is the result of over-distension of the walls of the ventricle, owing to the accumulation of venous blood, and on the left to deficiency of the stimulus necessary to excite the movement. The property of contractility is not finally lost nearly as soon as the movements cease—for the action of the right ventricle may be renewed for some time after it has ceased, by withdrawing a portion of its contents, either through the pulmonary artery, their natural channel, or more directly by an opening made in its own parietes, in the auricle, or in the jugular vein; or, on the other hand, the left ventricle may be again set in action by renewing its appropriate stimulus of arterial blood; hence, if the stoppage of the circulation has not been of too long a continuance, it may be renewed by artificial respiration, for the replacement of carbonic acid by oxygen in the air cells of the lungs restores the circulation through the capillaries, and thus, at the same time, relieves the distension of the right ventricle, and conveys to the left the due stimulus to its action."

Although in the third mode of death by hanging, or that by apoplexy, the sudden compression of the great bloodvessels of the neck cannot completely arrest the circulation of blood, inasmuch as it cannot reach the vertebral vessels; yet, if the pressure be sufficiently long and forcibly sustained, it can, by delaying the return of blood from the head to the heart, produce extreme congestion of the cerebral vascular system, which, as proved by post-mortem investigations, may or may not be accompanied by extravasation. As, however, in a considerable number of the cases of attempted suicide by hanging, which have recovered, no paralysis ensued, we have some reason to conclude that in many cases the apoplexy which results from compression of the great bloodvessels of the neck is of the congestive form, a point of considerable practical importance, inasmuch as it is in that particular form of apoplexy that the voice of the profession is in favour of bloodletting.

Compression of the important nerves of the neck must necessarily be followed by injurious results, although it may not act as a proximate cause of death in these cases. Thus, J. J. suffered considerable distress from laryngeal cough for several days after the attempt at suicide; he likewise complained of a loss of sensation in his jaws and ears, which was obviously ascribable to the forcible compression to which the nerves of the neck were subjected whilst the patient was in the state of suspension.

Much of the success of the treatment of a case of attempted suicide by hanging will depend on the nature of the injuries inflicted, the period at which assistance is procured, and a proper discrimination in the selection of remedial measures. Thus, if the surgeon be summoned to a person within a few minutes after he has suspended himself, although the breathing may have completely stopped, yet, provided there is neither dislocation nor fracture of the vertebræ, and consequent injury of the medulla spinalis, animation may, in many cases, be restored by an immediate recourse to artificial respiration. Electricity or electro-galvanism, if within reach, ought at the same time to be applied to the cardiac and spinal regions; if, on the other hand, the patient is cut down before respiration has ceased, and symptoms of congestion of the brain or apoplexy should present themselves, bloodletting ought at once to be resorted to. Although, as already stated, there is reason to believe that the apoplexy which results from compression of the great bloodvessels of the neck is frequently of the congestive form, yet, as we possess no infallible rule by which we can positively pronounce in these cases, whether cerebral hemorrhage has taken place or not, we should in every such case exercise as much caution in the abstraction of blood as we would in apoplexy from other causes; here, as elsewhere, we should, whilst endeavouring to remove congestion, or arrest hemorrhage, ever bear in mind that the brain has suffered from a violent shock, for the repair of which the system will require its best resources.

Much difference of opinion has existed both as to the quantity of blood to be taken away in cases of apoplexy, in which bleeding is indicated, and as to the precise situation from which the blood should be abstracted. The condition of the pulse, and the effects of the withdrawal of blood on it, will be the safest guides as to the former; with regard to the latter, we generally select the temporal arteries, agreeing, as we do, with Abercrombie, who, whilst treating on the subject, says:—"Much importance has been attached to bleeding from the jugular vein, as more likely to give immediate relief to the head; but we must remember, that the only jugular vein that can be opened is the external jugular, which has little communication with the brain,

and, consequently, bleeding from it is much inferior to bleeding from the temporal artery."—*Dub. Hosp. Gazette, Oct.* 1, 1854, *p.* 258.

145.—ON THE LAWS OF MORTALITY AND SICKNESS OF THE LABOURING CLASSES.

By Thomas Rowe Edmonds, Esq., Actuary to the Legal and General Life Assurance Society.

It has already been stated that sickness, in duration for each year of life, increases with the age, according to the same law which regulates the increasing proportion of deaths to the number living at successive ages. From the unity of law between sickness and death in relation to life, it necessarily follows that the sickness endured at any age bears a constant proportion to the deaths at any specified age. This proportion is commonly found to be, that of two years of sickness to one death. Occasionally this proportion is as low as one year and three quarters, and as high as two years and a half, of sickness to each death. In addition to the law determining the duration of sickness at each age, there is another law which connects together the number of attacks or cases of sickness with the number of deaths, and consequently with the number of living, and with the duration of sickness at each year of age. The law which regulates the attacks of sickness was first communicated to the public in 'The Lancet' of the year 1836, (vol. i., p. 855,) in an article wherein I showed, from recent observations, that the fatality of cases or attacks of sickness increased with the age according to the law which regulates the increase of mortality and the increase of duration of sickness for each year of life. I then stated it to be an inevitable consequence of the identity of the numbers regulating the proportion of attacks of sickness to deaths, of duration of sickness to life, and of deaths to life at each age, that the number of annual attacks of sickness for a given number living was the same at all ages, and that the duration of each attack of sickness increased with the age in the same degree as the mortality increased.

The most interesting and remarkable of the above laws of sickness, is that which makes the proportion of the annual attacks of sickness to the number living constant at all ages. Notwithstanding its extreme simplicity, the existence of such a law does not appear to have been suspected previous to its announcement; nor did there exist, at that time, any facts which could be appealed to as supplying direct evidence of its existence. The public were subsequently indebted to Dr. Farr for adducing facts directly proving the constancy of the proportion of the numbers attacked by sickness to the number living at various ages of adult life. In the returns from Friendly Societies published by Mr. Neison and Mr. Finlaison, a great multitude of facts have been collected in further corroboration of the proportion of attacks of sickness being constant, and independent of age—at least in the period of florescence. According to Mr. Neison, the constant proportion of living persons attacked by sickness in any year between 20 and 50 years of age, is 22 per cent. According to Mr. Finlaison, the constant proportion is 24 per cent. in each year. In both observations there is a depression of about 2 per cent. in the attacks near the age of 33 years, which is the age at which the greatest number of admissions of new members occurs; and which, consequently, is the age at which "selection" might be expected to have the greatest disturbing effect, whether on sickness or on mortality.

In the period of senescence, or at ages exceeding 50 years, the facts bearing on the laws of sickness (whether of attacks or of duration) are everywhere imperfectly recorded, on account of the difficulty of distinguishing cases of sickness from cases of debility from old age. Judging from all observations hitherto made, there appears to be no ground for doubting that the law of attacks, like the law of duration, of sickness, is continuous throughout the two periods of "florescence" and "senescence," or from the earliest to the most advanced age of adult life. The returns of Mr. Neison are not available for the purpose of determining whether the proportion of attacks of sickness is constant at advanced ages, as well as at ages under 50 years, because no attempt has been made to distinguish sickness from old age. But the returns of Mr. Finlaison, which professedly represent pure sickness, free from cases of debility from old age, are sufficient for the present purpose. There is, however, a preliminary correction to be made in the numbers which Mr. Finlaison takes to

TABLE

SHOWING, *for Decennial Intervals of Age, the Rates of* SICKNESS *in Rural, Town, and City Districts, according to each of three Observers; also showing the Mean Rates of Sickness and Mortality in each of the same three Districts, for comparison with the results of Theoretical Tables, in all of which the period of "Senescence" commences at the age of* 48 *years.*

AGE.	RURAL DISTRICTS.			TOWN DISTRICTS.			CITY DISTRICTS.			MEAN OF 3 OBSERVATIONS.		
	Finlaison.	Neison.	Ratcliffe.	Finlaison.	Neison.	Ratcliffe.	Finlaison.	Neison.	Ratcliffe.	Rural.	Town.	City.
	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.
25 to 35	1·02	·87	·78	·96	·89	·83	·90	1·11	·84	·89	·89	·95
35 " 45	1·19	1·08	·99	1·18	1·30	1·08	1·18	1·50	1.28	1.09	1·19	1·32
45 " 55	1·61	1·65	1·76	1·79	2·62	1·80	1·68	2·46	2·26	1·67	2·07	2·13
55 " 65	2·78	4·08	4·54	3·00	5·17	4·10	2·63	4·63	5·58	3·80	4·09	4·28

TABLE.—(*Continued.*)

AGE.	*Sickness* Observed. Mean of Three Observations of all Three Districts.	*Sickness*, Theoretical, Corresponding to Mean of Three Observations.			*Mortality* Observed, being Mean of the Three Observations.				*Mortality*, Theoretical, Corresponding to Mean of Three Observations.			
		Rural.	Town.	City.	Rural.	Town.	City.	Three Districts.	Rural.	Town.	City.	Three Districts.
	Weeks.	Weeks.	Weeks.	Weeks.								
25 to 35	·90	·84	·92	1·02	·73	·77	·99	·80	·64	·77	1·02	·77
35 " 45	1·17	1·13	1·24	1·37	·86	1·04	1·45	1·02	·86	1·03	1·37	1·03
45 " 55	1·87	1·73	1·88	2·09	1·35	1·68	2·18	1·61	1·31	1·57	2·09	1·57
55 " 65	3·95	3·65	3 98	4·43	2·72	3·45	3·12	3·01	2·77	3·32	4·43	3·32

represent attacks of sickness, which correction is of small amount at ages below 50 years, but is of great magnitude at very advanced ages. Mr. Finlaison assumes the number of persons who have been on the sick-list in the course of the year to represent the number of new attacks of sickness in the year. This assumption is obviously incorrect, for in such list is included the cases of sickness remaining at the end of the previous year, and transferred to the new year. The number of such transferred old cases of sickness is represented by the proportion of living constantly sick at the particular year of age, which is the correction to be applied in diminution of Mr. Finlaison's alleged numbers of attacks. For example, according to Mr. Finlaison's tables, and taking decennial intervals of age, the proportions constantly sick at the ages of 50 and 80 years, respectively, are 3¼ and 26 per cent. Also, according to the same tables, the annual proportion of persons on the sick-list is 26¼ per cent. at the age of 50 years, and 56 per cent. at the age of 80 years. If we subtract from the latter numbers the corrections previously given, we obtain the numbers 23 and 30 per cent. as the corrected per-centage of new annual attacks at the respective ages of 50 and 80 years. These two numbers approach sufficiently near to each other to warrant the presumption that a defect in the observation is the cause of the failure to obtain an exact coincidence. The apparent excess of attacks at the age of 80 years is in all probability owing to the complication of debility from old age with cases of sickness. Seven out of 30 cases of sickness would not have had the visible effect of producing incapacity for labour, if the man of 80 had not been subject to a debility from which the man aged 50 was comparatively free.

On inspection of the Table, it will be seen that at the mean age of 40 years, the rate of sickness obtained by combining all three observations, is 1·17 weeks to one year (or 52 weeks) of life, which is the same thing as 2·24 years of sickness to 100 years of life, or 2·24 per cent. of the living constantly sick. From the same table it will be seen that the mean number of deaths, at the same age, according to the same combined observations, was 1·02 in one year out of every 100 people living. There was consequently 2·24 years of sickness to 1·02 deaths, or 2·20 years of sickness to one death. This relation of sickness to death remains constant when other ages are compared in the assumed observation representing the mean of all three observations.

The relation of sickness to death, which is constant for different ages in the same observation, is subject to variation in different observations. For example, in rural districts, the proportion of sickness to one death, at every age, is found to be 2·52 years, when the average of all three observers is taken; whilst in town and city districts, the averages similarly obtained are 2·29 and 1·92 years of sickness to one death. This result, from combining the three observations on each of the three districts, is not, however, in accord with the separate observations, for Mr. Finlaison states the disproportion between rural and city sickness to one death to be much greater than the disproportion stated by the other two observers.

The present three observations, when closely investigated, do not, however, either of them make good the proposition, that the quantity of true sickness to one death is less in cities than it is in rural districts. Mr. Finlaison, in showing the effect of "heavy" labour in increasing the apparent sickness, has supplied a cause sufficient to account for the apparent excess of rural sickness over town and city sickness. The apparent sickness, according to Mr. Finlaison, depends in a great measure on the occupation or labour of members being "heavy" or "light." He has shown that, at any given age, whether in rural or city districts, whether the mortality is high or low, the apparent sickness is 20 per cent. greater for heavy than it is for light labour. In the two cases, the real amount of sickness is probably the same, but the incapacity for labour, or apparent sickness, is 30 per cent. greater in the case of heavy labour. This excess probably arises from minor degrees of sickness, which are sufficient to incapacitate for heavy, but not for light, labour. The proportion of "heavy" is much greater in rural than in city districts. We have thus a reason for expecting more apparent sickness in the former than in the latter districts. The cause now mentioned, when taken in combination with the opposite methods of treating superannuation, is also sufficient to account for the disproportion of rural to city sickness being greater, according to Mr. Finlaison, than it is according to the other two observers; for a considerable proportion (one-fourth part) of the sickness exhibited in the tables of Mr. Neison and Mr. Ratcliffe, consists of the life-time of superannuated members,

who do not engage in labour, whether heavy or light. On the other hand, Mr. Finlaison, in his tables, takes no account of superannuation, except to exclude it from any connexion with sickness. It thus happens that all Mr. Finlaison's recorded sickness is affected by the distinction of heavy and light labour, whilst three-fourths only of the sickness recorded by Mr. Neison and Mr. Ratcliffe is so affected.

The laws of mortality, of sickness, and of health, may be said to be the three chief regulators of human life. It has been already shown, that there exists such a parallelism between the two laws of mortality and sickness (in duration,) that when the mortality at any age is known, the sickness at the same age is also known, by means of the existing constant relation of sickness to death at all ages, (about two years of sickness to one death.) It remains to be seen that the degrees of health at different ages also bear a simple and constant relation to the mortality. It is first to be noticed that health-time, being that portion of a man's life-time which is not sick-time, will of course decrease with the age as sick-time increases; so that, at very advanced ages, sick-time and health-time will form nearly equal portions of a man's life-time. At the early ages, however, of adult life, the proportions of health-time to life-time decrease so slowly, that, for the purpose of estimating the amount of health enjoyed at any age between 20 and 50 years, the element of time may with propriety be neglected, and the degree or intensity of health be alone regarded. On reflection it will be easily perceived that the degree of health possessed at any age is identical or synonymous with the vital force possessed at that age. The mortality is the inverse measure of the vital force, and is consequently the inverse measure also of the intensity of health.

For the purpose of illustrating the combined effects of the laws of mortality, sickness, and health, let any two ages in the period of "florescence," differing from each other by the interval of 24 years, be taken, this being the interval in which mortality doubles itself. For example: if the mortality, sickness, or health, be numerically expressed for the age 20 years, we have only to multiply such mortality or such sickness by 2, in order to obtain the numbers expressing the mortality and sickness respectively as existing at the age 44 years. Similarly, the intensity of health at the age 44 years, is obtained by dividing by 2 the number expressing intensity of health enjoyed at the age of 20 years. If, instead of comparing people of different ages in the same class of population, we compare people of the same age in different classes of population, whereof the mortalities differ in the proportion of 1 to 2, we obtain the same results as those just stated: if the mortality is increased 100 per cent., the sickness will be simultaneously increased 100 per cent., and the intensity of health reduced 50 per cent. Sickness and death are unquestionable evils, which increase as the force of life or health decreases. Additions to intensity of life or health are unquestionable benefits, because they have the effect of diminishing unquestionable evils. Independently of these unquestionably beneficial effects, there is ground for assuming that life or health is intrinsically a benefit: whence it would follow, that additions to intensity of life, or robustness of health, must also be benefits. If the intensity of life or health be doubled, there is nothing to oppose the conclusion that the intrinsic benefit, pleasure, or happiness is also doubled.

All three observers have devoted much of their attention to investigations of the mortality of members of Friendly Societies, classed according to occupation, with a view to discover the effect of any particular branch of labour or occupation in prolonging or shortening the duration of life. So far as regards the great majority of branches of labour examined, the comparative results elicited by the three observers are unsatisfactory, since they fail to exhibit any principle of classification applicable to trades in general. In one important respect, however, the three observers are nearly agreed, viz., as to the two classes which occupy the extremes of the scale of vitality. The highest rank is occupied by agricultural labourers; the lowest rank is occupied by those in the occupation of clerks. The highest degree of vitality belongs to the class whose corporeal powers are most exercised; the lowest degree of vitality belongs to the class whose corporeal powers are least exercised. According to the observations of both Mr. Neison and Mr. Ratcliffe, the vitality of clerks is lower than that of painters, potters, and miners; that is to say, an occupation involving no corporeal labour is apparently more detrimental to life than occupations involving corporeal labour in a poisoned atmosphere.

According to the observation of Mr. Finlaison, "heavy" labour in rural districts is

attended by increased vitality, whilst in town and city districts heavy labour has no similar advantage over light labour. This opposition of results is open to question on the ground of imperfect classification of the trades taken as the constituents of heavy labour and light labour respectively. A large proportion of the trades carried on in towns and cities have been classed by Mr. Finlaison as heavy labour, although confined to the hands and arms, and involving little exercise of the chief or lower limbs of the body. Such labour cannot, with reason, be considered as the counterpart of the unmistakably "heavy" labour of the agriculturist. It is highly probable that, if the distinction between heavy labour and light labour were as well marked in towns and cities as it is in rural districts, a similar advantage would be shown to exist in all three cases in favour of the vitality of the classes whose labour is "heavy."

The amount of bodily labour undergone, or muscular force exerted, by any particular class of labourers, appears to afford a correct index of the vitality of that class. This principle, which has been shown to be applicable to the opposite extremes (agriculturists and clerks) in the scale of vitality, would probably be found applicable to the intermediate classes, if they could be arranged in well-marked grades of labour undergone. This principle, when applied to the aggregate of trades collected in rural, town, and city districts respectively, will serve to explain why the vitality of rural is greater than that of town populations, and that of town greater than the vitality of city populations. For it cannot be doubted that the average amount of labour undergone by each man is greater in rural than in town, and in town greater than in city, districts. The disparity in the vitalities of the rural, town, and city populations would be greater than that observed if the native residents alone of rural, town, and city districts had been compared; for there is a great interchange of population by migration between rural and town districts, and between rural and city districts, whilst at the same time the balance of gain by migration is always against rural districts, and in favour of town and city districts. This transfusion of the rural element unduly elevates the vitality belonging to the native populations of towns and cities, at the expense of the vitality belonging to the natives of rural districts. If such recruiting from rural districts were stopped, the population of a large city, in however healthy a locality, would, in no long period of time, become unable to maintain its members by propagation.

When classes of people engaged in different occupations are compared, there appears to be no reason for doubting that the vitality of each class is proportional to the amount of labour performed by that class. It is only when single individuals of the same or different classes are compared that the principle is found to be inapplicable. The vitality of an average agricultural labourer is greatly superior to that of an average clerk; but there are many clerks who possess a greater degree of vitality than that of an average agricultural labourer. An agricultural labourer commonly exercises his power of labour to its full extent, whilst many a clerk, possessing a great power of labour, does not exercise that power. In the former case, labour undergone and capacity for labour are almost exact measures of one another; but in the latter case the labour of an individual is no index of his capacity for labour. Amongst individuals engaged in the same occupation, it may be said, with great appearance of truth, that the vital force of each is proportional to his capacity for labour. Having arrived at this principle, governing the vitality of the members of any one trade, there appears to be no obstacle to the extension of the principle to individuals of different trades when compared together, and to say generally, that the vitality of any individual is proportional to his capacity for labour. In the majority of occupations (excluding that of agricultural labourers) the capacity for labour is generally in excess of the labour actually performed; but the proportional excess is probably nearly the same in most trades, whilst the excess itself has a continual tendency to diminution. This tendency is a consequence of the general law, that capacity for labour increases with the full exercise of that capacity, but diminishes when that capacity is insufficiently exercised.

The vital force of any human being is closely and almost inseparably connected with the capacity for muscular exertion. The greater the muscular force, the greater the vital force; a conclusion that might be arrived at without the aid of statistical observations of the number living and dying. Nearly all the known elements of animal vitality are included or involved in the capacity for hard labour. The animal

organization is tasked to its full extent by hard labour; if any of the principal organs be unsound, hard labour will prove the defect. The capacity for long continued hard labour cannot exist without powerful digestive and respiratory organs, a great strength of muscular fibre, and nearly all other known elements of strong vitality.

The vital force of mankind (and probably of animals in general) is subject to a composite law, consisting of three parts, one of which is immutable and the other two variable, and capable of being influenced by human agency. The constant annual rate of increase of the vital force throughout the period of "childhood," the constant annual rate of decay of the vital force during the period of "florescence" or reproduction, and the greater constant annual rate of decay of the vital force during the period of "senescence," are in all probability immutable quantities which, like the law of gravitation, form part of the foundations of the universe. The second part of the composite law consists in the extent of the two periods of childhood and florescence respectively. In different masses of population, and in the same mass at different times, the extent of the period of childhood has been known to vary so much as three years, and the extent of the period of florescence so much as ten years. The cause of these variations is yet unknown. If the cause, when discovered, should prove to be under the control of human agency, the extent of these periods may be permanently prolonged, of which the effect would be more than a proportional prolongation of human life. The third and last part of the composite law of vital force consists in the absolute amount of that force possessed at birth, or at any specified age, by the average individual of a class. This amount in England has been shown to vary so greatly, that there exist classes of population whose mortalities at the same age differ from each other in the high proportion of one to three. These great variations have been shown to have a close connexion with degrees of bodily labour, and are apparently, to a great extent, under the control of human agency. Without any interference of man, the laws of nature operate to promote the increase of the classes possessing the higher degree of vitality, and to retard the increase of the classes possessing the lower degrees of vitality. It is, however, within the power of man greatly to aid the operations of nature by offering additional rewards to the higher degrees of labour, and affording additional facilities for the increase of that part of the population which is now ascertained to possess the highest degree of vitality.—*Lancet, Dec.* 2, 1854, *p.* 453.

146.—ON SOME USES OF CHARCOAL IN SURGERY.

By Lawrence Ormerod, Esq., resident Medical Officer to St. Mary's Hospital.

[Great interest has lately been excited by the effects of charcoal as a deodorising and disinfecting agent. Amongst other good effects we shall probably find that it will prevent an attack of epidemic gangrene, and even hasten the cure of one so attacked. Mr. Ormerod says,]

I took the body of an English terrier (weight about 10 lbs.), placed it on a stone floor in a small apartment, and lightly covered it with charcoal. Although the weather was very warm, and the body in a state of the most utter putrefaction, not the slightest odour could be detected. By some accident the charcoal was disturbed, and a large portion of the mass was left uncovered. In spite of this, the circumjacent charcoal was sufficient to prevent any offensive stench. Upon seeing this, I left the body completely uncovered, merely surrounding it with the deodorising agent. This, again, prevented any disagreeable smell. Having determined this fact, I again covered the carcase. Dr. Stenhouse, in his paper above mentioned, remarks that charcoal was not, as generally believed, an antiseptic, but, on the contrary, acted most powerfully as a hastener of decomposition. This statement was likewise fully corroborated by my experiment, as in less than a fornight not a particle of flesh remained upon the bones, which were picked perfectly clean, and were of a snowy whiteness. I would in parenthesis state, that the plan I have mentioned would admirably suit the purpose of those who are in the habit of preparing skeletons for the use of scientific museums. The weight of the charcoal used in this experiment was about 5 lbs.

The fact, that the mere propinquity of charcoal to decaying animal matter is sufficient to counteract the horrible tendency that exists in such matter, is one of great importance; and the application of it to practical purposes must be of much utility. Why should it not now be used in the Hospitals for our brave army in the East? In spite of all the care that may be taken to render a military institution of this kind cleanly and sweet, still the difficulty of doing so is great in the time of war. A battle, or even a skirmish, sends in suddenly a troop of wounded. All immediately is hurry and confusion; all hands are called upon to relieve the necessities of the sick; the wards become filthy, because necessarily neglected; a stench arises, not only from the bodies of the crowded soldiers, but from decaying animal matter, the proceeds of wounds and ulcers; then that terror of the surgeon, hospital gangrene, arises, laughing to scorn all the best efforts of science and humanity to check its course. What, then, more beneficial could be afforded than a substance that possesses the marvellous property, not only of destroying an odour most distressing to the patient, nurse, and doctor, but also of destroying the malignant influence of the organic matter from which such a noisome smell arises, reducing putrefying matter to innoxious compounds, so that, with a rapidity most remarkable, it can even take up dead flesh from bones without either visible or palpable action? I understand that extensive use is now being made at St. Bartholomew's Hospital of charcoal in the dissecting rooms. It is intended to use bolsters and mattresses filled with it in cases of infectious disorders. If, in that great institution, proverbial for its cleanliness, for its salubrity, for its freedom from taint, and absence from all contagious disorder resulting from filth and malarious atmosphere, such measures are thought necessary to be adopted for the benefit of its inmates, surely some like means might be taken in the military hospitals, unhappily, although perhaps unavoidably, the seat of contagion, dirt, and inconvenience. Dr. Stenhouse having, however, fully pointed out, in his letter to the 'Times,' the advantages to be derived from this use of charcoal, I will leave that branch of the subject, and proceed at once to the question as to its value in a remedial point of view; merely observing, that, as reported in the papers, hospital gangrene has already broken out with virulence in Scutari, and instant measures should be adopted to use all plans which science and practical experiment have shown to be efficacious.

In the treatment of gangrene, the surgeon has three objects in view; the first is constitutional support; the second is the quick removal of the slough; the third is the destruction of that malarious influence which causes the disease to spread from one wound in a ward to all wounds that may be in it. The first of these three is foreign to the present object of this paper. To the two last, I now beg to direct attention.

1. The slough is to be removed by means partly local and partly systemic; and it is of the utmost importance that it should be removed as soon as possible. Not only does its presence act as a drain upon the body, weakening the patient by directing all his vital energy to the casting off of that which has become a foreign substance, but, every moment that it remains, there is a chance of the absorption of the putrid matter in the system,—the almost sure precursor of death. The formation of the line of demarcation depends, of course, upon vital action, and local applications are of very little, if any use, to hasten its appearance. The constitution must be supported well by stimulants and nutrients, in order to be able to perform the extra duty imposed upon it. But every means should be taken to remove rapidly the portion already dead, and cumbering the subjacent living structure with its baneful presence. The mechanical means adopted to this end are awkward to the surgeon, and unpleasant to the patient. Clipping away portions of the slough as fast as it is formed, by means of a pair of forceps and a pair of scissors, is an operation of no small severity, if we can judge by the exclamations of the sufferer. The part affected is intensely irritable; and, however carefully and humanely such removal may be attempted, there must invariably occur sundry pulls and twitchings upon the highly sensitive living part beneath. It is, indeed, rare that such an attempt is not accompanied by a greater or less amount of bleeding,—a proof that the living part has been wounded. Moreover, such removal is meant to be merely partial, and for the sake of convenience; it is not done with a therapeutical intention. If, therefore, a substance could be furnished that would rapidly absorb the slough, as soon as it was formed, without pain, without inconvenience, easy in application, posi-

tive in result, it will at once be conceded that a great desideratum has been obtained.

The cataplasma carbonis of the Pharmacopœia Londinensis has been long and extensively used in gangrene and fetid ulcerations. Its formula is:—

℞. Aquæ ferventis fl. ℥ x., panis ℥ ij., seminis contriti lini 3 x., carbonis contriti 3 iij. M.

Owing, however, to the expense of pure carbon, peat, or better still, bone charcoal, is most generally used; but, in consequence of the impurities contained in the two latter, the bulk of the poultice is made of that substance, linseed meal being mixed up with it. Charcoal having been long known as an efficient deodoriser, this cataplasm has been a favourite remedy for stinking wounds; but I am not aware that any notice has yet been taken of it as an external therapeutical agent; it has been considered merely as a preventive of noisome smells, not as an able assistant in the cure of disease. Even so acute an observer as Dr. Pereira observes, in his great work:—"In this country it is used as a therapeutic agent, principally as a disinfectant and antiseptic, to absorb the fetid odour evolved by gangrenous and phagedænic ulcers. For this purpose it may be used in the form of powder or of poultice. Its disinfecting and deodorising powers, however, are much inferior to those of chlorine or of the chlorides of lime and soda." Now it has been proved that charcoal is the very reverse of an antiseptic, and that its vast power of hastening decomposition is the cause of its utility, not only as a deodoriser, but, as I hope shortly to prove, as a local therapeutical agent.

Exception must be taken to the formula of the prescription above written. Although charcoal does not entirely lose its power when saturated with water, still its efficacy is much diminished. And this assertion is not weakened by the fact, that it is often thrown into the water in casks in vessels about to take long voyages, to "keep it sweet," as it is said. Sufficient charcoal is afforded to allow all bad matter to be absorbed. A case can be mentioned in point: in this hospital, charcoal was placed, in order to catch the urine dribbling from a patient suffering from incontinence, and so prevent the stench. When the charcoal was soaked, the smell was as bad as ever. The drier the charcoal, the more easily do the odoriferent particles enter its pores and become innoxious. It is remarked, in the notices of Hospital Therapeutics published in the number of this journal of Dec. 16th, that one of the necessary conditions that the fetor should be destroyed, is the perfect dryness of the article.

Remembering, therefore, the foregoing conclusions, which, for convenience sake, I now reiterate—viz., that it is highly important that the slough should be removed as soon as possible; that all mechanical means used to remove it are incomplete; that, as proved in my experiment upon the dog, charcoal is a wondrous hastener of decomposition; and that, for the perfect performance of this duty, it should be dry; it was determined to alter the usual formula of the poultice, in order that its effects might be seen upon the slough. The linseed-meal and bread were omitted, and as little water as possible was added—in fact, just enough to cause the poultice to cohere until it was placed upon the body of the patient, the natural heat of which soon caused the evaporation of the moisture, so that the application in a very short time was perfectly, or very nearly, dry. This plan was adopted in order to prevent the mess and consequent annoyance which would have been caused if the wound had been merely sprinkled over with powdered charcoal. There is some difficulty in concocting such a poultice as I have described; so I have thought of remedying this by having it made wet, and then putting it before the fire until nearly all the water is evaporated. This latter remark may appear trivial; but if I am fortunate enough to influence some hospital surgeons to use this poultice, I shall be satisfied if it prevent a groan and a mental curse from the nurse who has to make it. Both peat and some charcoal have been used; of the two, the latter is preferred, but I am not aware that it is much superior to the former in its action.

Poultices made in this fashion have had a most surprising effect in rapidly absorbing sloughs when fairly formed and (so to speak) isolated. They act upon the dead flesh of the wound as the powdered charcoal acted upon the dead flesh of the dog. In St. Mary's Hospital, last summer, there occurred an attack of epidemic gangrene. In one case it appeared, in a most severe and aggravated form, in the stump of a boy who had had his hand removed by Mr. Coulson, the sloughing extending up the

arm for about two inches. In three days the slough was removed entirely. In a neighbouring patient, who had suffered the like operation followed by the same misfortune, and treated with linseed-meal poultices, and the unguentum elemi, the slough took a week and more to remove. I enquired just now of one of our most intelligent "sisters," "What is the difference between the time of removal of a slough if treated by the charcoal poultices, and if treated by the 'green dressing' and linseed-meal?" Her answer was, that, by the former plan, it was taken away in half the time. An infant, suffering from a severe burn, had two enormous sloughing masses, extending, in one leg, from the knee to the ankle; in the other, involving the whole of the calf. In two days and a half both were removed. A man was admitted, Dec. 15th, with a sloughing sore upon his leg. A poultice of this kind was put on. In six hours a most decided change had appeared in the sore; the dead portion was reduced in size fully one quarter.

Such, then, is the power of this substance; its claim to it is easily verified by simple experiment.

Thus is the desideratum obtained; a substance, well known, innocuous and inodorous, is found, *which rapidly absorbs the slough as soon as it is formed, without pain, without inconvenience; easy in application, positive in result.*

2. The deodorising and disinfecting power of charcoal having been well tested by experiment, little remains to be said concerning the other division of the subject to which I wish to draw attention, viz., the possibility of preventing the malarious influence causing the disease to spread from one to all wounds in a ward.

It is not my intention to enter upon the subject of how does this epidemic spread in hospitals. Suffice it to say, that actual contact extends the mischief, and that often a patient lying near another suffering from it contracts the complaint. The distressing smell is alone a proof that particles of decomposing animal matter are flying about. Charcoal has most indubitably the power of absorbing these; the dryer it is, as I before remarked, the easier is its task. The charcoal poultice, then, made as I have described, will effectually prevent any odour or any putrefying portion proceeding from the slough and pervading the apartment.

I wish to lay no claim to aught of originality in this paper. If there is truth in my statements, the whole merit is due to Dr. Stenhouse, whose arguments arrested my attention, and to whose communication to the Society of Arts I am indebted for the idea.—*Med. Times and Gazette, Jan.* 13, 1855, *p.* 28.

147.—ON CHARCOAL AS A DISINFECTANT.

By G. J. Barford, Esq., St. Bartholomew's Hospital.

The substances with which we are acquainted as disinfectants or deodorisers are, chlorine, chloride of lime, lime, charcoal, &c., each of which possesses this power to a certain degree, but not all acting in the same manner. The disinfecting power of chlorine depends on its affinity for hydrogen; thus, decomposing water or aqueous vapour, by uniting with the hydrogen, while the nascent oxygen oxidizes the organic matter, so that unless aqueous vapour is present, chlorine loses a great part of its disinfecting powers, and simply disguises the noxious effluvia, and is itself an irritating, offensive, and corrosive substance. Chloride of lime acts by the oxidation of the putrescent matter; but to do this effectually it requires the presence of an acid; thus, unless a considerable quantity of carbonic acid is present to decompose the hypochlorite of lime, and give rise to the evolution of hypochlorous acid, the chloride of lime will do but little as a disinfectant. Lime acts by the absorption of carbonic acid and sulphuretted hydrogen, leaving other noxious gases unaltered; thus they are all open to serious objections; but the one which practically will be found the most effectual, I believe, has received the least patronage—this is *charcoal*, a body whose disinfecting powers have long been known, but its mode of application has been quite neglected.

Dr. Stenhouse lately called attention to his very ingenious ori-nasal respirator, which depends on charcoal for its efficacy, the action of which is given in the 'Journal of the Society of Arts' for Feb. 1854; the respirator having been noticed in 'The Lancet' of Nov. 25th, I need only mention it as an instance of the powerful

disinfecting power of charcoal; but at once call attention to the plan I have adopted in the application of this agent as a disinfectant, bearing in mind the results of Dr. Stenhouse's experiments, which prove that charcoal not only absorbs noxious vapours and putrescent odours, but at the same time oxydizes them; or, in other words, makes them undergo a slow but sure combustion, which must have its end in the conversion of deleterious gases into compounds, whose physical and chemical properties would admit of an easy separation or removal from their bed of formation, and which on evolution would not be the least deleterious. I therefore, previous to its use, heated the charcoal thoroughly in a covered crucible, with a small hole in its lid, to allow any oxidized material which it might contain to escape, taking care not to have the hole sufficiently large to allow the charcoal to undergo combustion; when thoroughly heated it was allowed to cool, so that on exposure to the air it should not oxidize; in this state it was put into shallow vessels, and placed wherever putrescent odours existed, and in a few minutes the whole of the smell disappeared; but in a day or two the charcoal lost its power. I then thoroughly heated it again, with the same precautions as before, and placed it to perform its duties a second time, which it did with as much efficacy as on the first application; thus, by the repeated cleansing of the charcoal every or every other day, it does not deteriorate, but the same quantity will effectually remove noxious gases for an indefinite period of time.

With Mr. Holden's permission I was enabled to give it a most perfect trial in the dissecting-rooms of St. Bartholomew's Hospital, which at this time of the year must abound in noxious gases and putrescent odours, thoroughly heating the charcoal, and placing it in shallow vessels about the rooms. It acted so promptly, that in ten minutes not the least diffused smell could be detected. So quick and effectual was its action, that arrangements are being made for its constant use. It answers just as well as a purifier of water-closets, drains, wards of hospitals, and sick-rooms. As a purifier of hospital wards, both civil and military, it might be applied with great advantage, saving patients from the unpleasant smells and effluvia from gangrenous sores, and for this purpose a wire gauze construction, containing the charcoal, might be made to surround the affected part at some distance from the dressing; thus the patient himself, and those in adjacent beds, would not be subjected to the influence of the putrescent odours. All these the charcoal would effectually absorb, doubtless with advantage to the patient and his neighbours also. Other quantities of charcoal might be placed in shallow vessels about the wards, and purified every morning as above mentioned. Being at the command of the poor as well as the rich, it admits of universal use; and though it may be objected to as a purifier of the wards of hospitals and chambers of the sick, under the fallacious notion that it would emit carbonic acid, and also on undergoing its daily cleansing would again give off the absorbed gases, yet this notion can never enter the minds of those who understand its action, seeing that carbonic acid cannot be generated unless the charcoal is heated in free contact with the air. This is prevented by having a covered crucible, in which it can be heated to any temperature without undergoing combustion; and the supposition that the absorbed gases are given off again when the charcoal is heated, will be removed by the fact, that they are all oxidized and converted into sulphuric, nitric, carbonic acids, and water, &c., and the heating of the charcoal is for the whole and sole purpose of removing these bodies, which exist in so small a quantity, that they could not be the least prejudicial, even if driven off in the centre of an inhabited room, but, of course, they all pass up the chimney. Thus charcoal is more efficacious than any other disinfectant, when applied as above described, absorbing gases of whatsoever kind, not requiring the presence of any other substance to assist its action, but without stint or scruple collecting noxious vapours from every source, not disguising, but condensing and oxidizing the most offensive gases and poisonous effluvia, converting them into simple, inert, stable compounds; it is simple and economical, coming within the reach of the poorest, and can safely be placed in the hands of the most ignorant, thus combining advantages not possessed by any other disinfectant.—*Lancet, Dec.* 16, 1854, *p.* 505.

It seems almost impossible to exaggerate the importance of the recent modes which have been devised for the application of charcoal to disinfecting purposes. It is still used with the greatest success in the St. Bartholomew's dissecting-room. A few pans of charcoal, thinly spread, are exposed in various parts of the room, and

all the trouble needed is, that the material be kept dry. For this it is necessary simply to warm it before a fire or on a stove once daily. Its effect on the atmosphere of the rooms becomes apparent in a very short time indeed after its introduction, and the place is constantly preserved with an odour not more offensive than that of a cleanly kept butcher's shop. The advantage is not only in respect to smell, since it is further observed, that the bodies keep much longer and in better condition,—a fact easy of explanation by reference to the influence of a noxious gas-laden atmosphere in promoting decomposition. The grand recommendation of the charcoal is, that it is at once an effectual and a *bona-fide* disinfectant. It does not conceal the odours, or substitute others for them; but actually destroys, by decomposition, the poisonous materials on which they depend, and replaces them by others, either innocent, or even useful. Its action, as is well known, is by mechanical catalysis.

The applications to which charcoal may thus be put seem to be quite without limit. Here is a substance by which, almost without trouble or expense, an end may be put to the odours of the dissecting-room, the ward, or the sick chamber. The smell of the cesspool, the sewer, or the ill-constructed water-closet need no longer be tolerated. The candle-maker, the soap-boiler, the tanner, and the bone-grinder, may henceforth pursue their respective avocations without being nuisances to the neighbourhood. It was supposed, at first, that it would be necessary to compel any noxious gas to pass through the charcoal as through a sieve, in order to secure its decomposition, and on this supposition depended the principle of the "charcoal coverlets" for wounds which we described some months ago. It is now found, however, that no such necessity exists, inasmuch as the substance preys with such avidity on the pernicious elements that it requires only to be exposed to free communication with them. Thus to deodorise a Hospital ward, it is only necessary to place plates of dry charcoal at short intervals about the room, say one to each bed, and to secure them from moisture. If only one case in the ward be the cause of smell, it may suffice to hang from the bed of that patient a muslin bag, containing the invaluable material, or to lay it anywhere in near proximity.

A man is now attending the Hospital for Diseases of the Skin on account of syphilitic rupia, who has lost his nose, and suffers from caries of the nasal bones. It is proposed to have constructed for him a gutta percha nose, and to place within it a receptacle for charcoal, by which means he will be enabled to avoid being, as he has been for years, a nuisance to all who approach him.—*Med. Times and Gazette, Feb.* 10, 1855, *p.* 136.

148.—SOME REMARKS ON CHLOROFORM.

By Dr. Corrigan, Physician in Ordinary to the Queen in Ireland.

[Dr. Corrigan makes the following remarks on the exhibition of chloroform by the stomach. He says,]

First—With regard to its dose. For a considerable time past we have been in the habit in our hospitals of administering chloroform in larger doses than are usually advised in works on Materia Medica. To procure sleep in fever (*vid.* Dr. Gordon's instructive communications in Hospital Gazette of February 1st); to afford relief from pain in painters' colic, &c., the minimum dose given is usually half a drachm repeated every two or three hours, and the dose is very frequently increased to a drachm. In delirium tremens, the dose is generally a drachm at first, and is often increased to two drachms. It may, however, be taken not only with impunity, but with advantage in much larger doses.

On the 18th of February, I saw Mr. H——g. He had been leading a most intemperate life, drinking as much as two pints of whiskey daily for a fortnight previously.

When I saw him, he was without sleep for a fortnight, and with the usual signs of the onset of a most severe attack of delirium tremens. I prescribed for him the following mixture:—

Chloroform ℥v., pulv. glycirrhiz. ʒv., mist. camp. ℥ixss.

with directions that he was to take at a dose an ounce, which was equal to half a drachm of chloroform. He again drank after I saw him on the 18th (Monday), and

in the evening took one dose of the mixture, or half a drachm of chloroform. He went to bed in his usual state, and lay without sleep. In the course of the night, when suffering under extreme thirst, he stretched out his hand for a bottle of soda-water; in mistake laid hold of the chloroform mixture, emptied it into the tumbler and swallowed it at a draught. It contained four drachms and a half of chloroform. He slept well after taking it, and suffered neither from burning nor pain next day. He was sick in his stomach on Tuesday and Wednesday, but slept well each night. On Thursday some appearance of jaundice set in. I saw him again on the following Saturday. He was jaundiced and felt slightly sick, but all traces of delirium tremens had disappeared. I could not attribute the supervention of jaundice to the dose of chloroform. It was a complication not unlikely to arise from his habits. We have the main fact, in which at present we are interested, that more than half an ounce of chloroform may be taken into the stomach in a single dose, without causing any poisonous effects. Combination of chloroform with opium will be found, I believe, to form a most useful exhibition in delirium tremens. Its exhibition, on the contrary, by inhalation, in delirium tremens, is of very doubtful utility, and is attended with extreme danger.

Dr. Gordon has, in the February number of this Journal, given some important practical information on trials of its administration by inhalation in fever for the purpose of procuring sleep. The results discourage its use, and as far as my observation has gone, I would also altogether disapprove of its exhibition by inhalation in delirium tremens. The risk to life is very great; two fatal cases have come within my knowledge: in one, death was instantaneous; in the other, the patient sunk into collapse under its administration, never rallied, and died with blue lips and cold surface in less than an hour. Patients suffering under an attack of delirium tremens will not unfrequently suddenly drop dead after a moment of excitement; but still when a patient, even in such a disease, passes into death without rallying from the well-known effects of chloroform inhalation, it appears to me to be sufficient reason for refraining from its use. Even where its inhalation produces sleep, either in fever or delirium tremens, the effect appears to be but transitory. This has been already noticed in Dr. Gordon's observations in respect of fever. In the following case this effect is also illustrated, and although the patient did not die, the escape from collapse was sufficient to prevent re-employment of the remedy.

Mr. K. was seized with delirium tremens, of the asthenic kind, on May 6, 1852. In the course of next day, 7th inst., he had eighty minims of tinctura opii, in camphor mixture, without benefit. On the 8th the quantity of opium was increased; he had sixty minims of tinctura opii in one draught, and afterwards two draughts, with 30 minims in each, without benefit. On the 9th he had a draught every two hours, with thirty minims in each. He took in all eight of these draughts, but still no sleep, and during the night took six draughts more, containing each forty minims tinctura camphoræ, and thirty minims tinctura opii. A similar treatment was continued up to the 11th inst. There was still no sleep, the tongue was dry, and the pupils contracted, showing that the poisonous effects of opium were beginning to develope themselves.

In this condition, as opium could be of no further use, and as I was then not sufficiently acquainted with the valuable action of chloroform given by the mouth, and its freedom from danger, I determined to use chloroform inhalation. It was evident that without sleep the patient must very soon die; and opium had failed to procure it. I used about two drachms of chloroform by inhalation, and with the necessary precautions. The patient became as usual excited, and then suddenly let the head fall back. The lips became blue; the breathing laboured; the pulse became very weak and intermitting; and the pupils dilated. Fresh air, ammonia, and dashing with cold water, kept up respiration and the heart's action; and he passed from this state into quiet sleep; but soon after, awakening at the end of a few hours, the symptoms of delirium tremens returned, putting on, in addition, an appearance of maniacal delirium. This, next day, 12th inst., passed into delirium tremens of the same form as at first, which required for that and next day full opiate treatment. The case recovered; but the risk to life in the case itself at the time of inhalation, the relapse of the symptoms, and the fatal issue in other cases, appear to me to be sufficient reasons for discouraging very strongly the use of chloroform by inhalation in delirium tremens; and we can the better afford to dispense with its aid now

that we are aware in what considerable doses it will be borne by the stomach: and that while it procures sleep it does not entail risk to life. The external application of chloroform is one of our most useful anodynes, but in the directions for its employment externally, as before noticed of its internal administration, it appears to me that it is not usually ordered of sufficient strength; one part of chloroform to seven or eight of ointment or liniment is not of sufficient strength. I think it will not often disappoint if the liniment prescribed is directed to be made of equal parts of chloroform and olive oil. In this form it will be found to give often great relief in ordinary neuralgic pains, and to remove instantaneously those distressing pains, often described as cramps, really neuralgia, which accompany cholera, and which are so often referred to the sides of the fingers and toes, and described as running along the tendons, very often most acutely felt in parts where there are no muscles to give rise to cramps.—*Dublin Hospital Gazette, Nov.* 15, 1854, *p.* 308.

149.—REMARKS ON THE EXHIBITION OF CHLOROFORM.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

Great difference of opinion exists amongst eminent men as to the safety of this agent. Mr. Syme thinks its exhibition as safe as any other powerful agent, and that its danger depends upon its mode of exhibition.]

In inquiring into the reason of this difference between the experience of chloroform in London and here, we have not far to search for the explanation: it must lie in one of three things—viz., difference in the chloroform, difference in the patients, or difference in the mode of administration: with regard to the chloroform, I believe that most of that which is used in London is made in Edinburgh, and I know that some of the fatal cases might be shown to have occurred with Edinburgh chloroform.

With respect to the patients, it appears that great care is taken in London to use chloroform only in persons free from chest affections, especially cardiac derangements: here we never ask any questions as to the state of the heart or constitution of the patients. In all cases where chloroform is required for an operation, it is freely given. Now, considering the frequency of cardiac disease, and particularly of fatty heart,—which, in fact, is I believe rarely absent, at any rate in elderly persons—and considering also the immense number of patients operated on, you cannot doubt that many hundreds with fatty degeneration of the heart have had chloroform administered to them here; we even give chloroform without scruple, where we know disease of the heart exists. Within the last week, a case in point occurred in my practice. A patient, with great dread of pain, and also with a horror of chloroform, had long endured severe pain from his disease, till existence had become a burden, because he could not venture to undergo the necessary operation without chloroform, while his medical adviser considered that to take it would be for him almost certain death, on account of organic disease known to exist in the heart; for which he had consulted, and which it may be remarked had been recognised by Dr. Addison, of Guy's Hospital. At length his medical attendant said to him, that he had suffered so much from his complaint, that even if he died under chloroform this would be better than remaining as he was; while, if it should so happen that he should recover, he would be able to enjoy the rest of his life free from the disease. The patient could not resist the force of this argument, and came to Edinburgh prepared for either alternative. I performed the operation under chloroform; and the first thing that he did on waking was to ask for a cigar.

As another example, I may mention the case of an old gentleman, aged seventy-four, affected with disease of the heart, from whose bladder I removed a large stone, some years ago. Chloroform was administered as he lay in bed; and he was put so fully under its influence, that he was taken from the bed, was operated on, and put back to bed, before he woke from his sleep. He recovered perfectly, but some time afterwards died, and Dr. Begbie, on examining the body, found the disease of the heart, which had been diagnosed during life. We cannot, therefore, attribute the absence of deaths here to our being more discriminating than others in the patients to whom we administer chloroform; the very reverse, in fact, being the case.

I think, then, that we are necessarily led to the conclusion, that the difference of results depends on difference in the mode of administration.

So far as I can ascertain, from what I have heard and read upon the subject, there are important differences between the mode of administration of chloroform here and in London. It appears that here it is given according to principle, there according to rule. There great attention is paid to the number of drachms or minims employed; here we are entirely regardless of the amount used, and are guided only by the symptoms of the patient. The points that we consider of the greatest importance in the administration of chloroform are—first, a free admixture of air with the vapour of the chloroform, to ensure which, a soft porous material, such as a folded towel, or handkerchief, is employed, presenting a very large surface, instead of a small piece of lint, or any other apparatus held to the nose. Secondly, if this is attended to, the more rapidly the chloroform is given the better till the effect is produced; and hence we do not stint the quantity of chloroform. Then—and this is a most important point—we are guided as to the effect, not by the circulation, but entirely by the respiration; you never see anybody here with his finger on the pulse while chloroform is given. So soon as the breathing becomes stertorous we cease the administration; from what I have learned, it is sometimes pushed further elsewhere, but this we consider in the highest degree dangerous. Attention to the tongue is another point which we find of great consequence. When respiration becomes difficult, or ceases, we open the mouth, seize the tip of the tongue with artery-forceps, and pull it well forward; and there can be little doubt that death would have occurred in some cases if it had not been for the use of this expedient. We also always give the chloroform in the horizontal position, and take care that there is no article of clothing constricting the neck. There are thus considerable differences between our practice and that which prevails more or less elsewhere. We use no apparatus whatever, take the respiration for our guide, attend to the condition of the tongue, and never continue beyond the point when the patient is fully under the influence of the anæsthetic.—*Lancet, Jan.* 20, 1855, *p.* 55.

150.—*Anæsthesia by Cold.* By Thos. Wakley, Esq.—[Mr. Wakley thinks that we may produce a sufficient degree of local insensibility by cold to enable us to do without chloroform in many cases. His words are:]

I have tried the ice in several cases, in both hospital and private practice, and in almost every instance the success was evident, the patient when blindfolded being ignorant of the use of the knife. It affords me great pleasure to confirm the evidence of Dr Arnott upon this interesting subject, and to draw the serious attention of surgeons, both at home and abroad, to this agent, whereby patients can be freed from the pain which attends many every-day operations we are called upon to treat with the knife, and reserve chloroform for those very severe cases where either the duration of the operation or the depth of structure requires the employment of that potent agency.—*Lancet, Feb.* 3, 1855, *p.* 140.

151.—ATMOSPHERICAL OZONE AND THE CHOLERA.

According to Professor Schönbein, the discoverer of cotton-powder, a complete absence of ozone has been remarked in the atmosphere at Berlin during the invasion of the cholera. Dr. Bœckel, of Strasburg, informs us, that in that town also the presence of cholera coincided with the absence of ozone, and that ozone re-appeared as soon as the epidemic decreased.

Dr. Billiard, of Corbigny, is of opinion, that the diminution of ozone in the atmosphere is the first cause of cholera, and that this modification of the air brings forth a change in the animal organisation, in consequence of which, the liquids contained in certain vessels and the substances contained in the digestive tube, are withdrawn from vital action, and only remain subject to the forces by which inert matter is ruled.

Thus the production of a putrid fermentation, the disengagement of gases, and

other physical phenomena, by means of which, according to Dr. Billiard, "all the morbid symptoms of cholera, from its period of incubation, to its fatal, or favourable termination, may be explained.

What is ozone? To this question we will endeavour to give a comprehensive answer, as the subject is well worthy of attention, and its assiduous study, added to a daily meteorological observation, promises to be no less interesting to the physiologist and pathologist than it is at present to the chemical philosopher. Ozone is nothing else than oxygen itself, but so different from the body to which, since Priestley's discovery, we are accustomed to give the name, that it has been extremely difficult to find out its real nature.

Oxygen has no smell. Ozone, on the contrary, has a very penetrating odour; resembling, at the same time, that of chlorine mixed with air, and of phosphorus or sulphur in combustion.

It is the odour which manifests itself after repeated electrical discharges. The new permanent qualities acquired by oxygen when it changes into ozone are not confined to its smell alone. Its oxydizing power is much greater than that of common oxygen. The latter combines only very slowly with mercury at an ordinary temperature; the former, on the contrary, unites itself very rapidly with this metal.

Van Marum was the first who found this remarkable body, in 1785. Having at his disposal the great machine of Taylor's Museum, he excited sparks in a tube filled with oxygen. After 5,000 sparks the oxygen had acquired a strong smell, "which seemed to us," says this natural philosoopher, "to be clearly the smell of electrical matter." From 1785 to 1840, these remarkable experiments were completely lost sight of; but in the latter year, Professor Schönbein, while decomposing water by a galvanic battery, remarked, that the production of the hydrogen gas was accompanied by a peculiar smell, and published a paper on this subject. What was this new body? A simple one, or a composition of oxygen with hydrogen or azote. The ingenious chemist left the question undecided, but gave the name of ozone to the odorous substance.

Eight years after, Mr. Williamson, a member of the Royal Society of London, pronounced, that according as ozone is produced by a galvanic battery, developed by the electric spark, or brought forth by the action of air or phosphorus, it is a hyper-oxide of hydrogen and azotic acid, or a mixture of the two. Professor Schönbein had meanwhile continued his researches. In 1850 he expresses an opinion that ozone is a highly oxygenated combination of hydrogen. In 1851, Messrs. Martignac and De la Rive came to the conclusion, after a series of experiments, that ozone is nothing else than oxygen in a peculiar state of chemical activity, impressed upon it by electricity. Berzelius and Professor Faraday also believed in a simple modification of oxygen, in an isomeric or allotropic state of the body. Professor Schönbein, taking up the question for the third time, ranged himself in 1851 to the opinion of M. Martignac and De la Rive. Most chemists, however, still hesitate to admit the modification of oxygen; but the experiments published in 1852 by Mr. E. Fremy and Edmund Becquerel seem to have removed all doubts on the subject.

Thus, ozone is only a peculiar form of oxygen produced by electricity; a change analogous to that which the solar rays bring forth in chlorine by rendering its affinities more powerful, or to the modifications which are excited by heat in sulphur, phosphorus, and carbon.

This fact having once being admitted, the question naturally arose, whether the changes we bring about in our laboratories are not produced spontaneously in the atmosphere; and this was very soon affirmatively answered. Since 1850, Professor Schönbein had ascertained that ozone decomposes iodide of potassium, and concluded that the best re-agent for finding out the presence of ozone is starched paper containing a small quantity of the iodide. Paper thus prepared and exposed to the action of the atmosphere soon revealed the presence of ozone. But it was evident that this singular body could not always be contained in the air in the same proportions; and to study these variations, a scale must necessarily be formed. An ozonometrical measure was easily established by dividing into ten shades the chromatic interval between the absence of ozone, which is white, and the deepest colour which this substance can possibly bring about by decomposing iodide of potassium. By means of this scale, the daily variations of atmospheric ozone may be ascertained

in the same manner as those of the temperature and weight of the air are indicated by the thermometer and barometer.

Several men of science, impressed with the importance of this new branch of meteorological observations, have devoted peculiar attention to it. The following table, published by the 'Strasburg Medical Gazette,' gives us the mean of the observations of Dr. Bœckel, of Strasburg, and Mr. Simonin, of Nancy:—

	Simonin.		Bœckel.	
1853.	Morning.	Evening.	Morning.	Evening.
April ...	5·93	6·66	5·50	6·33
May ...	5·71	6·71	5·70	6·60
June ...	5·55	6·37	4·34	6·25
July ...	4·44	5·42	4·10	6·50
August	4·39	4·74	4·33	6·18
September .	5·23	5·80	3·00	4·83
October ...	3·94	4.32	3·00	3·50
November .	3·50	0·93	1·60	0·50
December .	5·16	1·58	4·00	2·00
1854.				
January ...	—	—	5·35	2·71
February ..	—	—	7·00	4·50
March ...	—	—	3·50	4·40

Dr. Bœckel makes the following remarks:—

"In summer, the ozonometrical mean is, according to my observations, constantly more considerable during the day-time than it is at night; the ozonoscopical paper being exposed from morning to evening to two maxima of electricity; while during the night the re-active paper only traverses one maximum period, which is between two and four in the morning.

"In winter, the inverse seems to take place; the electrical affinities being then most likely less energetic, in consequence of the absence of the sun, although experience seems to have proved that a greater quantity of electricity is produced in winter.

"During a fog, the ozonoscope frequently marks zero, as only the rapid formation of vapours, or their precipitation in rain or snow, is accompanied by a disengagement of electricity. This took place during the last four months of last year, which were frequently foggy. The ozonometrical mean of these months has constantly been inferior to that of the remainder of the year.

"Thus we have here a body, the existence of which was not even suspected a few years ago, and which never ceases to act upon us, and upon all animated nature. As to the intensity of its action, it cannot be doubted; for who can question that considerable variations in the oxydizing power of respirable gas have a powerful influence upon respiration and consequently upon all the vital functions."

According to Dr. Bœckel, malaria always shows itself with the zero of the ozonoscope, and the same takes place when intermittent fevers are prevalent.

According to Professor Schönbein, a considerable quantity of ozone was observed in the atmosphere at Berlin during an epidemic of influenza, and under a medical constitution predisposing to pulmonic affections; and the contrary took place under the reign of a gastric constitution.

Finally, as we mentioned at the head of the article, the cholera coincided at Strasburgh and Berlin with an absence of atmospherical azone.—(From *La Presse.*)—*Med. Times and Gazette, Jan.* 6, 1855, *p.* 16.

152.—CHOLERA TREATED BY THE ARTIFICIAL PRODUCTION OF PERITONEAL OR CELLULAR DROPSY.

By Dr. B. W. Richardson, Mortlake.

[Dr. Richardson suggests a novel method to make up for the loss of fluid which seems to be one great cause of the collapse in cholera. Instead of giving fluids by

the mouth, or injecting them into the veins, he would inject them into the cellular tissue or the peritoneal cavity.]

As the loss of fluid from the system is the cause of the collapse, as in many cases it cannot be replaced by the natural process of imbibition from the intestinal surface, and as the blood cannot circulate and reaction occur, without such replacement, there is open before us these plain and simple physiological problems—Are there any other means by which fluid can be introduced into the exhausted circulation? and, if so, which of these is the safest, and most calculated to effect the object desired?

We have seen that the mucous membrane of the alimentary canal, while carrying on an extensive excretory process, cannot invariably be made a medium for absorbing fluid to any great extent. Some practitioners have, however, believed that the administration of large quantities of fluid by the mouth, during the intervals of rest from vomiting, has been attended with benefit; and there surely can be no objection to a plan so reasonable, unless the swallowing process should itself excite vomiting, and especially as patients themselves almost invariably crave urgently for drink.

By other practitioners, and by one set of charlatans, the external application of water to the body has been advocated; and this plan is also reasonable to a certain extent, for it is well known that, at a push, the skin will take on a modified form of absorption. Thus the body has been found in some degree to be nourished by the immersion of its parts in nutritive fluids, in cases where admission of liquids into the stomach is rendered impossible by disease: and in the voyage of Captain Bligh, after the mutiny of the *Bounty*, when the sailors were without drinking water for many days, and were exceedingly exhausted, their pangs of thirst were remarkably assuaged by dipping their clothes into the sea, and afterwards wrapping them round their bodies. I do not know from experiment to what extent the skin will absorb water; but it is quite certain that in cholera, where the loss is so sudden and extensive, and where a supply equal to the loss is required, no amount of immersion could be expected to do marked good, to say nothing of the inconvenience of such an application in cholera cases.

The most important step that has as yet been made on the human subject towards supplying the exhausted system with fluids in cholera, is that of throwing a considerable quantity of fluid directly into the circulation through the veins: and it cannot be denied that, in practice, this plan has been attended with some amount of success. That it should not be generally successful, is, I think, strictly explainable on physiological grounds. In the first place, the plan is objectionable on the mechanical argument, that to throw a large amount of fluid through a vein directly into a heart itself exhausted, is sufficient to paralyse that organ to a serious degree; for I have found, by direct experiment, that a weakened heart often succumbs instantly from overdistending it with blood itself. To meet this difficulty, I have sometimes thought that a long narrow syphon tube, with one end fixed in the vein, and the other immersed in the fluid, would be much better than the syringe; since the suction power of the circulating organ would gradually take in from the reservoir the required element. I throw this out merely as a suggestion, unsupported as yet by experiment.

Another objection to the direct transfusion of water, or of water impregnated with saline or other materials, into the circulation, is, that such fluid is not the proper stimulus for exciting the heart to contraction; and that, even if the heart does contract upon it, and throws it round the pulmonic circuit, it is, again, not a substance upon which the process of respiration can properly act. When a saline substance, injected into a vein, accomplishes anything, it does so, I imagine, by dissolving the half-clotted blood in the cardiac cavities, and by carrying the blood, thus diluted, round the lungs. The mention of this idea indicates how slowly fluids should be introduced into veins, and supports the notion of the syphon tube, alluded to above.

But the last and main obstacle to the success attending injection into a vein, is, that by this process no such competent amount of fluid can be introduced so as to produce a permanent effect: and, in truth, we too often find in practice that, after such injection, the patient only rallies for a brief period, to sink again under a continuance of the discharge from the intestines. I do not know what is the largest amount of fluid that has ever been injected into the vein of a cholera patient; nor yet the largest amount of fluid that has been known to be lost during the disease. Each of these questions ought at once to be ascertained, and might easily be so: the first, by each practitioner keeping a careful record of his treatment of cases; and

the second by weighing patients at the commencement and end of the disease, and by calculating the loss of weight sustained. In hospitals, this last fact could easily be learned, without teasing the patient, by having beds made as weighing machines; taking account of course, of the alterations in weight produced by clothes or other appendages. Speaking from general observation, however, I may say that the largest amount of fluid I have ever heard of as having been injected into a vein in the course of a case is from seven to ten pounds, and the most I have ever seen injected is from three to four pounds; whilst I have known a cholera patient to pass off by one vomit from two to three pounds, and repeat this many times in the course of three or four hours. I speak on this matter with great caution, and strictly subject to correction; wishing only to convey to the mind the idea that, by the ordinary processes now in use, no sufficient quantity of fluid can be injected by a vein to make up a tithe of the serum lost, which is the point to be desired. I make no mention of any of the risks that may arise from injection into a vein, such as introduction of air or phlebitis; because these, though important in themselves, are of secondary consideration in extreme cases of cholera, when compared with the main object of diluting the blood and re-establishing the circulation.

In contemplating the subject of transfusing watery matter into the system after the modes I have described, the question occurred to me, whether we ought to confine ourselves to these modes, or whether any other means existed of introducing fluid matters into animal bodies in very large quantities, and in a manner which should secure their absorption. This thought led me to look back upon the pathological characters and treatment of those diseases in which serum is largely thrown out into serous cavities or cellular tissue. I recollected that in cases of this class (dropsical cases,) not only were several gallons of fluid thrown out of the circulation into the system without immediate danger, but that patients thus situated could tolerate a degree of purgation which would absolutely destroy healthy individuals. This immunity must arise solely from the fact that the effused serum lies as a reservoir, from which the circulation is fed while the purging is going on. The idea furnished an important suggestion; and I consequently commenced a series of experiments on animals, which, as they are as yet in an imperfect state, I shall only describe in general terms, reserving the particulars for my next report.

The experiments up to this time have mainly consisted in exhausting dogs and cats by starvation and violent purgation by large doses of elaterium, and afterwards injecting either their peritoneal cavities or their cellular tissue with large quantities of distilled water, at the blood temperature. The results, in general terms, are as follows,—

I find that either into the peritoneal cavity, or into the cellular tissue, a quantity of water, varying from a tenth to the fifth part of the weight of the animal, may be injected with little risk. The effect of this is to induce a sleepy condition, which lasts from twenty to thirty hours, long before which time all trace of the injection is lost. If more than this is thrown in, the sleep, or rather torpor (for the animal only remains quiet, and rouses when spoken to,) ends in death. This was the case in one dog weighing ten pounds, into whose cellular tissue I injected three pounds of water. In animals that have been much exhausted, more fluid may be injected, and the absorption is greatly quickened. In animals which have died from the experiment, and have been examined immediately after death, I have never found any trace of the injected liquid; but the blood has always been exceedingly fluid, and has coagulated slowly, if at all. I should notice also, that in two cases, in which a cat and a dog were operated on while under the influence of an anæsthetic agent, they died; the one in four, the other in forty-eight hours, without ever rallying fairly from the effects of the narcotic. I should mention, too, that I have met with one or two casualties in the course of these experiments, which have arisen purely from their novelty, and cannot be considered as bearing on the merits of the system. Thus, in two young cats of the same age, I injected a sixth part of their respective weights of water into the cellular tissue of one, and into the peritoneum of the other; the result was, that the one whose peritoneum was injected, after remaining apparently well until three or four hours previous to death, died about twenty-four hours after the operation. On opening the body all trace of fluid was absent; but I found that, from having made too large an incision into the peritoneum, and from the animal having reclined on the belly, about two inches of intestines had slipped into the

opening, and had become strangulated, and deeply congested. In another instance, where I inserted pins into two openings, erysipelatous inflammation came on in the surrounding skin; but not in another part of the body, where the wound was left untouched. In a third casualty which occurred to me, I learnt a very useful lesson. Into the peritoneum of a dog three pounds of water were thrown, and one pound into the cellular tissue of the thigh. The animal seemed pretty well for a short time after the operation, and the fluid was absorbed in the course of three or four hours, although it constituted a fifth part of the weight of the whole body. In a little time, the creature gradually became exhausted, and died in eleven hours. On opening the body, I found that all the fluid injected had disappeared; but in the peritoneum there lay about a pound of semi-coagulated blood, which had escaped from an artery that had been wounded by the trocar. The blood in the veins and heart of this dog was like water itself, and showed no tendency to coagulate. The heart was flaccid and collapsed. In this case, the rapid absorption of the injected fluid arose doubtless from the hemorrhage, and from the too rapid and copious flow of fluid into the exhausted circulation. For, while the blood lost its solid constituents on the one hand, it gained too large an amount of water on the other. This animal had been reduced somewhat by purgation. Previous to the operation, he weighed twenty-six pounds; immediately after it, thirty pounds; and, at his death, a little more than twenty-four pounds. He must consequently have made use of all the injected fluid, and have lost somewhat more than one pound additionally, during the interval between the operation and death.

Setting aside, therefore, these casualties, I infer from my experiments as they now stand, that into the peritoneum or the cellular tissue of a patient in a proper state of collapse from cholera, water to the extent of at least a tenth, or even a fifth, part of the whole weight of the body might be injected with safety; and that the absorption would be almost immediate.

The performance of the operation for producing this artificial dropsy is exceedingly simple. The instruments required are simply a scalpel, lancet, or trocar, a common catheter, and an ordinary enema syringe. If the cellular tissue is to be injected, a deep puncture is made with a lancet in some fleshy part, the abdomen is the best, and the blunt point of the catheter is then introduced, and wedged sufficiently far into the cellular substance to allow the skin around it to be firmly grasped, so as to keep it steady in its place. The other end of the catheter being connected with the enema pump, an assistant very gradually throws in the fluid, and, as the tissue distends, gentle compression may be made with the hand so as to force on the fluid under the skin. Into the leg of a dog of middle size, a pound and a half of water may thus be injected in a quarter of an hour; and the experiment may then be recommenced at some other point if desired.

For puncturing the peritoneum, I am now having made a special trocar, which I will describe at a future time; but the scalpel or lancet, if used with care, answers exceedingly well. A careful plunge into the cavity is made in the linea alba; and the blunt point of a catheter is introduced, and grasped as before mentioned. Fluid may in this case be thrown in with much greater rapidity for a considerable time; but I have observed that, as the body distends, the action of the diaphragm becomes somewhat interfered with, upon which the operation should cease for the time. I have injected three pounds of water into the peritoneum of a middle-sized dog before producing this effect. The wound must be carefully closed with stitches; but a little of the water will always escape, and become diffused in the cellular tissue of the abdomen.

The steps of the operation in the human subject would not vary materially from those I have already described. The opening into the peritoneum should be made with a small scalpel through the linea alba on the left side, about two inches below the umbilicus; the blunt end of the catheter should then be introduced and steadily grasped, the fluid being meanwhile made to pass slowly through it, so as to prevent the injection of air. The freeness with which the fluid flows through the tube will show whether or not the peritoneum has been freely entered. For injecting the cellular tissue, some point of the body should be selected where the parts are loose, such as the abdominal parietes, or the back; in these parts the cellular substance is extensive, and would offer but little resistance to the passage of fluid.

I am not at this moment able to say which of the two kinds of operation is best,

but I am strongly inclined in favour of injecting the peritoneum; for, although the absorption of fluid from the cellular structure seems most rapid, the difficulty of introducing water in full quantities into the peritoneal cavity is much less, and the pain attendant is considerably milder. In either case, too much care could not be taken to throw the fluid in slowly, to have it at a temperature not less than 92° Fahrenheit, and, if the peritoneum is the part injected, to be mindful not to distend the cavity to the extent of interfering with the action of the diaphragm.

As yet I have had no favourable opportunity of trying the suggestion I have here thrown out in cholera; but that it might be put into application in desperate cases and might prove advantageous, I have no doubt whatever. The advantages of it would be:—

First. That an abundant store of fluid would be supplied, which the exhausted circulating system would rapidly and effectually take up.

Second. That as this imbibition would take place through the capillary system, the fluid would enter the veins freely charged with the solid constituents of the blood, and would pass to the lungs in a condition suitable for respiration.

Third. That the process would in no way interfere with the administration by the mouth of astringent solutions or such other medicines or liquids as the practitioner might consider indicated.

But, although I am thus sanguine on this subject, I wish it to be understood that the first trial of this process should commence in a case where the patient is in the last stage of collapse, where other remedies have failed, and where death is imminent. This, it is true, would scarcely be putting the new remedial measure to a fair test; but my ideas of the value of life are so great, that I would not, even in so important a disease as cholera, recommend the employment of a new remedy wantonly, or without that due care for the welfare of the sick man which every honest practitioner of medicine intuitively feels.

The subject once started, branches off in various ways, and opens many points for investigation. If simple water can be taken into the system through the cellular tissue or peritoneal membrane, it remains to be ascertained whether nutritive or medicinal substances, which could not obtain admission by any other way, might thus be introduced. It is interesting and important to know, that the peritoneum has been found capable of absorbing some medicinal agents. I am informed by Dr. Cormack, that many years ago, he, and the late distinguished Professor of Anatomy at St. Andrew's, Dr. John Reid, threw into the peritoneal cavities of dogs various medicinal substances; and that the proofs of the absorption of these substances were speedily manifested. Dr. Christison, also, in his 'Treatise on Poisons,' p. 29, thus remarks:—"The serous membranes possess an activity of absorption which hardly any other unbroken texture can equal. Accordingly, many poisons act much more rapidly through the peritoneum than through the stomach. When oxalic acid is introduced under the same collateral circumstances into the stomach of one dog and the peritoneum of another, the dose may be so apportioned that the same quantity, which does not prove fatal to the former, kills the latter in fourteen minutes."

It may be urged that the act of injecting either the peritoneum or cellular membrane could not be performed without some risk of exciting inflammatory action. And that this would often be the case in *healthy individuals* I have no doubt. But in the collapse of cholera, when all the great processes of the economy upon which inflammation depends are in a state of abeyance, such a result is in no way probable; and if it were, it ought under the circumstances to be regarded as a matter of secondary consideration.

I should scarcely omit to refer, in conclusion, to the fact (though it is one pretty generally understood) that impostors, in feigning diseases, have been known to inflate their own cellular tissue or that of their children with air, without any harm following; and the authors of the article, "Feigned Diseases," in the 'Cyclopædia of Medicine,' make special reference to the statement, that some French conscripts were known to inject water into their own peritoneal cavities, to produce a factitious ascites; while numerous instances have lately been recorded, in which, after the operation of tapping, such irritating substances as iodine and brandy have been thrown into the peritoneum without leading to fatal results.

Dr. GOOLDEN, physician to St. Thomas's Hospital, expressed the high opinion

which he entertained of Dr. Richardson's paper. He thought, however, that it was a great mistake to attribute collapse to the serous vomiting and purging. He had seen many cases of fatal collapse in cholera, in which there had been neither one nor the other of these occurrences. How, then, could this state of collapse be obviated or relieved simply by the introduction of fluid into the system? In support of his views, Dr. Goolden referred to the nearly uniform fatality of the cases treated by injection into the veins. He, however, advocated the introduction of large supplies of fluid into the stomach, as he found that in this way sickness was allayed. He would give ice, or water at an ordinary temperature. In 1832, when in a remote part of the country with a party of friends, he saw a case of cholera which had made a strong impression on his mind. The woman of the inn where he was with his friends was seized with cholera, and rapidly became cold and collapsed. No medicine chest and no drugs of any kind could be procured, and at last a messenger was sent off seven miles in search of a doctor, but no doctor and no medicine was obtained. The woman called urgently for water. A pail of water was brought to her bedside, and he (Dr. Goolden) threw into it, he hardly knew why, the contents of a saltcellar. Cupful after cupful was taken, till the woman had finished all that was in the pail. It was then refilled, and this second supply she also drank. Altogether she swallowed about two gallons of this solution of salt. After the first few draughts the sickness abated; and her recovery was speedy and perfect, though she took no medicines of any kind. Dr. Goolden enlarged upon the mortality which arose from too active drugging, both medical and domestic. During the present epidemic, thirty-six cases of collapsed cholera had been admitted into St. Thomas's Hospital; and of these twelve only had died. But what was most deserving of notice was this, that in twenty-one of those cases in which no drugs had been given there were only six fatal cases. The following plan of treatment is now adopted at St. Thomas's. An emetic of ipecacuanha is first administered: then from half a drachm to a drachm of sulphuric acid in peppermint water is given every half hour or hour, until the vomiting and purging cease. When the vomiting is obstinate, a few drops of creasote are given. Sinapisms, warm fomentations, and thorough covering with blankets, were also means always employed. The patients were allowed to drink water *ad libitum*. In the previous epidemic "secondary fever" had carried off two-thirds of the cases; in the present epidemic not one case had been lost in St. Thomas's Hospital from this cause. This was probably owing to opium and other dangerous medicines, so freely employed on the former occasion, not having been employed on the present.

Mr. CARPENTER, of Croydon, called attention to the manner in which the red corpuscles of the blood were affected in the cholera. These corpuscles performed the function of carrying oxygen from the lungs through the arterial system, and of carrying back carbonic acid through the venous system to the lungs. When, as in cholera, a large quantity of fluid is lost from the blood, the corpuscles are arrested in their circulation, they contract, and in the stage of collapse they become altered in their chemical characters. Injection into the veins had been unsuccessful, because the red corpuscles were over-distended by the sudden introduction of water, and burst. It was a desideratum to find a means of introducing fluid, which should not be liable to this objection; and this was in some degree afforded by the process described by Dr. Richardson. The difficulty presented was the want of albumen, and of correspondence in the specific gravity of the fluid injected with that of the blood. In the treatment, he objected to terebinthinates and ethers, as liable to produce secondary fever; and he would trust rather to the administration of cold water in abundance, and mild saline solutions.

Dr. CORMACK considered the paper of Dr. Richardson as strictly philosophical and extremely able. The new proposal which it conveyed to the profession was a step in the right direction, and was in accordance with that sounder practice in cholera which was now becoming general. He (Dr. Cormack) could not concur with Dr. Golden, in thinking that the collapse of cholera sometimes occurred when there had been no pouring forth of serous discharge from the intestinal mucous membrane. He had never seen such a case: he had never been able to hear of any such case well authenticated. Though the discharge did not appear outwardly, by vomiting or purging, it might on dissection be found absolutely to fill the intestines. This was recorded by various observers. Again, the vomiting and purging was gene-

rally far advanced or almost finished before cases were received into hospitals: and the fact of no discharge being reported was not satisfactory evidence that none had occurred. On what does the shrunken, senile, mummified aspect of the collapsed patient depend? It can only depend upon the loss of fluids from the body! If it be true, then, that the loss of fluid leads to collapse, it follows that the prevention of that loss must form an essential part of our treatment. It likewise is evident, that if from the severity of the case or the neglect of early treatment, the case had advanced into collapse, a rapid supply of fluid is obviously essential. A little seasonable aid of this kind may help the patient over his difficulty. We do not throw in the fluid to *cure* the cholera! Our object is to enable the patient to outlive the paroxysm, which we know must soon terminate either fatally or favourably. The cholera paroxysm is similar in kind to, and not generally more prolonged than that of an ague. The fluid artificially introduced, it appears, need not be exactly similar to that which is lost; but the nature of the fluid to be used with most advantage we cannot yet exactly determine. Upon the whole he (Dr. Cormack) would, with Mr. Carpenter, prefer simple water, if the peritoneum was to be injected; and an abundance of water slightly salted with common salt, or acidulated with sulphuric acids, if the patient were to drink it. The chief point, however, seemed to be to give plenty of fluid. Some medicines were useful, but fluids could never be dispensed with. Ice and cold beef-tea (not strong) he generally ordered *ad libitum*. Dr. Cormack concluded by referring to the case detailed by Dr. Goolden as being full of instruction. Cases of such severity are generally so much interfered with by treatment that they rarely convey much information. The recoveries which take place under opposite methods of treatment, and such cases as that of Dr. Goolden, clearly establish the paroxysmal character of cholera attacks, and point out that such attacks may spontaneously terminate in recovery, just as do attacks of simple ague.—*Association Med. Journal, Aug.* 25, 1854, *pp.* 765, 768.

153.—CHOLERA TREATED SUCCESSFULLY BY SALINE INJECTIONS.

By Henry Ancell, Esq.

[We give the following case to show the effects of the saline treatment in recovering from an apparently hopeless state of collapse. The woman was 44 years old, the upper housemaid in a private family. Mr. Ancell says]

August 16th, I first saw her at half-past 11, a.m., six hours after the attack, in a state of complete collapse; there had been violent vomiting, with perfectly colourless fluid dejection, containing rice-like grains and white flocculi; violent cramps, and total suppression of urine; she was pulseless, with a feeble, scarcely audible, fluttering heart, lividity of the countenance, coldness and blueness of the extremities, coldness of the tongue and breath, and clammy perspiration. Frictions of the surface were ordered, and sinapisms to the arms, legs, and epigastrium. Brandy, ether, and ice, and several other remedies, were given with a view to restore the circulation, all being rejected. Up to half-past two o'clock there had not been the slightest sign of reaction; the lividity and coldness of all the extremities increased, the respiration was hurried, the countenance more collapsed, and dissolution was manifestly impending, although her mental faculties were entire, her voice tolerably good, and she could reply to questions. A case in the neighbourhood, in which the collapse was not so complete, terminated fatally on the previous day, four hours after I saw it; and from my experience of the epidemic in the year 1832, my opinion was that, under all ordinary modes of treatment, or if left to itself, this case would probably terminate in an hour or two, and could not be much longer protracted.

Under these circumstances, with the co-operation of my friend Mr. Lane, I proposed the injection of a saline fluid into the blood. The patient at first withdrew her arm and feebly objected, having always had a prejudice against bleeding and operations of every kind; but was soon overruled. A solution of about one drachm of chloride of sodium in a pint of water, at about blood heat, was accordingly injected into the basilic vein. During the injection the pulse returned to the wrist, but was extremely feeble, and shortly afterwards she expressed herself less faint; this was

followed by a distinct rigor, which lasted perhaps ten minutes; and this by an improvement of the countenance, a slight degree of warmth to the extremities, and a complaint of pain in the back and head. After the rigor, she had very little cramp; but the vomiting and diarrhœa continued with unabated violence.

At six o'clock, about three hours after the injection, the collapse had returned. Diluted sulphuric acid and laudanum, and every other remedy given to avert the vomiting and purging, had proved unavailing, and she was again pulseless, cold, and livid. The tube had been left secure in the basilic vein, and in a feeble and gasping tone, she begged that she might have some more of the fluid. I now injected about two pints more of the solution of chloride of sodium somewhat stronger. The pulse returned, and during the operation she twice or thrice complained that it made her back ache; there was no rigor, but the reaction appeared from the first to be more complete than before; nevertheless, the purging continued unabated; if the stomach was allowed to remain at rest by giving nothing, the vomiting returned at intervals, and everything taken into it was immedintely rejected.

The pulse continued at about 110 and tolerably strong, until near 11 o'clock; I then watched its rapid failure in power until 12 o'clock, when it was again nearly imperceptible. The lividity of the countenance and gasping respiration returned; the action of the heart was nearly imperceptible; she was excessively restless, and intimated that the fluid had run off by her bowels, and that she wished for more. It appeared to me, that the effect had shown either that a simple solution of chloride of sodium was not adequate to the restoration of the normal fluidity of the blood and the circulation, or that the quantity injected was insufficient; and I therefore resolved to employ a stronger solution in larger quantity, and to fall back upon a mixture of salts employed by several practitioners in 1832. Five pints of a solution composed of 64 grains of chloride of sodium, 64 grains of carbonate of soda, and 32 grains of chlorate of potass to each pint of water, (16 ounces,) were now injected—half a grain of iodine being added to the five pints. The pulse returned, as in the former operation, but was much stronger, and the remarkable and satisfactory result followed, that, instead of remaining stationary and then failing, it continued to improve from the period of the injection: so that at four o'clock in the morning, the action of the heart, the pulse at the wrist, the temperature, and the general aspect of the patient so much improved, that I removed the tube from the vein. The vomiting and purging continued, although somewhat abated.

Aug. 17th. Three o'clock, p.m., twenty-four hours after the first injection of the veins, about thirty-six hours from the attack, after the continuance of a state of apparently hopeless collapse for at least four hours, and the subsequent injection of from seven to eight pints of a saline fluid into the circulating current with a view of restoring the circulation, the patient was in the following state:—the action of the heart perfectly regular and uniform; the pulse 96, tolerably strong and regular; the lungs crepitant throughout, and the respiration good; the temperature of the surface moderate; the lividity of the countenance replaced by a deep sallowness; pain in the epigastric and præcordial regions; great thirst; the purging and vomiting unabated, and the respiration slightly hurried immediately before and after the vomiting; no clear evidence of any return of the renal secretion, and the choleraic character of the dejection unchanged.

Sinapisms were reapplied to the epigastrium and to the extremities, and during the day various remedies were resorted to for the purpose of arresting the vomiting, none of which appeared to do any good except ice, which was most grateful, and was taken in considerable quantity. In the evening I prescribed fifteen grains of trisnitrate of bismuth in a little barley-water, followed by some ice, to be given every hour to eight doses should the vomiting continue, and large nourishing enemata, consisting of warm milk and beef-tea, to be repeated several times during the night.

Aug. 18th. The patient had, upon the whole, a much better night. The reaction was completely established, the pulse 110, and rather full. The vomiting nearly ceased after taking five doses of the trisnitrate of bismuth. She had retained some ale, and also some port-wine, on the stomach. The purging continued, but was less frequent, and two of the enemata were retained for about an hour. The alvine evacuations had changed to a bright yellow colour, were much less liquid, but slimy with green streaks. The sounds of the heart were quite normal; the respiration was a little hurried previous to straining to vomit, which occurred occasionally, when

a small quantity of yellowish-green ropy mucus was ejected. The tongue, which had hitherto been perfectly clean, was now white; there had been a little sleep; her intellects were perfectly clear; but she complained of great weakness, and was restless.

Aug. 19th. She had a very bad night, with diarrhœa and vomiting, although she retained some warm brandy and water in the stomach; violent pain over the whole abdomen; great flatulent distension, particularly around the umbilicus; pulse rapid; tongue brown and dry; hiccup; cold perspiration; complexion and general surface yellow; extreme restlessness; deep moaning; occasional slight wandering. A large hot linseed poultice was applied over the abdomen, and a turpentine enema administered; but she became listless for an hour or two, and died at two o'clock; three days and eight hours from the attack, and nearly three days from the first injection of saline fluid into the basilic vein.

Autopsy, seventeen hours after death.—The body was plump and in good condition; its surface remarkably free from lividity, except in the decumbent parts, where the sugillation was not more than usual at this period after death; and the *post mortem* rigidity was well marked. The *fat* was nearly an inch and a half thick under the abdominal integuments. *Muscles* of a remarkably good colour. The *heart* was rather large and flabby; a small quantity of limpid fluid was found in the pericardium; the left auricle and ventricle were collapsed and empty; the right auricle and ventricle were loaded with a gelatinous coagulum, a portion of which was black and tar-like, a portion parti-coloured, and another, probably that which at the period of death was in the highest position in relation to the other portions, was colourless. The coagulum extended a considerable distance into the pulmonary artery, the greater part being colourless; it also surrounded and adhered to the valves and chordæ tendineæ, but was easily peeled off. The *lungs* were collapsed, crepitant, without the slightest sign of disease, and with rather less than the usual hypostatic congestion. The *stomach* contained about five ounces of a yellowish green, thick, ropy fluid: its mucous coat was softened, thickened, easily scraped off, and covered with a thick ropy secretion; there were large ecchymosed spots of a dirty brown colour in the subjacent textures, particularly around the pyloric extremity; and the large vessels in the sub-peritoneal surface were congested with dark blood, in some of which the blood was coagulated. The *small intestines* contained a large quantity of greenish-yellow ropy fluid; their internal coat was thickly lined with a tenacious yellow ropy fluid, with difficulty washed off; the mucous membrane thickened, softened, and easily peeled off; the subjacent tissue highly vascular, from the pylorus to the ilio-cæcal valve, more especially throughout the duodenum, where it nearly resembled a piece of scarlet cloth. Numerous enlarged glandulæ solitariæ in the course of the ileum. The *large intestines*, particularly the colon, were greatly inflated, and exhibited morbid appearances, in a minor degree, but similar to those observed in the small intestines; they were loaded with a yellow semifluid substance. The liver was somewhat friable and pale, and the blood pressed from several of the hepatic veins permeating it resembled coloured water; under the microscope, it presented a very decided morbid appearance, having far less colour than blood-proper, and the red corpuscles being irregular in shape, unequal in size, and apparently imperfect, dissolved, or otherwise damaged. The *gall-bladder* was distended with dark green bile, becoming yellow on dilution, and the ductus choledochus filled by concreted bile. *Spleen* healthy. *Kidneys* healthy, except that in one a very small quantity of an apparently purulent fluid could be pressed from the tubuli uriniferi. The *bladder* was completely collapsed. A tumour twice the size of an orange replaced one of the ovaries. The *blood* in the large vessels generally was tar-like and gelatinous.

Remarks.—I have related the circumstances of this case precisely as they occurred, believing that considerations of the deepest interest at the present moment are involved in them. In the first place, I have to explain the causes of some of the imperfections in the proceedings narrated. Called upon suddenly, in a case of the utmost severity, where no time was to be lost, occurring in a private house, I had to obtain as quickly as possible the necessary co-operation, the solution of salts, and, in the absence of any appropriate injecting apparatus, the best instrument I could contrive. My fear was that the patient would die before the operation could be performed. The salts were dissolved in distilled water, and the solution filtered; but it had to be

warmed by dilution with hot water from the cistern in the house. I was unprepared with a thermometer at the time of the first two injections, and the temperature was judged of by my own sensation. Mr. Talbot, of St. Mary's Hospital, and myself made the *post mortem* examination, without any other assistance; and, from circumstances relating to the family, we were limited to time. It was accordingly done imperfectly, at least in my view of the points which ought to be made out in such a case. If my own opportunities of investigating the remedial powers of the direct admixture of substances with the blood in this fearful malady were greater than they are, I should postpone the publication of this case until I had made a series of experiments, not only more carefully and exactly, but on a less empirical—a more systematic principle. I have adopted the other course, because I believe the facts are eminently suggestive, and calculated to guide others, possessed of better opportunities, in the conduct of this important inquiry.

Here is a case in which the patient was perfectly restored from the collapse stage of cholera in its most intense degree. Although eight pints of a saline solution, by no means of the purest character, either as respects the salts or the menstruum, had been mixed with the blood in the vessels, the patient lived three days, and no disturbance was produced by it either in the lungs, the brain, or, as far as evidence goes, in any organ or structure. The *post mortem* morbid appearances were strictly those incidental to cholera; the lungs were specimens of healthy lungs; and the intellectual faculties were perfect until the patient was moribund from the fully developed disease. Then, what an interesting tale does this case unfold of the sequel to the collapse. Whoever has had an intermittent fever will well remember the blueness of the nails, the aching of the fingers' ends, the state of the features and of the surface of the body, the miniature collapse that constantly preceded the rigor; and here we have the intense collapse of cholera apparently overcome by an antidote, and followed immediately by a rigor. This rigor is said to "usher in" various diseases, but it is almost always a secondary phenomenon, the commencement of reaction. The reaction established, we find, in the case before us, all the symptoms of the most intense gastro-enteritis, the more prominent of which had, indeed, existed from the commencement, and from their intensity had aggravated that collapse which is the primary effect of many poisons, and particularly animal poisons introduced into the blood. Even the mineral poisons, as, for instance, arsenic, in deadly doses, however introduced into the blood, produce effects very closely resembling the symptoms of cholera. We see in this particular case, also, the extraordinary intensity of the gastro-enteritis, exhausting and destroying the patient, but not until pretty clear indications had been given as to what would have been the result had it been less intense, and had life been still further prolonged; the clean tongue of the stage of rapid and sudden collapse was succeeded by a white fur, as soon as the vomiting abated; and this was very rapidly followed by the dry brown tongue of typhoid fever. To comprehend these remarkable circumstances, we must avail ourselves of the little that is known of the effects of poisons introduced into the blood. In Gaspard's experiments, recent pus injected into the blood of dogs produced abundant urine, reiterated vomiting, fœtid stools, tenesmus and other symptoms, and death in a few hours; the intestines were thickened, and the rectum and colon especially inflamed. Among the symptoms resulting from the injection of fœtid pus were great prostration, vomiting a blackish liquid, and extremely fœtid dejections; or, when the quantity was larger, frequent whitish very fœtid stools, ardent thirst, great prostration, hiccup, dyspnœa, violent palpitation, &c.; and after death the blood was very coagulable with petechiæ, &c. Animal putrilage produced bilious, gelatinous, and then bloody vomiting, gelatinous and bloody dejections, or the most violent dysentery, or black vomiting; and in small quantities, all the symptoms of malignant fevers; and, after death, the stomach, duodenum, or rectum, were inflamed. Vegetable putrilage produced the symptoms of dysentery, malaria, and low fever, and the mucous membrane of the intestines, especially of the duodenum, rectum, and some of the small intestines, were of a violet-red colour, and inflamed.

The more minute details of the experiments indicate that each distinct poison produces a specific effect in the blood and tissues, and is attended with its peculiar symptoms, among which prostration, partial or general gastro-enteritis, and the symptoms of typhoid or malignant fever, are the most common. There is sufficient analogy in the symptoms of cholera to lead to the inference that it is a poison in

the blood, producing more intense sedative effects in the blood itself, and a more violent determination to the gastro-enteric surface, than any known poison; and sufficient distinction, to lead to the inference that it is a poison *sui generis*, and totally distinct from all other poisons, whether known only by their effects, or otherwise. Our present knowledge of the physiology and pathology of the blood renders both the analogy and the distinction quite intelligible. Although in some cases of cholera the recovery from the state of collapse may be quickly followed by health, inasmuch as the poison may have expended its virulence, and the gastro-enteric mischief may be slight, yet in these cases of fearful intensity such a result is scarcely to be hoped for; the recovery from collapse is only the recovery from the early stage of the disease, and we may expect it to be followed by gastro-enteritis; or by pneumonia, or some other result of local congestion or disorganisation; or by typhoid or malignant fever, complicated with some of these local effects; and a remedy for the collapse ought not to be regarded as a remedy for the disease. The purging and vomiting, at the outset, are no doubt the effect of the disorganisation of the blood; and the intestinal canal is the outlet both of the poison and of the devitalised portions of the blood; but I conceive it would be an error in pathology to regard these symptoms, even from a very early period, in this point of view simply; they depend also upon the irritation or inflammation of the coats of the alimentary canal, set up during the elimination of the poison; hence the dejections, even from the commencement, contain a large portion of epithelium scales, and the disorganisation bears, to some extent, a proportion to the intensity of the poisoning. In Mason's case, I conceive the quantity of the poison originally received into the blood must have been very large; but for the remedy employed, it would have destroyed life in less than twelve hours by its direct effect on the blood; failing this, it produced this result by the intensity of the gastro-enteritis, and probably, in part, by the poison not having been completely eliminated. Had the quantity of the poison originally received been less, the probability is, *cæteris paribus*, the gastro-enteritis would have been less, but on recovery from the collapse we should have had a fully developed typhoid fever to contend with. On the other hand, had the quantity of the poison been less, and had it also been completely eliminated, during the progress of the collapse (I believe, a frequent result), there would have been a less complicated gastro-enteritis to deal with. As recovery often takes place very rapidly, even after collapse in a slight degree, it is probable that the gastro-enteritis set up during the elimination of the poison, or any effect produced during the collapse, is, in such cases, comparatively trifling. I have since witnessed the autopsy of a fine young woman rapidly destroyed in the stage of collapse, wherein the intestinal affection, although well marked and general, was still slight in degree; and, no other organ being materially affected, the suggestion presented itself, that could she have been carried through the collapse, her recovery would have been rapid. In the slighter cases, in which the quantity of the poison received into the blood is very small, producing diarrhœa only, the poison is probably very quickly eliminated, and the blood not being so extensively disorganised, nor so quickly inspissated, if the elimination by the bowels be checked, as it usually is, by the remedies we employ, the remaining portion of the poison is quickly destroyed or discharged by other outlets, and the collapse, with all the other consequences, are averted; and even where the quantity of the poison is larger, the greatest advantage results from arresting the purging in the early stage before the blood becomes inspissated, inasmuch as the rapid loss of the fluid parts of the blood is doubtless concerned in preventing the elimination of the poison by other organs.

We know little of the laws which regulate the action of poisons received into the blood, either upon the blood itself, or upon the whole animal economy through the blood. Nevertheless, it appears that each has its specific effect, while there are effects common to nearly all. The choleraic poison appears to act in the blood as a sedative. There is some reason to think that it arrests the ascensive metamorphoses of the albuminose, so that all the albuminate of soda of the liquor sanguinis is not normally developed, since the latter will not permeate living membrane, which the former does. That it acts by an arrest of development in the materials of the liquor sanguinis, rather than upon the blood proper, appears plausible also from this —the retention of the urea in the blood is the usual result of the kidneys failing to separate this substance; and not only does this take place in cholera, but according

to some of the best analyses, the urea accumulates, indicating the continuance of those metamorphoses in the blood by which nitrogenous compounds are converted into the urea. Again, the red corpuscles appear to remain, up to the latest moment of life, without any serious deterioration; else a saline fluid could not with its other effects so instantaneously restore the functions of the blood.

The corollary of these remarks is, that a saline fluid, mixed with the blood by injection into the veins, has the certain effect of *prolonging life* in the worst cases of the collapse stage of cholera; even where, from its intensity, the disease must necessarily prove fatal. Since our mission is, under all circumstance, to prolong life if possible, it is certainly justifiable, in otherwise hopeless cases, to perform this operation; and it even becomes questionable whether we ought to allow any such case to succumb without resorting to it. It is also a fact that a few individuals have recovered from an otherwise hopeless state of the disease after this operation. Were it resorted to more generally in cases hopeless of recovery, on the principle of prolonging life, our efforts would very soon be directed to learn the best mode of operating, as respects the quantity of fluid to be introduced, the nature of the fluid, the times at which it might be repeated, &c., with a view to obtain the utmost prolongation of life possible: and I am sanguine enough to believe that the accuracy of observation which this would induce must lead to important results in relation to the cure of the disease. The primary object, as respects this operation, is to discover the proper remedy for the collapse, taking care that the effects of such remedy shall not be incompatible with recovery from the disease, and the restoration of the patient to health. Viewing a patient in the most extreme state of collapse from cholera, it should always be borne in mind that in such a patient, until life is extinct, there does not *necessarily* exist any disorganization totally inconsistent with recovery. Finally, I have seen a man pulseless, speechless, cold, and livid, rise up in bed and converse after the injection of a saline fluid into the blood, and remain for several hours capable of declaring his wishes to those around him, or of taking his farewell of the world, of forgiving or being forgiven, or it might be of making his peace with God.—*Association Journal, Aug.* 25, 1854, *p.* 760.

154.—*On Cholera.* By Dr. Ottinger.—Dr. Ottinger says: "Epidemic cholera is to be considered as a disease, based on an alteration of the vegetative nervous system, (a morbid affection of the sympathetic nerve,) which rapidly terminates into paresis, or even paralysis of the vaso-motor nerves. In consequence of a morbidly increased exosmotic process, the serum of the blood exudes through the intestinal canal, occasioning desquamation of the intestinal epithelium. A remedy is therefore indicated, which exerts a most stimulating effect on the vaso-motoric nervous system." Valerianate of ammonia has proved itself, according to his experience, to fulfill the above indication, with an appropriate external treatment. He gave no other internal remedy to his cholera patients in the present epidemic. If the treatment was commenced before the appearance of collapse,—conditions of constitution and age not being previously unfavourable,—the result was most satisfactory in the majority of cases. Even after the pulse had been imperceptible for hours, after the face and extremities had exhibited icy coldness, &c., &c., re-action was established, and recovery ensued, without any subsequent cholera typhoid.

He administered every quarter or half-hour a tablespoonful of the following mixture:—

℞. Valerianat. ammon. ℈j.; aq. destill. ℥iij.; syrup. simp. ℥ss.

When re-action set in, the remedy was given in longer intervals. Externally he caused frictions to be made every hour, alternately with ice or Burgundy vinegar. These were invariably followed by constant rubbing with hot flannels. If, notwithstanding these means, the cutaneous warmth did not return, he ordered a hot-bath (from 23° to 33° Reaum.), with an addition of an ounce or an ounce and a-half of caustic potash.

As a dietetic beverage, he gave the patients ice-water, Seltzer-water, or claret, besides ice-pills. The undoubted success of this treatment in six cases (four of which were decided cases of asphyxiated cholera), encouraged him to submit his method of treatment to the impartial judgment of his professional brethren.

A week later he adds that he had seen the success of his treatment in six further

severe cases. In some the pulse had been imperceptible from twelve to fifteen hours, with corresponding unfavourable symptoms, so that he does not hesitate to recommend the remedy strongly.

The rational treatment of cholera must consist in adopting such measures as will rouse the sunken innervation of the ganglionic nervous system, and prevent its deeper prostration. Only thus can we hope to check the rapid progress towards dissolution. As soon as the innervation has become normal, the morbid exosmotic tendency of the blood ceases, and the abdominal secreting organs resume their ordinary functions, together with the peripheric terminations of the capillary system. If this salutary re-action can be obtained without any morbus medicamentarius (as unwelcome salivation, or artificially produced cholera-typhoid), he does not see why a remedy should not be employed which promises to be beneficial, without such unpleasant consequences.

The valerianic acid exercises a directly stimulating action on the abdominal ganglia, ammonia on the nervous centres and the whole organism. Thus, a combination of both—valerianate of ammonia—offers one of the most diffusible stimulants, which rapidly enters the circulating fluid, to exhibit its volatile effect. In awakening the prostrate nervous power, it corrects, indirectly, the morbidly altered secretions and excretions, and the abnormal blood-krasis. The success of the remedy depends on its methodic employment. It must be administered as early as possible, and without any other pharmaceutical agent. From 3 to 6 scruples were invariably sufficient to subdue the disease.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 622.

155.—ON THE PATHOLOGY OF SACCHARINE ASSIMILATION.

By Dr. George D. Gibb, &c.

[This is the abstract of a paper read before the Medical Society of London.]

Before entering upon the pathology, the author gave a brief summary of the physiology of the subject to the present time, and referred to the discoveries of Bernard, Pavy, and himself. He gave a table of the pathological conditions of saccharine assimilation, and considered some of the more important. He considered the special saccharine function in diabetes to be paralyzed, and believed no sugar was secreted in the liver in this disease, and in consequence the converting action of the stomach became deranged, permitting of the formation of large quantities of sugar in that organ, secondary to the arrest of the saccharine function in the liver. This view was opposed to that of Bernard, who looks upon the liver as secreting an excess of sugar in diabetes. The principal evidence the author brought forward in support of his own view was the invariable absence of sugar in the liver after death, and the "starved" state of the organ, from its poorness in fat. In fatty liver only did he consider the saccharine function abnormally active. The gorged condition of the portal venous system, found after death, he looked upon as due to hepatic obstruction, from paralysis of the special function of the liver, from taking too much gastric sugar to that organ, which it could not assimilate. If thoracic disease was a frequent termination of diabetes, he believed it was from the want of naturally-formed hepatic sugar to undergo combustion, and not from excessive combustion, as maintained by Bernard. He admitted that diabetes and allied affections were under the governance of the nervous system. He considered an occasional connexion existed between oxalic acid and sugar. Sugar, he said, was one of the great sources of fat in the economy, and he denied that fat was ever re-converted into sugar; there was no evidence to prove it. He confirmed the observations of Dr. Goolden in relation to the sympathy existing between the nervous affections and saccharine assimilation.

[Dr. Gibb chiefly depended on the copper tests, together with the fermentation test by yeast. His copper tests were Trommer's and Barreswil's solution.]

Dr. Garrod said, that with regard to the tests made use of, he thought that the fermentation test, when carefully employed, was the one which gave direct, and the most satisfactory, evidence of the presence of sugar; but it required, for its indications to become manifest, a considerable amount of that principle to be present—two grains to the ounce often failing to give an indication. Dr. Garrod felt assured from experience, that, by the different copper tests, many errors were frequently made;

and, in fact, sugar may often be present, and not detected, and again absent, and the indication produced by other bodies. In water, the copper test was one of great delicacy; but by no means so in urine. Dr. Garrod considered that sugar, in small quantities, too minute for the fermentation test, might be most readily detected in urine by treating that fluid with excess of trisacetate of lead, filtering, removing the lead with bicarbonate of soda or potash, and again filtering, by which means a colourless urine was produced, giving, on the addition of a few drops of a concentrated solution of potash, and heat, a bright orange colour, even if not more than a quarter or half a grain of sugar were contained in the ounce, creatine, alkaloids, &c., which might be present, not interfering with this test as with the copper one. By the above modification of Moore's test, Dr. Garrod had been able to detect a trace of sugar, a few hours after meals, in perfectly healthy subjects; whereas that passed at the time most removed from the influence of diet, gave no indication. With regard to the point insisted on by Dr. Gibb, that the liver of diabetic patients after death gave no sugar, Dr. Garrod was able most positively to deny its correctness, having, in one case, prepared a considerable specimen from that organ, and in another found its presence. That it might be absent in some cases, when the urine before death had ceased to show the presence of sugar, he would not deny. With regard to the connexion between oxaluria and glucosuria, although fully prepared to acknowledge the occasional presence of sugar in urine containing oxalate of lime, yet he had arrived at the conclusion, after paying considerable attention to the subject, that oxalic acid in the animal body is not produced, as Dr. Prout considered, from the amylaceous or saccharine elements, but was a product of the decomposition of uric acid. Amongst other reasons for this opinion, he mentioned the frequent occurrence of mixed urates and oxalates in the urine, the frequent alternations from one to the other in the deposits, the constant occurrence of calculi in which urates or uric acid alternated in the layers with oxalate of lime, the ready production of oxalic acid from the artificial oxidation of uric acid, as by the peroxide of lead, and the probable production of this same acid in the spontaneous decomposition of uric acid in albuminous fluids. Dr. Garrod did not believe that a force capable of breaking up *sugar* into oxalic acid existed in the human body, either in health or disease; whereas a force equal to the disintegration of *uric acid*, and thus forming oxalic acid, was known to be present.

[The following is from Dr. GIBB's papers in 'The Lancet:']

Before entering into the pathology, it may be permitted me to say a few words on the physiology of saccharine assimilation.

The *liver* has been shown to be the fountain-head for the formation of sugar, and in health contains a certain quantity of that substance; where it is formed from the blood brought to this organ by the portal vein, and afterwards is carried by the hepatic veins and vena cava inferior to the right side of the heart, whence it is conveyed by the pulmonary arteries to the lungs, and there undergoes the process of combustion. This fact clearly proves an important connection between the saccharine element and respiration, which time will not permit me to go into; but the experiments of my friend Dr. Pavy show that the presence of fibrine in the blood is essential to the decomposition of the sugar. The formation of sugar in the liver is a process totally independent of saccharine or feculent food, and commences before the birth of the individual, as has been demonstrated by Bernard. The quantity of sugar, however, varies in different classes and tribes of animals, as I have satisfactorily proved on many occasions, being much greater in those whose livers naturally contain much fat, of which the seal and the porpoise may be cited as examples, and also in ducks and geese, and most of the sea-birds. The liver of the cod and other fishes which abound in fat also contain it in large quantity. This fact also, first noticed by myself, shows the strong relationship which fat bears to the presence of sugar, and helps very much towards the elucidation of the pathology of fatty degeneration; in fact, its importance cannot be over-estimated.

Sugar also exists in the serum of healthy arterial blood, in minute quantity, and also in different parts of the venous system, but varying in quantity; thus, Dr. Pavy has found sugar in the jugular veins; it exists in the vena porta but in small quantity, being, however, richer in the former than in the latter. The chyle of the thoracic duct contains a little sugar, derived from the lymphatics of the liver, the contents of which acquire a saccharine impregnation, from the saturated condition of

the tissue of this organ. The chyle going from the alimentary canal does not contain it. Sugar is an important ingredient in the well-known secretion of the mammary gland, and is familiarly known as sugar of milk; it is present also in the milk secreted in the breast of the newly-born babe, whether male or female. Sugar is also found in the urine of pregnancy, and can be most clearly demonstrated during the process of gestation; and lastly, sugar is found a normal ingredient in the urine of the aged.

Such, then, are the natural or normal states in which sugar exists in the animal economy, and which may conveniently be comprised under the term *Physiology of Saccharine Assimilation.* This classification will permit of our comprehending the states in which the presence of sugar in the fluids and solids of the body constitutes disease, in contradistinction to those which we denominate healthy.

The pathological conditions form a very considerable class, and comprise some of the most incurable diseases to which the human frame is liable. These conditions may be regarded, perhaps, merely as symptoms; but as they indicate constitutional derangements of great importance, we may, I think, most justly consider them under the head of the 'Pathology of Saccharine Assimilation.'

Diabetes.—That eminent philosopher, Dr. Prout, has satisfactorily established that in this disease the primary assimilating organs are unable to assimilate the saccharine principle. In health the *reducing* function of the stomach, on all the forms of the saccharine principle, is accompanied by the development of a *low* sugar, which is speedily *converted* into albuminous and oleaginous matters. The existence of sugar therefore in the stomach is only momentary. Now, in diabetes the *reducing* function of the stomach is morbidly active, while the *converting* function is more or less suspended or paralyzed. It follows therefore that in the diabetic stomach sugar is found in large quantity, particularly when vegetable aliments have been taken; and from the stomach the sugar readily passes into the sanguiferous system. Dr. Prout correctly observes, therefore, that diabetes does not consist, as some have supposed, *in the development of sugar in the stomach, which is a natural process, but in the greater or less destruction of the converting, and consequently of the still more important organizing, functions of the assimilating organs.*

I think it will be admitted that this explanation of one of the first steps in the derangements of this disease is most clearly the truth, and quite incontrovertible. The discovery of sugar in the liver, subsequent to the enunciation of Prout's doctrine, so far as I have investigated the matter, does not appear in any way either to interfere with it or in the slightest degree to set it aside. He has acknowledged, however, that sugar does appear as the result of secondary assimilation in advanced stages of diabetes, but that it never appears in the healthy state of the system during the secondary assimilating processes. The latter part of this assertion is now disproved by Bernard's discoveries. From the very large quantity of sugar thus generated in the stomach, it is a matter for little surprise that the blood should be thoroughly impregnated with it, notwithstanding that this has been denied by some; but the fact is now perfectly established. The blood takes up a large proportion of the sugar, but not all, as the alvine discharges are found to be mixed up with perhaps the more solid and crystallizable portions of it, which may be distinctly seen after the lapse of a few hours. The various emunctories of the body assist in relieving the blood of its surcharged saccharine principle; and hence by the kidneys the urine in large quantities is passed, containing much of the sugar; the saliva contains it; the expectoration, if accompanied with, or independent of, phthisis, is mixed up with it; and, lastly, the perspiration, if there is any, which is but seldom, has been proved to contain it by Simon, Nasse, Magendie, and others, and becomes as much impregnated with it as any other secretion in the body.

It is a matter of great difficulty to obtain a diabetic liver *immediately* after death, but those which I have examined some hours after death did not contain any sugar whatever. At first I doubted the correctness of my experiments. On considering, however, that by the same means I had detected sugar over and over again in other livers, and the fact of being familiar with the means of recognising sugar, my mind became convinced that the *liver* in diabetes is either free from sugar altogether, or deficient in quantity. This was a most important discovery, and quite opposed to the views entertained by Bernard. Could this be explained by the derangement of

the secondary assimilating processes, as a consequence of the portal vein taking so much sugar to the liver already formed, instead of permitting of the process of assimilating it naturally from the blood, during its circulation through the hepatic structure? This was evidently one of the causes. This I thought also in some measure might explain, possibly, why this affection so often terminates in thoracic disease, not from its taking too much sugar for destruction in the lungs, but from its taking too little directly from the liver itself, the liver not having the power, unless in a slight degree, of converting the gastric sugar into that form peculiar to itself, and also into fat.

It may be mentioned here, that I am not unmindful of the experiments of the injection of grape and cane sugar into the general circulation through the jugular veins, and their appearance in the urine; nor of those in which the same sugars have been injected into the portal system, and, undergoing the necessary modifications in the liver, have not been detected in the urine. They clearly point to the natural function of the liver in health, in exclusively absorbing the saccharine principle from without. But when this principle becomes excessive, in such a disease as diabetes, the peculiar function of the liver is more or less paralyzed.

My friend, Professor Beale, of King's College, who has chemically examined several cases of the kidney and liver in diabetes, tells me he could not find a trace of sugar in the liver; and in a valuable paper in the 'Medico-Chirurgical Review' on this subject, he mentions that the liver contained a less quantity of fat than in health —"It appeared to be as it were starved, and its secreting cells seem to manifest a tendency opposite to that of fatty degeneration." My own view is thus confirmed by Dr. Beale, and is quite opposed to that of Bernard in so far as the liver is concerned. He believes that diabetes depends upon the excessive and abnormal production of sugar in the liver (dependent upon an unusual excitation of the functions of the great sympathetic system of nerves), whereas the contrary is the fact. If such were the case, we ought to have diabetes as a consequence of fatty disease of the liver, which I have shown elsewhere to contain sugar in the quantity mentioned as occurring in the diabetic liver by Bernard; but with the exception of impregnating the fluid of co-existing dropsies, we do not find it in the urine, and certainly there is no diabetes.

I am disposed, therefore, for the present to consider diabetes as the result of the cessation of the natural function of the secretion of sugar in the structure of the liver, and the consequent derangement of the primary assimilating processes. The primary assimilating organs are thus influenced in some peculiar way, possibly by an abnormal condition of the bile, secreted from an unsaccharine liver, or possibly by an extension of the nervous influence from the liver, sympathetically affecting the stomach and bowels, and also by the powerful reaction produced by the pancreatic fluid, which contains much fat, as the secreting cells of the pancreas have been found gorged with fat in this disease by Dr. Hyde Salter. The pancreatic fluid in health is the real agent in converting starch into sugar, which is slowly and gradually effected, the sugar being absorbed as it is produced. In diabetes this function may be greatly increased, which the fatty condition of this organ would tend to prove.

These, singly or combined, influence the *converting* function of the stomach, and subsequently secondary assimilation.

Without disputing the correctness of the view entertained by Dr. Bence Jones, that there is a stoppage of the healthy changes in the food during digestion, and which occurs at the sugar,—the starch having become converted into dextrin, and the dextrin into sugar—the conversion of which into vegetable acid and carbonic acid, he states, does not take place; this view being that of the effects of the disease, my theory is strengthened by the post-mortem appearances, to some extent, which have been noticed by various observers, as well as by myself, meagre though they are. In the numerous examinations made by Prout particularly, he found organic disease of many of the viscera, most generally the lungs, in different individuals; while in others all these organs were observed to be apparently sound. He considered, from the diversity in the organs affected and the occasional absence of *all* organic disease, that the presence of sugar in the system does not depend upon, nor produce visible organic lesion of, any particular organ; but that when organic diseases do exist,

they are merely concurrent affections. Functionally speaking, he was induced to believe, from very long attention, that the liver is always deeply involved in diabetes. This is an admission of great importance from such an accurate and experienced observer, and is entitled to great weight, more especially since the discovery of its sugar-secreting function.

The most constant diseased appearances noticed by Prout after death have been, he says, rather of a chemico-mechanical nature, than strictly organic. These he comprised under three heads:—First, an enlarged and congested state of the kidneys; secondly, a gorged condition of the veins terminating in the portal system, particularly in the veins of the mesentery, and an unusually dark-coloured and fluid condition of the venous blood throughout the assimilating organs; thirdly, but not so constantly, a vascular state of the mucous membrane of the stomach and upper portion of the alimentary canal.

The first and third are what might be expected, from the nature of the disease; but the second clearly points to hepatic obstruction in the circulation of the blood through the liver, and most probably arising from the quantity of sugar being carried in the blood to that organ, thus helping to paralyze its proper function.

The evidence furnished to prove that the liver is the great organ at fault in diabetes, subject to the influence of a centre such as the medulla oblongata, is, I think, of much value, and it is a source of satisfaction in my being enabled to state, that if I differ with such an observer as Bernard, it is not as to the seat of the disease, but in the nature of the special function implicated, which, at the same time, however, is the principal key-stone, if allowed the expression, of the disease.

It might be urged, against the view I have taken of diabetes, that just before death, the urine ceases to contain sugar, as I have myself witnessed, and that the liver might equally not contain it. Now, this is met by the fact, that sugar in diabetic urine or blood can be detected for any length of time, and so can it in a liver containing it in excess, as the fatty liver, a specimen of which I found to contain a trace of sugar eleven days after removal, although in a state of commencing decomposition. At this moment, some diabetic urine which I obtained from a patient at St. Bartholomew's Hospital, under the care of Dr. Burrows, on December 8th, 1853, contains sugar. This peculiar property of diabetic sugar was first mentioned to me by my friend Dr. Pavy, and is not possessed by that artificially produced through experiment on a living body. Moreover, if sugar existed in such quantity in the liver as Bernard believes, it ought to be detected with perfect facility even a few hours after death; but such is not the case—it is absent. Since the foregoing was written, at a recent discussion upon this paper, Dr. Pavy mentioned that neither Bernard nor himself had found sugar in the diabetic liver after death, which is a point of some importance to establish; I am prepared to believe, however, in the existence of an occasional and perhaps rare exception to this, as has occurred in the experience of Dr. Garrod.

One of the uses of healthy assimilated sugar in the liver, independent of its combustion in the lungs, is the formation of fat, and at a later day, I have no doubt that the relationship existing between fat and sugar, which has been prominently brought forward by me elsewhere, will yet be proved and established. It is one constantly presenting itself in experiments connected with sugar, and latterly I have come to the conclusion that it is one of the great sources of the fat in various parts of the body; and further, that sugar is convertible into fat, and fat is not convertible into sugar. This, I may mention, is contrary to a view I had formerly held, in common with others, but which I now hasten to correct. The only argument in favour of the conversion of fat into sugar is the action of pure and recently-formed pancreatic juice, which Bernard says emulsionates fats or oils with the greatest facility; the emulsion remains for a long time, and the fatty bodies soon undergo a fermentation, which separates the acids they contain.

There are some other points of interest, in relation to fat and sugar in diabetes, which help further to prove certain points in its pathology. In a paper by Dr. Beale, in the British and Foreign Medico-Chirurgical Review, on the Chemical Composition and Microscopical Characters of the Liver and Kidneys in Diabetes, especially with reference to the amount of fat present, he found that the kidneys contained a much greater proportion of fatty matter than is present in health, and the liver a lesser quantity than in health. In the healthy kidney, the amount of fat is not quite 4%,

whilst in diabetes it is occasionally upwards of 25%. In a given quantity of diabetic kidney, he found the fat exceeds that present in the same quantity of diabetic liver, while in a state of health the *converse* is the case; and on comparing the analyses of two of his cases, it is found that the *healthy* liver contains *more than twice the quantity* present of fatty matter than in the diabetic organ. What is proved by these facts of Dr. Beale's? The circulation of saccharine fluid through the kidneys, depositing fat, as a strictly mechanico-chemical result; and a want of the same fluid, healthily secreted, to circulate through the liver, and hence a notable absence of fat! The pancreas may have much saccharine matter circulating through its structure, from the fact of its secreting cells being gorged with oil globules, as shown by Dr. Hyde Salter.

The sugar-secreting function of the liver, then, is arrested in diabetes; and as sugar is necessary for the purposes of the economy, it is morbidly secreted elsewhere, in the stomach especially, from all kinds of food, in which organ the converting process is in abeyance. Our attention, therefore, in the treatment, must be directed towards removing the condition of the nervous system acting upon the liver, so as to restore its functions, and at the same time the converting the function of the stomach, which had become stopped, secondarily to the cessation of the formation of sugar in the liver.

In relation to nervous derangement, being the first step in the disease, and which reflects so much credit upon the sagacity of Bernard in clearly establishing, I believe the special governing centre to be the medulla oblongata, through its important branches and great sympathetic communications. Bernard considers these phenomena in relation to saccharine urine and diabetes, due to increased activity of the function of the liver, dependent upon an unusual excitation of the functions of the great sympathetic system of nerves. He has proved by experiment that the pneumogastrics act wholly as incident nerves in reference to the saccharine function of the liver. Thus, when they are divided in the neck, to produce artificial diabetes, the current of galvanism must be passed upwards through their central extremities, and not downwards through their peripheral. "The galvanism, acting on the medulla oblongata through the upper extremities of the pneumogastrics, produces an influence which, like that derived from the puncture of the nervous centre itself, is transmitted downwards along the spinal cord, and thence by the splanchnics to the hepatic organ."

In terminating this branch of my paper, it may be mentioned that Drs. Bright, Copland, and Watt, of Nottingham, looked upon diabetes as depending upon derangement of the nervous system, but they did not agree as to what part of it. Dr. Copland fixes the seat of it in the great sympathetic system, and instances are recorded where the sympathetic, pneumogastric, and splanchnic nerves were found very much enlarged in diabetes.

Tuberculosis.—In the fatty liver sugar exists in larger quantity than in health. If the condition of the liver was only found in conjunction with pulmonary affections, we might, indeed, explain the manner in which it arises on physiological grounds, from diminished oxygenation and combustion of carbon due to impeded pulmonary circulation and respiration, a consequence being the accumulation of those elements in the liver which could not be got rid of in the lungs. To some extent, perhaps, this might apply to phthisis; but how can we explain the presence of fatty liver in other diseases, where the lungs are apparently quite sound? Further research and investigation are necessary, before a satisfactory solution can be attempted. We know, from the researches of physiologists, the important bearing sugar possesses in relation to respiration, and the error that some have fallen into in regard to the peculiar saccharine function the liver possesses, being over-exerted, so to speak, in diabetes, which, however, is not proved, and which, as I have endeavoured to show, is actually in abeyance. The knowledge of the fact, that fatty liver contains an excess of sugar, will be the means of helping to work out this problem, and its relation to tuburculosis. This leads me on to the consideration of scrofulous matter, and its connection with sugar.

In Jan. 1850, I opened a large abscess, situated on the back, below the right scapula, in a female aged 23, the subject of general external scrofula. The fluid withdrawn was of a yellowish colour, specific gravity 1028, inodorous, neutral, and of a creamy consistence. In the course of chemical examination, I applied the different

reagents for testing the presence of *sugar*, when I found that Moore's test and Trommer's test gave positive proof of the presence of a considerable quantity of that substance. Microscopical observation showed the usual characters of tuberculous matter, in the presence of cells filled with granular matter, free granules and fat globules, together with pus and lymph corpuscles. In February, this large abscess, having become again filled, was opened, and exit given to a thick, cream-like fluid, of a dark drab colour, mixed with cheesy particles of the same colour. On examination for sugar, the results were again positive, and the microscope showed a large number of pus corpuscles and scrofulous matter; no blood discs were to be seen.

From these experiments, the suggestion naturally arose, of a possible connection between sugar and tubercle, and which seemed of sufficient importance to investigate by the most careful research.

The first opportunity was seized to obtain tubercles in a crude and softened state from the lungs of a phthisical patient, who died, aged forty. Those from the left lung were examined: the crude tubercles were treated in the same way as for examination of the liver, whilst the softened tubercles were boiled, filtered, and then examined. A very small amount of sugar was detected, particularly so in the crude form; but the results were not conclusive, by any means, of the relationship supposed. The case was one especially suited, too, from there being a large fatty liver, which contained a quantity of sugar, as usual. Very recently, through the kindness of my friend Mr. Vertue Edwards, the resident medical officer at the Hospital for Consumption at Brompton, I was enabled to obtain from the body of a female, aged 23, who had succumbed from phthisis, portions of tuberculous lungs, enlarged and tuberculous bronchial glands, the glands of the mesentery in the same condition, and some fatty liver. All these were submitted to a series of the most carefully executed experiments for detecting the presence of sugar—some of them several times; but with the exception of the fatty liver, which, as usual, is found to contain a large quantity of sugar, I did not obtain the least evidence of its presence in the tubercles, either in the glands or lungs. Moore's test, only, indicated the presence of this substance; singly, however, it cannot be relied upon.

Further experiments are still necessary, on other specimens of tubercle, more especially if in a state of softening.

Magendie has stated, there are only two diseases known in which the quantity of sugar in the economy has been proved to be increased—*phthisis* and *glucosuria*. It had long been observed, that the urine, as well as the expectoration, of phthisical patients, is sometimes saccharine; but a distinction, he says, must be made between such cases and those which only become phthisical as a result of diabetes. Bernard declares that the urine, in true phthisis, is not saccharine; but that the quantity found in the blood is much increased. Having observed such increase in the blood of a young girl who had been bled, and whose urine exhibited no sugar, he predicted, although no other sign of phthisis was present, that she would fall a victim to this disease, which she did.

Reynoso, again, has detected sugar in the urine of patients affected with tubercle, the sugar being more abundant in those cases in which the malady was in its most advanced stages, and when the inflammatory symptoms were most intense.

In the early stages of incipient phthisis, I have no doubt sugar will be occasionally found in urine. I am not prepared to offer any examples in proof of this; but a case given by Dr. Goolden, in his paper in 'The Lancet' of July last, is one well adapted to direct attention to the subject. The patient was a girl, aged 13 years, suffering from constant headache and pains in various parts of the body; in fact, general neuralgia, but the subject of incipient phthisis, and probably the brain suffering from strumous congestion, as Dr. Goolden very correctly observed. Here the urine was found to contain an abundance of sugar, which subsequently disappeared under treatment.

Dr. Venables has also observed, that saccharine urine is a very common affection amongst the scrofulous children of the poor. I feel persuaded its absence will, by and by, be found the exception, in cases of strumous congestion of the brain, and in hydrocephalus.

I have learnt with much gratification, that it is contemplated, at the Hospital for Consumption at Brompton, to examine the urine of all the phthisical patients in relation to the question of sugar in the urine. Mr. Bird, one of the resident clinical

pupils at that hospital, tells me he has lately found sugar in the expectoration of a phthisical patient.

All these facts, in relation to tubercle and diabetic sugar and their connection with one another, possess features of unusual interest to the anxious inquirer after truth, and, collectively, will prove of service in helping to work out further the pathology of diabetes and allied affections.

It may not be out of place here, to speak of secondary phthisis as a result of diabetes. This is almost one of the invariable terminations of this disease: but it is not constant, as some erroneously suppose. I am disposed to consider the phthisical termination more as the result of the want of sugar for consumption in the lungs, obtained through the ordinary and regular channel of the liver; and hence the cessation of a very important function of the lungs, with a deposit of tubercle. This view is opposed to that of Bernard, who attributes the most serious pulmonary affections to the fatigue experienced by the lungs, in their efforts to decompose the large quantity of sugar which he says is formed in the liver, but which, as I have before stated, is not formed in that organ in true diabetes, one of the special points in its pathology, as I believe, being an arrest of its true sugar secreting function.

Abscesses.—The experiments, already mentioned, in relation to the presence of sugar in scrofulous pus, were not sufficient in themselves to prove that other kinds of pus necessarily contained sugar; but to test the subject further, pus from variously-situated abscesses, and from sinuses, &c., secreting matter, was examined with satisfactory proof of saccharine impregnation; thus showing that sugar was a natural ingredient of pus, no matter what its colour, nature, consistence, or situation.

To mention a few examples: the pus from a large mammary abscess, which was healthy, laudable pus, contained sugar in small quantity. Pus from a bubo contained sugar. Very fœtid pus from the sac of an abscess over the right malar bone in a girl, contained sugar. A chronic fistula in the left breast of a female, in which blue pus was secreted, containing the cyanuret of iron, contained sugar. Common abscesses, wherever situated, when examined, were found to contain sugar.

The fact was thus positively proved, that sugar is one of the normal constituents (so to speak) of pus, and it is to its presence that the sweetish taste is due.

Dr. Mason Good, in the second volume of the 'Study of Medicine,' in describing pus, says, "It has a sweetish, mawkish taste, (apparently from its containing sugar,) very different from that of most other secretions. He appears to have been the first author who has *supposed* its presence in this fluid.

Its presence may possibly be due either to the albumen found in pus, which, according to Dr. Wright, contains from 58 to 83 per cent., or to a property possessed by a pyogenetic membrane of secreting it with the components of the pus itself. In a letter addressed to the Academy of Science by M. Pelouze, on 'Various Facts in Chemistry applied to Physiology,' he states that there is sugar in the albumen of the fowl's egg. Pus also contains fatty matters, which may likewise account for its presence. But whether the sugar is formed by the decomposition of the albumen, is secreted from the pyogenetic membrane, or is due to the presence solely of either fat or albumen, I am not prepared to say. The presence, however, of either of the latter, both being proximate principles, is a sufficient proof of its elimination from the body.

I have not, myself, examined pus from diabetic abscesses, but I find that a patient with diabetes, under the care of Dr. Fricke, (U. S.?) suffered much from abscesses on the shoulder, and backs of his hands. The pus from these always contained sugar.

Diseases of the Nervous System.—From the researches that have already been made into the physiology of saccharine assimilation, more especially in connexion with experiments on the nervous system, we find that a sympathy of no ordinary character exists between the base of the brain and the liver. To some extent this is already borne out by the occurrence of not merely saccharine urine in many diseases of the nervous system, but true diabetes has been a concomitant affection of many of them. When it was discovered that the liver contained sugar, and that its secretion depended upon the integrity of the pneumogastric nerves, it naturally led to the inference that nervous influence exerted a governing power on the chemistry of that organ, which was further confirmed by the experiment of puncturing the floor of the fourth ventricle, which produces artificial diabetes. But this experiment is incomplete until we know whether the secretion of sugar in the liver itself is sus-

pended or increased after its performance. If absent, which I believe it to be, then is the theory which I have ventured to propound of the pathology of diabetes further confirmed. In a careful examination of Bernard's writings, and those of Dr. Pavy, I do not find this point attended to. Very much credit is due to Dr. Goolden, of St. Thomas's Hospital, for the application of these views to the pathology of diabetes in connection with, or in relation to, brain affections, and his observations, as far as they have gone, are entitled to some weight.

These nervous diseases shall now be briefly glanced at:—*Epilepsy.*—The researches of MM. Michea and Alvaro Reynoso were the first to show the presence of sugar in the urine of epileptics after an attack of convulsions: this fact they distinctly proved by a number of experiments, and it has been confirmed by other writers. In my own experience, I have most clearly demonstrated its presence on several occasions, both immediately and for some little time after the attack; and I have, on one occasion, noticed its presence when an attack was threatened, but which passed off. In Dr. Todd's "Clinical Lectures on Paralysis and other Affections of the Nervous System," is a case of epileptic hemiplegia (the 57th), where epileptic attacks immediately followed the cessation of frequent periodical attacks of gout, and which left a state of temporary paralysis of the left side; the urine here was found to contain a small quantity of sugar. In an interesting case embodied in Dr. Goolden's paper in 'The Lancet' of July last, the urine was of the specific gravity of 1022, very turbid, with an excess of lithate of ammonia, acid, and containing a very decided quantity of sugar, which continued for several days: there was a slight tendency to chorea in this case. In the cases I have examined, the specific gravity of the urine has ranged from 1018 to 1025; there was nothing unusual in the quantity secreted, and I have not found its presence beyond two or three days at farthest.

In *Chorea, Paralysis, Neuralgia, Dentition, Concussion of the Brain,* and probably many other nervous affections, sugar will occasionally be found in the urine, and, in some instances, diabetes has been a consequence of the affection, if from an injury. Some of the cases briefly described by Dr. Goolden appear to owe their origin to injuries of the brain; and as these were cured or removed, the diabetes disappeared. A case of diabetes was cured by the supervention of an attack of paralysis. He has found sugar in the urine in a large number of cases of epilepsy, paralysis, and chorea, which has disappeared as the symptoms are relieved by treatment. He states, that severe cases of neuralgia, especially tic douloureux and sciatica are often, but not universally, attended with saccharine urine, which disappears when the nervous affection is relieved; and he gives a good case of general neuralgia in illustration; and also mentions another of facial neuralgia under Dr. Dundas Thomson's care. He likewise speaks of sugar being very common in the urine of children who suffer much from teething.

I am enabled to confirm in a great measure the observations of Dr. Goolden; for I have on several occasions found sugar in the urine of individuals labouring under nervous diseases. It is not at all an uncommon thing to find it in the urine of children suffering from dentition, and very recently I have found that it is occasionally present in the urine of hooping-cough, both in its simple and complicated forms; of the latter I may mention epilepsy, bronchitis, and serious brain disease. Reynoso has found it in the urine of hysteria.

Affections of the Base of the Brain, such as tumours, chronic disease, wounds of the fourth ventricle, injury or division of the pneumogastric nerves, will be accompanied or followed by the presence of saccharine urine. The experiments of Bernard are too well known to require a description; but I would refer to the paper of my friend Dr. Pavy, in the "Guy's Hospital Reports," for several additional and very striking experiments in puncturing the floor of the fourth ventricle in rabbits, and which reflect great credit upon him for the care and patience with which they were so successfully performed. Tumours pressing upon this part of the brain, or disease implicating it, or its immediate vicinity, will produce saccharine urine, I believe, as satisfactorily as if an experiment was performed artificially. The complete division of the pneumogastric nerves arrests the special saccharine function of the liver, as no sugar is found either in the liver itself, or blood going from that organ to the heart and lungs, after its performance. It is found, however, in the urine. A partial division, compression, picking, or injury of the pneumogastrics, will most probably lessen or decrease the amount of sugar in the liver, and produce it in the urine.

Their irritation produced by the influence of galvanic currents will equally render the urine saccharine; and it is a curious fact, mentioned by Magendie, that in a recent autopsy of diabetes two dark spots were observed at the place where the part in the fourth ventricle must be penetrated to produce the sugar.

It will appear, then, that some most important connexion exists between the nervous system and the special function of saccharine secretion possessed by the liver, which is clearly proved by numerous experiments, some of which are referred to in a previous part of my paper. It further appears, as shown by Bernard, that if the line of communication between the liver and medulla oblongata is destroyed, either by the division of the splanchnic nerves or by section of the spinal cord as high in the canal as is consistent with the life of the animal, that irritation of the pneumogastrics, by puncture or galvanism, is no longer capable of "stimulating the secretive function of the liver to produce a super-saturation of the system with saccharine matter."—(Dr. Pavy's paper.)

I shall not make any remarks on the latter part of Bernard's view, but shall content myself with observing that the nervous system has under its control the saccharine function of the liver, which seems to be influenced one way or the other by not a very great amount of nervous derangement, and it is a condition which should never be lost sight of in the treatment of any affection accompanied not necessarily with diabetes, but with a saccharine state of the urine.

Diseases of the Respiratory System.—Some observers, and especially M. Alvaro Reynoso, maintain that any cause producing an impediment to healthy respiration, or in any way impairing that function, will produce the presence of sugar in the urine. The theory they give in explanation is, that the combustion of the sugar is arrested in the lungs, in consequence of the defective action of these organs; it passes into the systemic circulation, and is excreted by the kidneys. To prove the truth of this proposition, M. Reynoso brings forward instances in favour of his view. He has detected sugar in the urine of persons subjected to etherization; in that of rabbits suffocated or drowned; in that of living animals prevented from breathing freely; in that of persons affected with tuberculosis, pleurisy, asthma, and chronic bronchitis. In addition to these, Dr. Garrod has reported a case, in the fifth volume of the "Transactions" of the Pathological Society of acute bronchitis, with the urine saccharine. Dr. Beale found a considerable quantity of sugar in the prune-juice expectoration of a patient suffering from pneumonia, secreted shortly before death. I myself have found it in the urine of simple pertussis, as well as when complicated with bronchitis or other chest disease. And Dr. Bence Jones has found sugar in the urine of a woman under the care of Mr. Cæsar Hawkins, who had been kept for twenty-four hours and more under the influence of chloroform; this has been subsequently confirmed in some instances by Dr. Beale.

Magendie states that it has long been observed that the urine as well as the expectoration of phthisical patients is sometimes saccharine. Bernard again declares that the urine in true phthisis is not saccharine, but that the quantity found in the blood is much increased.

The question is of some importance as to the origin of sugar in the urine in impeded respiration, and deserves careful study. To the opinion of Reynoso I am disposed to attach some weight, in regard to this condition of respiration being the cause of the sugar in the urine; but I cannot altogether agree with Bernard, that there is an increased activity in the sugar-secreting organ with the formation of a quantity of sugar in excess above that for healthy combustion in the lungs; and hence its elimination through other channels. The impeded respiration, however, may act in a two-fold manner—first, in not completely consuming the sugar going to the lungs from the liver, thus passing into the systemic circulation, as Reynoso supposes; and, secondly, in exciting or irritating the pneumogastric nerves, which would, more or less, interfere with the due amount of saccharine matter being secreted in the liver, and thus permit of its generation elsewhere. One of the so-called weighty reasons advanced by Bernard against Reynoso's theory is, that after division of the vagi, although respiration is greatly embarrassed, no sugar appears in the urine; that great physiologist, however, appears to have forgotten that when these are divided the liver ceases to secrete sugar, and respiration becomes embarrassed from the same cause as interferes with this function of the liver; this, therefore, is not a refutation of the important part of Reynoso's theory. Notwithstanding this, however,

we will admit that pneumogastric irritation produced by the respiratory disease or obstruction, whatever that may be, acts in the same manner of producing sugar in the urine, as experiments artificially performed.

An observation of Bouchardat, referred to by Dr. Beale, to the effect that, in two cases of slight diabetes, the urine was secreted free from sugar upon causing the patients to inhale oxygen, is said to support the theory of Reynoso. I believe the lungs to be in a debilitated condition in diabetes from the want of naturally secreted hepatic sugar. The inhalation of oxygen may act as a beneficial stimulant, and may assist, in some manner, to restore the proper function of the liver; and this being effected, even for a time, it will cause the diabetes to disappear. As remarked before, the gastric sugar circulating in the general system, cannot be compared to hepatic sugar which is more especially suitable for respiratory purposes.

Cholera.—In this disease, where there seems to be an arrest of the assimilating processes, the secondary especially, we cannot be surprised to meet with sugar in the excretions during the progress of the affection. It has not as yet been proved that the liver especially is not one of the principal organs at fault, as shown by the total arrest of the secretion of bile; without, however, going into a disputed and an unsettled question, it cannot be denied, that the functions of the liver, during the prevalence of the disease, are in abeyance, and if one of these suffer, the others by implication may do so likewise. This will account for the presence of the sugar in the evacuations, and in the perspiration.

M. Doyere was the first to find grape sugar in the perspiration of cholera patients, which was subsequently confirmed by M. Poirson, one of the house-surgeons of the Salpetrière, who found a large quantity of it in an aged woman, who died of the disease, whose skin presented a viscous secretion; a part of that examined was removed from the face.

Magendie has found large quantities of sugar in the evacuations of cholera patients. And M. Vernois mentions that he found sugar more easily in the liver in epidemic cholera than in the normal state. Can this be due to hepatic venous obstruction from impeded respiration, or to the non-secretion of bile?

Milk.—When the specific gravity of milk is taken, it may determine the richness in cream, but not the amount of sugar present. This latter condition is one of some importance, as bearing upon the pathology of milk, and deserves more attention than has been devoted to it, and so far as my own researches have extended, it appears to have been scarcely noticed, with some few exceptions.

If milk is poor in cream, it will not nourish the child, and will produce simple emaciation; it may contain, however, a sufficiency of sugar, which will compensate to some extent for the absence of the cream, as it is quite capable of conversion into fat during the processes of assimilation.

On the other hand, should there be a plentiful supply of cream, an abundance of sugar, and the external characters of the milk unexceptionable, taken with an apparently healthy condition of the mother, with the production of great emaciation, copious diaphoresis, and diuresis in the child, we must at once suspect the process of saccharine fermentation within the breast, and the development of infusoria; the microscope is here indispensable. The process is a remarkable example of perverted saccharine assimilation, produced by deranged secretion and nervous excitement.

There are many other pathological conditions of the milk due to faulty saccharine assimilation, which will be considered in a future paper. Lehmann detected sugar in the urine of a puerperal woman, in whom the secretion of milk was suddenly suspended on the fifth day after delivery, a forcible illustration of the connexion between saccharine assimilation and the secretion of milk.

Effects of Certain Medicines.—Were not the investigations of M. Reynoso deserving of much credit, from the manner in which they were conducted, in relation to his experiments on the urine, we might almost seem doubtful of the results obtained. It appears that certain medicines, whether absorbed through the lungs in vapour, or through the stomach in substance, possess the power of rendering the urine saccharine, but to be detected only by the most carefully conducted experiments. Ether and chloroform have already been mentioned, when considering diseases of the respiratory system, as occasionally producing this result. In the urine of persons under the influence of the bichloride, iodide, or sulphuret of mercury, the salts of antimony,

opium, and narcotics in general, or sulphate of quinine, Reynoso found sugar. Reynoso further states, that dogs which have been submitted to the action of arsenic, lead, or sulphate of iron, excrete sugar in the urine. He has likewise detected sugar in the urine of persons treated with carbonate of iron. Previous to reading an account of M. Reynoso's researches on the presence of sugar in the urine, I had long entertained the belief that over-doses of medicine might produce saccharine urine, but had not myself investigated the subject. One of the substances which I thought especially likely to derange saccharine assimilation was tobacco, when used to excess in smoking and chewing.

An example in the production of diabetes by an over-dose of nitrate of potass, I find recorded in the proceedings of the Academy of Sciences of France, in January, 1849, by Dr. Cardan. A man took three ounces of nitre, instead of Epsom salts, which was followed by great irritation of the intestinal mucous membrane, and abundant diuresis; the irritation subsequently disappeared, but true diabetes remained. The report does not state whether the man was cured.

I shall not venture, at present, to hazard any observations upon what has just been said, as a very wide field has yet to be explored before coming to any satisfactory conclusions; but it would almost appear that this function of the liver is affected by medicinal agents more easily than has been heretofore imagined.—*Lancet, Feb.* 3, *Mar.* 10, 24, and 31, 1855, *pp.* 156, 255, 312, 335.

156.—LITHOTOMY SIMPLIFIED.

By George Allarton, Esq. M.R.C.S., &c.

[This is the title of an interesting pamphlet in which this important operation is attempted to be improved. Mr. Allarton's operation is, in fact, a modification of the Marian, which Mr. Coulson says had been abandoned 100 years ago. Dr. Willis lately attempted to revive it, though with some modifications, in a very interesting monograph, which we noticed some time ago.]

In the *Marian Operation* a grooved staff was introduced and cut upon in the middle line, and various instruments were then employed to dilate the opening into the bladder.

Mr. Coulson here remarks—"Dilatation was the theory, laceration the practice: the fact is, that the whole of the canal, from the opening in the bulb to the body of the bladder—that is to say, the membranous portion of the urethra, the prostate and neck of the bladder, are more or less contused, lacerated, and injured in a frightful manner."

Willis remarks on this method, "No time was allowed for stretching, and as a consequence the parts were torn." Now, the real truth is, that the parts were not injured by the rapidity of the stretching, but by the nature of the instruments employed to stretch them, as we shall hereafter see. Nevertheless, in spite of this frightful laceration and bruising, the operation was so successful that we find surgeons disposed to recur to it even in our own time.

In the year 1727 Douglas, having observed the capability of distending under a gradual dilatation of the male and female urethra, suggested making an opening through the perineum into the urethra, and then dilating with gentian and other tents, and came to the conclusion that "these artificial fistulas may be dilated so as to extract any stone without cutting the body of the bladder or lacerating any of its parts." In this conclusion, says Dr. Willis, I unreservedly agree.

In 1796 Deschamps proposed a mixed method, but his incision, though directed to the membranous portion of the urethra, was oblique and not in the median line, but he dilated as in the Marian method, but more slowly.

In 1819 Sir Astley Cooper, with Drs. Neil and James Arnott, succeeded in extracting a stone through a fistulous perineal opening, enlarged by the beautiful hydraulic dilator of Dr. Arnott. The dilatation lasted thirty hours—the stone was as large as a small walnut, the diameter of the passage being dilated to three-quarters of an inch in diameter.

In 1826 Monsieur Guerin, of the Bordeaux Hospital, proposed an operation as follows: On the grooved staff an incision was made, commencing at the bulb, and

carried down the raphé through the sphincter ani into the rectum. The groove was then entered by the membranous portion, and a director carried on into the bladder, and the staff withdrawn. The forefinger was now gently pressed into the bladder, where it was retained some time, and the dilatation completed with dried tents pierced with a canula for the escape of the urine.

In 1830 Dr. BUCHANAN, Surgeon to the Royal Infirmary of Glasgow, recommended an operation similar to the one known as Dr. Arnott's method, ignorant that it had already been carried into practice by Sir A. Cooper, at the recommendation of the Arnotts.

In 1840 I performed an operation subsequently described, for the removal of a calculus, by cutting in the median line and dilatation.

In 1843 a successful operation on DR. ARNOTT'S plan was performed by Mr. Elliott. Another by Dr. Wright, of Malton, and in the same year an unsuccessful case by Mr. Fergusson, of King's College Hospital, upon, if I remember right, an aged man, who died from exhaustion. In 1846 was published in the 'Medico-Chirurgical Review' an account of, perhaps, the most successful method of operating that has ever yet been practiced; and as it appears to be scarcely known to the profession, I shall take the liberty of extracting verbatim, the portion of the review which especially treats of it.

Extract from 'Medico-Chirurgical Review:' "This operation is published to the world by an Italian named Joseph Bresciani de Borsa, in an essay on *Theoretical and Practical Surgery*, published at Verona in 1843, and *Theoretical and Practical Observations* published at the same place in 1844."

The Reviewer says, "It is not often that works on Medicine and Surgery by Italian practitioners come into our hands, and, judging from the specimen before us, we regret that we do not see more of them in this country. Dr. Bresciani de Borsa is in large practice in Verona, and is Surgeon to the General and Lying-in Hospitals of that city. The author details at great length the symptoms, modes of detecting, and prognosis of stone in the bladder. After dwelling minutely and ably upon the anatomy of the parts concerned, and describing all the various operations which have been devised for the removal of calculus, he presents us with an account of his own mode of procedure, which he states to have been attended with an amount of success which seems to us almost incredible, much as we are disposed to rely upon the veracity of so industrious and candid an observer.

"This operation is, in fact, a modification of that of which Signor Manzoni of Verona published an account in 1808; and Dr. De Borsa declares that of one hundred cases operated upon by that gentleman and himself, only one has died, and that from causes irrespective of the operation.

"MANZONI'S operation consisted in cutting into the spongy portion of the urethra only, and then dilating the bulbous and prostatic portion sufficiently with the finger to admit of the introduction of the forceps and the removal of the stone. Dr. De Borsa, however, judging it more reasonable to cut into the more dilated portion of the urethra, carries his incision from the bulb to the prostate, and even unavoidably scarifies the latter, when it advances more than usually forward. As the admission of such an operation depends upon the dilatability of the prostate being satisfactorily proved, the author advances several reasons for this, founded on the nature of its anatomical structure, and cites various facts observed by himself and others. He then details the various steps of his operation. 'Having placed the patient in the usual position (it is much preferable to retain him in this by means of assistants than by ligatures, for the mere ceremony of adjusting these causes a great dread to the patient, which may alone suffice to induce a low and fatal form of fever), introduced the staff, and made a sufficiently large external incision, I opened with a small, lancet-pointed, double-edged, strong scalpel, the whole of the membranous portion of the urethra, so as to expose the instrument to the extent of about ten lines, in doing which, it may in some cases easily happen that the apex of the prostate is also cut; although in the case in which the patient died it was found entire. I now take hold of the handle of the staff, and passing my left forefinger into the wound, feel the groove exposed; and as others would pass some sort of gorget through the prostatic portion of the urethra and neck of the bladder, I only introduce my finger into the bladder, being certain that it never can make a false passage, since I keep it in contact with the metallic instrument. I take care, nevertheless, not to pass my

finger along the groove, because I should then thrust it against the internal or posterior angle of the wound, and then against the great bulk of the prostate.

"'Scarcely do I touch the groove before, instead of following it, I pass my finger upon the right side of the staff (as regards the patient), and carry it quietly and without any obstacle into the bladder. One of the advantages of this modification is that it enables me, in most cases, to come at once in contact with the stone. I then remove the staff, still, however, retaining my finger within the track of the incision, and gently moving it about in a semi-rotary manner, effect a much greater dilatation of the prostatic urethra and neck of the bladder. Next I pass in the forceps behind my finger, and seize the stone.

"'It has to pass along a track of only from twelve to fifteen lines, or even less, since the inverse cone once formed by the forceps approximates the neck of the bladder and urethral aperture, for which reason the space is shorter and more easily dilated. I remove the stone with two, or at most three semi-rotations, performed with circumspection and care along an axis, which, commencing at the neck of the bladder should pass through its neck, and following the centre of the prostatic urethra, terminate in the centre of the perineal aperture.

"'I am, in general, not more than a minute, after opening into the urethra, before I have extracted the stone, and the operation has always succeeded in my hands. Safety, simplicity, and celerity, I have already said, are desiderata in every operative process, and I am in a condition to prove that they attach to this one recommended by me. All I employ are simply a bistoury and the forceps; simplicity, surely, in comparison with the multiplicity of instruments which has been recommended in the various and numerous modes of performing cystotomia.

"'By passing my finger along the staff, I secure that safety which is not usually a characteristic of operations for stone. It is related that even the celebrated Scarpa passed the gorget, which was looked upon as the palladium of his fame, in between the bladder and the rectum. The same thing has occurred to many, otherwise skilful operators. I am certain of not injuring either the pudendal artery, the prostatic venus plexus (so frequently in a varicose condition in the aged), the body of the bladder, or the rectum. Farther, by the preservation of the whole of the prostatico-vesical canal, inflammations between the rectum and bladder are avoided, inasmuch as urinary infiltration is prevented, in consequence of the prostate not being divided. The rapidity of the operation is shown by the fact, that instantly after I have opened the urethra I have the stone in my hand; and any one who has even once performed this operation will bear witness to my assertion.

"'I may observe, that it seems almost impossible that so many operators, at all periods, have written so much, and cudgelled their brains to invent new operative proceedings for lithotomy, by deep prostatico-cystic incisions, while with a simple urethrotomy the desired end may be obtained, as I can prove by so many cases.'

"The author next enters into a detailed account of the twenty or thirty modifications of the operations of lithotomy which have been at various periods devised, exhibiting the particulars in which his own may be advantageously compared with these, and terminates his essay with a report of the discussion upon the dilatability of the prostatic portion of the urethra which took place in the medical section of the scientific congress held at Padua two or three years ago. At this, several eminent practitioners expressed doubts of the feasibility and safety of his proceedings; but he truly observed that facts are far beyond theoretical objections. Upon it being insinuated by some that success attendant upon the removal of small stones ought not to confer approval upon a method, Dr. Borso replied by exhibiting large calculi which he had removed. He stated, however, that neither this nor any other form of operation can be universally practised, and that the bilateral operation of Dupuytren is best adapted for stones of great magnitude, and the hypogastric operation when this is excessive. So, too, when the prostate is much diseased, the hypogastric operation may be more eligible. The dilatability of the prostatic urethra, independently of incision or laceration, however, was considered so doubtful by many, that a commission was appointed to conduct some experiments on the dead body. However, the commission was formed, and he consented to exhibit the celerity with which the finger and forceps might be introduced into the bladder, but declined allowing the soundness of his practice to depend upon conclusions drawn from circumstances so different from those prevailing in the living body. Here he only exer-

cised a sound discretion. In the two instances experimented upon, on examination afterwards, the prostate was found to be only slightly lacerated to the extent of two or three lines, although the forceps had been introduced, and the stone removed by the aperture.

"This for the dead body; but the author reiterates that, in the living subject, a division of even the apex of the prostate is exceptional and unessential, and that in the only case in which death allowed of an examination being made, the part was found entire, as testified by competent witnesses, although the stone was fourteen lines in diameter.

"He observes, moreover, of those who persist in maintaining that he must lacerate the prostate in dilating it, that they must find great difficulty in explaining Manzoni's great success, who only opened into the spongy portion of the urethra, leaving entire between the end of his incision and the apex of the prostate, the bulb and the membranous portion of the urethra, and extracting the stones without any laceration whatever, and with uniform recoveries.

"The position the author occupies, as one of the élite of the profession, in his own country, his great experience and evident candour, lead us to accept his statements as well grounded, and to hope that they will be put to the test of experiment among ourselves."

I have made this long extract, considering it to be one of great importance, and as also necessary to show in what respects my operation differs from the one here described.

I may first observe that the ligatures are not at all necessary to control the patient, unless we have a deficiency of assistants. The method I employ is simply this: I introduce a grooved staff in the usual manner, and of the usual size, and confide it to an assistant, with directions to keep it perpendicular and hooked up against the pubes; I then introduce the index finger of my left hand into the rectum, placing its extremity in contact with the staff, as it occupies the prostate, and press it firmly against the staff, so as to steady it; then, with a sharp-pointed, straight knife, with tolerably long and rough handle, I pierce the perineum in the middle line, about half an inch above the anus, or at such distance as may appear necessary to avoid dividing the fibres of the external sphincter,—I carry the knife steadily and firmly on till it strikes the groove of the staff, the deep sphincter lying between the knife and the directing finger, which enables me to judge of the distance as the knife passes along. If the incision be not made exactly in the median line, the contracting fibres of the injured muscles draw the point of the knife from its direct line and interfere with the accuracy of striking the staff; hence the advantage of the long rough-handled knife, which affords a firmer hold and better purchase. Having struck the groove of the staff, I move the point of the knife along the groove towards the bladder a few lines, and then withdraw it, cutting upwards, so as to leave an external incision of from three-quarters of an inch to one and a half inches, according to the presumed size of the stone—the escape of urine indicates the entrance to the urethra. I then introduce a long ball-pointed probe or wire through the external opening into the groove of the staff and slide it into the bladder, to sufficient depth to insure its safe lodgment in that viscus, and withdraw the staff. I then well grease the index finger of the left hand and pass it along the probe, with a semi-rotary motion, through the prostate into the bladder; which procedure is achieved without difficulty, and when the stone is free it comes at once in contact with the finger, and, if of moderate size, passes at once into the wound on withdrawing the finger, the patient having power to strain upon and thereby facilitate the extraction of the stone; this last-mentioned power being one of the great advantages of this operation—the incision being made strictly in the median line no muscles are divided, and the integrity of the bladder being preserved, it is under the control of the patient, who exerts, at the wish of the surgeon, a powerful propulsive effort which keeps the stone in or in contact with the internal extremity of the wound, where it is easily seized by the forceps and extracted by mild persevering traction. Now, as the aperture is necessarily the size of the finger which produces it, if the stone be large, some other dilating power must be employed in addition to the dilating effect of the forceps and stone combined; for this purpose Weiss's three-bladed female dilator, Arnott's hydraulic dilator, or, what is at once ready and effective, the addition of the vulcanized india-rubber finger stalls, one over another, until the finger is sufficiently

enlarged for the purpose, the outer covering being well lubricated with lard before being introduced. But Arnott's dilator, where it can be procured, is by far the most efficacious, though not the most expeditious means. Should the stone be of unusual size, it may be readily broken by a short, strong, and straight lithotrite, or by a strong and suitable pair of forceps closed by a screw, if the stone be soft and yielding—I say readily, because the stone is, in this operation, within so short a distance of the external aperture that mechanical aid can be brought to bear upon it without the slightest difficulty or risk; again, should the stone resist the efforts to crush or extract it, the wound can be readily enlarged upwards or downwards, by dividing the deep fascia, or even be converted into a bilateral aperture sufficient to extract any average sized stone. I believe the deep fascia to be the great obstacle to the extraction of the stone: I have observed that it acts like a ligature round the finger or forceps, and resists the extraction of the stone. The patient suffers little in this operation, and merely complains of the pricking-stabbing sensation of the first thrust of the knife; the subsequent extraction of the stone does not appear to cause pain; he passes his urine freely by the urethra as well as by the wound, from the time of the operation, and there can be little doubt that the wound might be nearly healed by the first intention with perfect safety. Two of my patients were up and out the day after the operation, and one was walking out on the third day (a cold, snowy, frosty day). The wound, left entirely to nature, without tents, &c., heals in about three weeks. The patient, from the completion of the operation, excites no anxiety for his safety—he usually sits up and moves about on the following day, and I cannot well imagine the advent of inflammatory or other bad symptoms.

The three principal features of this operation, as differing from De Borsa's, are—First, the introduction of the finger into the rectum as a guide, by which the staff is held steady, and the course of the knife guided, so as not to approach too closely to the rectum and to insure the striking of the groove of the staff on the first attempt.

Secondly,—The cutting on the staff daggerways, and completing the incision at one introduction of the knife.

Thirdly,—The complete withdrawal of the staff on the introduction of a long probe, which renders the chance of lacerating the prostate less, on the forcible introduction of the finger.

The great advantages of this mode of operating can scarcely be estimated, since any surgeon of average skill and anatomical knowledge may undertake the operation with every prospect of success, and with the almost certainty of his patient doing well: it is impossible to miss the bladder, and equally so not to extract the stone. Nothing but constant practice can make perfect, but if there be a great operation, which can be done successfully with little practice, this is the one; to perform it first on the dead subject is no doubt a great advantage, but the operation on the dead is quite a different thing, there is little resistance to the passing of the finger, the sensation which it produces is totally different—there is no expulsive effort of the abdominal muscles, to propel the stone into the wound—there is no blood—no evidence of feeling—yet I would recommend every man, about to operate for the first time, to try his hand upon the dead subject first. Let him learn to construct Arnott's dilator, and estimate the respective powers and advantages of different dilators,—reflect upon the anatomy and relative situation and distance of the parts to be operated upon.

The first operation which I performed for removing a stone on this principle, was fourteen years ago; the calculus was sharp at one extremity, and had become engaged in the neck of the bladder, whereby the flow of urine was arrested, and the patient, a youth of about nine years of age, was suffering the most intense agony. A catheter was introduced which pushed back the calculus, but, impelled by the efforts to pass urine, it instantly returned to its seat. I passed a staff into the bladder, introduced the forefinger of my left hand into the rectum, and then made an incision in the raphé just above the anus, and carried the knife on into the membranous portion of the urethra—here the extremity of the calculus was touched by the point of the knife, and I made an effort to seize it with a pair of dressing forceps, but it slipped back into the bladder.

I then passed a probe into the bladder, guided by the staff, and withdrew the latter instrument; I then thrust the index finger of my left hand slowly and carefully along the probe into the bladder, when the stone immediately followed the withdrawal of

the finger and was readily extracted with the forceps. A flexible catheter was then introduced through the urethra, and secured by means of a ring and tape to the penis. In the course of the night urgent desire to make water caused the little fellow to get up, when the catheter escaped and the urine followed in full stream. The next day my patient was up and about, and he never had any difficulty afterwards in passing his water by the natural passage, the wound soon healed, as it was small, and very little urine escaped by it at any time.

The calculus was as large as a large French bean, but elongated and sharp at one end. This case produced a deep impression upon me—the facility with which I pursued the object of my search, the freedom with which the urine subsequently passed, the entire absence of any unfavourable symptom, and the patient getting about at once and without any apparent suffering, convinced me that an operation by dilatation was not only practicable, but free from danger.

In 1849 I was called upon to attend a little boy aged two years and seven months, who had been suffering for some months with symptoms of stone. During the few weeks antecedent to my attendance, he had prolapsus of the rectum to a frightful extent, the gut being ulcerated and inflamed, and covered with particles of grit and dirt. I prescribed full doses of laudanum with a little bicarbonate of potash, which afforded great relief. I was in this case very anxious to try Dr. Arnott's operation. I wrote to Dr. James Arnott, of Brighton, on the subject, but the patient being a violent-tempered child, and residing two miles from my house, I felt that it would involve great trouble and delay in effecting the necessary amount of slow dilatation, although I had provided myself with all the instruments necessary for the operation. On pointing out to the Doctor the difficulties with which I should have to contend, he recommended me to try De Borsa's operation, and kindly referred me to the number of the *Review* containing its description, when I was not a little astonished to find that it was an operation very closely resembling the one I had performed in 1840. The morning for the operation arrived, I determined to try Arnott's dilator, but to employ it rapidly instead of gradually, and was only prevented doing so by discovering that the tube of the dilator had been imperfectly soldered, and consequently permitted the contents to escape. I, therefore, commenced the operation which I had before so successfully practised, and completed it in a few seconds, and with astonishing ease.

The child being bound, which in this case was essential, from its violence of temper, I introduced a grooved staff, which my friend, Mr. Hadley of Birmingham, held firmly at a right angle with the body, and hooked steadily underneath the pubes. I then passed the index finger of my left hand into the rectum, when I could readily detect the calculus alongside the staff—I pressed the point of the finger against the staff so as to steady it. I then entered a straight narrow-bladed scalpel about half an inch above the anus, and carried it forward to strike the groove of the staff in the membranous portion of the urethra, but did not succeed in consequence of the shortness of the handle, which prevented me holding it with sufficient firmness. I therefore withdrew my finger from the rectum, and passed it into the wound, feeling for the groove of the staff with the finger-nail, and then opened the membranous portion of the urethra.

I now introduced a long probe-pointed wire through the groove of the staff into the bladder and withdrew the staff, then introduced my finger, previously greased with lard, with a semi-rotary motion into the bladder, where it came at once into contact with the stone, and so powerful were the expulsive efforts that I scarcely needed the forceps to extract the stone, which was an inch and three-eighths long, and seven-eighths of an inch wide.

During the night the urine passed freely through the wound, as well as the urethra.

The prolapsed bowel, which was most troublesome during the operation, had resumed its natural state and had acted freely during the night. The next morning I was both astonished and alarmed to find that my patient was out—in the temporary absence of his mother he had got up and crossed the street in his nightdress without any apparent inconvenience—and from this time he continued to run about and play as before the operation. The wound was healed in about three weeks.

The last case upon which I operated at the commencement of the present year, was a young man of twenty years of age, who had been suffering frightfully during

the last two years, and latterly had passed large quantities of blood with his urine—he was excessively emaciated, and by no means in a desirable state for the operation, but by opiates and a few days' quietude and careful diet his condition was much ameliorated, and I decided upon removing the stone (a large mulberry calculus). My friend, Mr. Hadley, with Mr. Duncalfe, kindly rendered me assistance, and the operation was performed as the others. I at once struck the groove of the staff, and completed the incision at one introduction of the knife. I had, however, not provided myself with a dilator, and the stone, which was an inch and a quarter in diameter, and round, required some little effort to extract it, the forceps increasing its diameter to nearly two inches.

Yet the patient assured me that he suffered only during the stabbing incision, which was momentary, and, as in the previous cases, the urine flowed freely through the urethra from the first, and when the patient pressed his hand on the wound, which he generally did when he wanted to make water, the urine flowed without obstacle by the natural passage. This man was operated upon on the Monday afternoon, and on the Wednesday following, when I called to see him, he was out walking in the frost and snow, and never had an unfavourable symptom—he rapidly acquired flesh, and was quite well and the wound healed in three weeks.

The advantages of this operation are these:—The impossibility of missing the bladder—the smaller amount of cutting than in the lateral operation—the neck of the bladder being uninjured—the smaller amount of blood lost—the prostate being merely dilated not incised—the urine being at once passed by the urethra as well as by the wound, unless union by the first intention be effected—the facility with which the stone is reached, the patient being able to propel it towards the wound—the very short distance between the external opening and the interior of the bladder—the capability of breaking or crushing the stone, and washing out the bladder and freeing it from any minute particles—the small amount of pain—the absence of danger from urinary infiltration—no muscle or vessel of any consequence being divided, no subsequent imperfection can arise—no danger of wounding the rectum—the rapid recovery,—the patient being able to go about the next day—and the great facility with which the operation can be done by any practitioner of ordinary skill and ability.

The recommendations for guidance in the operation are few:—Have your knife straight and narrow bladed, with long handle,—have a good long ball-pointed probe—take care to pare the nail of the dilating finger, so as not to injure the prostate—employ plenty of grease to lubricate the finger—mind that the probe is well in the bladder before you withdraw the staff—and if the stone be large, run a probe-pointed scalpel down the groove of the staff, and cut down towards the rectum through the deep fascia, the rectum still being protected by the finger; or, while your assistant is employing traction and the stone dragging forward the deep fascia, carefully divide with the point of the knife the obstructing margin of fascia downwards towards the rectum, which will enable you to extract the stone more easily—take especial care to have the means of dilating, and instruments for crushing or breaking a large stone—have your forceps as light as possible—take care to wash out or inject the urethra, so that no coagula of blood obstruct the flow of urine—and after the operation place a piece of soft moistened sponge against the perineum to absorb any discharge.

Reflecting upon the unfortunate cases which have so recently and unjustifiably been dragged before the public, I cannot but regret that this novel and safe mode of operating has not been published and circulated sooner amongst the profession, as I flatter myself that its general adoption would render the mortality trifling indeed, and put an end to the deplorable misfortunes which will occur occasionally to the best operators. Every surgeon who has performed an important operation must have felt how cruel and unjust is the criticism too frequently indulged in by rival practitioners, and it is but too true, that these criticisms and comments generally proceed from a class of men either incompetent themselves or jealous of the pretensions of their neighbours. That abuses should be enquired into, there can be no doubt, but that an error of judgment or manipulation should be visited by public censure and exposure, is monstrous. First, carefully select the best men for public appointments, then protect them to the uttermost in the exercise of their onerous and responsible duties, and let it not be imagined that a man who commits an error of judgment, by which the life of a fellow creature is sacrificed, remains unpunished—

the accusing conscience is a far safer and more salutary monitor than the brawling censure of the unfeeling, the unprincipled, and the incapable.

I am quite sure that a safe and easy method of removing stone is a desideratum which can scarcely be appreciated—had we but the correct statistics of lithotomy in hospital and private practice, we should not find men always pursuing the too fatal steps of their predecessors, fearful of the result, yet dreading to deviate from the long beaten but treacherous track.

If, as I have shown, the calculus can be got at so readily in the median line, without dividing muscles, arteries, or other important parts—with the certainty of never missing the bladder, and never dividing the seminal ducts—with the capability of extracting large calculi or readily crushing them, the patient all the time retaining the expelling power of the bladder to aid the efforts to extract the stone—urinating freely by the natural passage—suffering little during the operation, and comparatively nothing after, and able to walk about on the following day; surely we may claim for such an operation the favourable consideration of the profession. That it should have a fair trial, I am most anxious—that it will succeed, I am most sanguine—and that it will be a great boon to sufferers from stone, I most firmly believe.

Having described the mode of performing the operation (see page 291), I shall proceed to explain its anatomical bearings, and what I conceive to be its advantages over the ordinary lateral operation, enumerating at the same time the structures divided in the respective operations.

On taking a survey of the perineal region, we cannot but be struck with the peculiar arrangement of the deep perineal fascia, which is destined to support the weight of the viscera, to resist the powerful action of the abdominal muscles, to give attachment to the perineal muscles, and to close in the great pelvic aperture. This fascia, so important in the median operation, is stretched across the perineum, and divided into two layers, separated above by the thickness of the pubes, in the middle by the length of the membranous portion of the urethra, and then becomes united at the anus. Between the layers of deep fascia are situated the membranous portion of the urethra, Guthrie's and Wilson's muscles, Cowper's glands, the arteries of the bulb, and part of the internal pudic arteries, but as we cut in the middle line in performing this operation, and the parts above-mentioned are placed on either side of that line, it is obvious that they will not be divided or injured if the operator be careful.

As regards the instruments employed for distending the aperture made into the bladder, we may mention Weiss's two and three-bladed female dilators, which will answer very well where the dilation is not required to be considerable; but no instrument can approach Dr. Arnott's hydraulic dilator,—it is one of the most beautiful applications of physics to surgery that has ever been made,—and so manageable yet so powerful is this instrument, that we can dilate with it suddenly—or almost imperceptibly, and to any required extent,—at the same time feeling sure that however much the parts are stretched they cannot be bruised, since the pressure is distributed so equally over all the parts submitted to it.—*From "Lithotomy Simplified."* By George Allarton, Esq. London: Ash and Flint, Wellington-street, Southwark.

157.—ON LITHOTOMY.

By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

[Mr. Syme gives us an excellent lecture on this subject; but before proceeding to the practical part of his lecture, he makes a few remarks on the symptoms of stone.]

If the patient has a frequent desire to pass urine from uneasiness, which is relieved after micturition, then the case is probably one of mere irritability. If the pain occurs during micturition only, there is probably obstruction to the flow of urine, such as is produced by stricture; but if the pain be little or none at the beginning of micturition, increases towards the close of the act, and lasts with severity for a short time after it, then the probability is, that there is a stone in the bladder. There are also other symptoms of more or less importance; but without entering upon them, I must hasten to speak of the operation.

The stone is in this case obviously too large for crushing, which is not expedient, except for very small stones; it must, therefore, be cut out, and how is this to be done? In some respects there is, and long has been, a great approach to unanimity

on this subject, but there is a point of the utmost importance, with reference to the safety of the operation, which is still not yet sufficiently understood or recognised by the profession generally. All in this country are agreed that it is not expedient to cut above the pubes, or to use the recto-vesical operation, and that the lateral operation of Cheselden is the best; all are also agreed that the incision in the skin should be free and large in proportion to the size of the parts, and that its anterior extremity should be situated at the raphé of the perineum; also that any resisting fibres of the levator ani, or other perineal muscles, should be divided, and that the incision of the urethra should not extend further forwards than the middle of the membranous portion. So far, ninety-nine surgeons out of a hundred are agreed; but now comes the difficulty, how far must the incision be carried through the prostate? In order that you may understand this question fully, it will be necessary for us to begin at the beginning, and to take a rapid glance at the history of the subject. The great fundamental error in the operation of lithotomy formerly arose from the notion that incisions through the membranous parts were mortal: as the neck of the bladder being a membranous part, it was supposed that to cut it would be to ensure the death of the patient. This prejudice was the foundation of the operation by the "*apparatus major*," as it was called, in which an incision was made as far as the prostate, and then instruments were introduced, in order to dilate the prostatic part of the urethra and neck of the bladder. This, however, was a delusion, for the part admits of dilatation only to a small extent, and if stretched to a greater degree must be torn, so that the operation by the apparatus major was really a process of tearing, except when a very small stone required to be removed. The next step was the sudden and surprising appearance of Frère Jacques, who, throwing aside the details of the tedious process previously used, as John Bell expressed it, thrust his dagger-shaped knife into the patient's hip, with no other guide than a round staff, and extracted stones of the largest size without difficulty or delay. It is true his patients generally died, but some recovered, and this was enough to open the eyes of the profession, and to show that the dread entertained of cutting the neck of the bladder was groundless. The surgeons of France, Holland, and England proceeded to act upon this conviction; but certainly the most celebrated step in advance, at least as we think, was made by that distinguished countryman of ours, if I may so call him, Cheselden, who operated by his lateral method with a success, that, taking all in all, has hardly been surpassed. It might be said that the operation was now perfect, and that nothing more was required. Unfortunately, however, the difficulty was only begun, the question being, what did Cheselden cut? You would say, there could be no difficulty in ascertaining this; as being a good anatomist, he must have been himself aware what parts he divided, and would communicate the fact to others. You will find, however, that eminent commentators differ in their statements as to his incision, and you will have little difficulty in understanding how this happened when you consider that the part is not within reach of sight, so that the operator himself can alone have any knowledge where the knife is going, and even he can judge but imperfectly at a part beyond the reach of touch. Rau was a very successful lithotomist; he is even said to have operated in 3000 cases without losing a patient, though that is hardly to be credited; but Rau would never tell any one what he cut. Albinus thought he knew from what he had seen as his assistant, and described as Rau's an operation which misled Cheselden, who, having been induced to try it in consequence of Rau's great success, found that he lost two-thirds of his patients, and therefore went back to his own method. It appeared, in fact, that successful lithotomists were lucky enough to get into a knack of making their incisions safely, and that having acquired this power they were able to employ it themselves, but were unable to communicate it with any certainty to others. This appeared very unsatisfactory, and at an early period of my professional career I endeavoured to ascertain with precision, what it was that required to be cut. It was plain that the success of some surgeons and the failure of others could not be due to chance, but that there must be some reason for it; and I had not investigated long before I thought I saw this reason. The prostate is made up for the most part of a texture of no great firmness, and which does not much resent injury. You will see ample evidence of this if you examine the prostate of a person who has been long treated for retention of urine by an unskilful operator. You will probably find numerous small passages in the gland made by forcible thrusts of instruments, yet these tracks

are soundly healed, and the patient has been apparently none the worse for them. But it appears that there is one part of the prostate that is not thus tolerant of injury, and this is placed just at its base. If you take a chestnut, and run a bougie through it from the apex to the base, as I have done with the one before you, and hold the bougie as if it were introduced into the bladder, you will be able to form a tolerable idea of the form and position of the prostate gland. It is agreed by all that the incision through it should not be made directly downwards or directly to one side, but midway between these two directions. Now, at the base of the gland, where it joins the neck of the bladder, there is a dense texture, forming a sort of ring around the urethra, represented pretty well in superficial extent by the *hilum* of the chestnut; it is thickest immediately under the mucous membrane, whence it gradually tapers away. This part of the prostate seems endowed with an extraordinary degree of sensibility. If you pass a bougie in a patient who has never had it done before, you may introduce the instrument up to the prostate and a little way into it without producing more than an odd sensation, scarcely amounting to discomfort on the part of the patient; but when you push it on to the neck of the bladder, he perhaps faints and falls at your feet. When a patient affected with stone is going down stairs, he suffers to a very great degree, and this appears to be due to the stone being then thrown on this sensitive part; and in the same way great suffering is experienced when the calculus is forced upon the neck of the bladder in the empty and contracted condition of that organ. This sensitive part is, as I before remarked, of very dense texture, and it is very tough and unyielding. I have tried careful dilatation of it on the dead subject, but have hardly been able to enlarge the ring to more than a circle of an inch in diameter. There can be no doubt that if this texture be torn the patient will die; if it be extensively lacerated, death will probably occur within two days; and if the injury be of less extent, chronic inflammation will be set up about the neck of the bladder, leading with no less certainty to a fatal termination. If, however, the prostate be divided as far into its substance as this ring extends, the rest of the gland tears very readily, and without any bad consequence, the muscular fibres of the neck of the bladder separate from each other, and the mucous membrane also yields to the dilating force, so that sufficient space can be obtained without difficulty for the extraction of any stone which the outlet of the pelvis will admit. The result, then, of my investigation was that the different success of different surgeons must depend upon this ring being divided sufficiently by some, and not by others. I then inquired into the means used by different surgeons for making the incision in the prostate, and found that all those who were successful used instruments that might ensure the complete division of the texture in question. This odd-looking instrument is the knife used by the late Mr. George Bell, of this city, who operated very extensively and successfully for stone. This, again, is Dubois' knife, and this the "bistourie cachée," which, when properly set, will ensure, like the other two instruments, the necessary division of the prostate. The most curious illustration of this subject is furnished by the knife of M. Le Dran, who is said never to have lost a single case. It is a curious fact, that I at one time contrived an instrument of precisely similar form, and used it until the introduction of chloroform made it no longer necessary; and you may believe, that instead of being sorry to find that I had been anticipated by Le Dran, I was very glad to find the precedent of so distinguished an operator. The knife runs along a straight director introduced into the bladder through the wound in the perineum; and the blade, which is two-thirds of an inch in breadth, is sharp only along the oblique anterior edge. I still believe that such an apparatus is the most certain means of dividing the prostate to the proper extent, when the part is withdrawn beyond the reach of the finger, as it may be when the patient is conscious. When the prostate has been divided sufficiently freely, and the part has been properly dilated with the finger, the urine necessarily escapes, and accordingly I never knew a successful operator for stone introduce the forceps till this had occurred. Not long ago, however, I read, to my extreme surprise, a recommendation from a lecturer on surgery, to take advantage of the flow of urine, so that the stone might be washed into the grasp of the forceps. Now, I do not hesitate to say, that any surgeon who so conducts his operation that the urine does not escape till the forceps are introduced, cannot be a successful lithotomist; he may get on pretty well with young patients, but he will lose a large proportion of adults. Mr. Liston, one of the most successful operators

for stone, would have been horrified at the idea of introducing the forceps before the urine escaped.

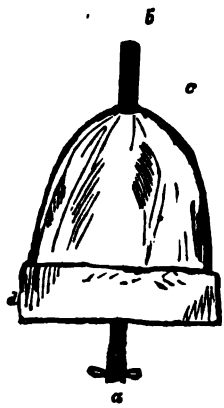
I had long taught the principle I am endeavouring to impress upon you, but had great difficulty in doing so, because Mr. Liston, whose authority was deservedly much respected, had adopted an unhappy way of describing, and a still more unhappy way of representing, the incision through the prostate. In the first three editions of his Practical Surgery, he represents a section of the prostate with an incision, leaving the base entire, and says—"the less that is cut the greater will be the patient's safety." You will remember, the question is not between recklessly extensive incisions, and very limited ones, but between the latter and incisions sufficiently free to ensure the division of the unyielding ring. I had a correspondence with Mr. Liston upon this subject, in which I insisted on the mischievous tendency of the expressions he had used, whilst he, on the other hand, maintained their correctness. At length, in the fourth edition, published only a year before his death, there appeared a paragraph, which I read with very great satisfaction, and which I will now read to you: "I introduce a sketch here originally published some time ago in illustration of some of my surgical lectures in the 'Lancet,' to show what it is indispensable to cut. If that is effected, the dilatation can be carried to any required extent. The two white triangular spaces at the base of the gland, in front and behind, show a section of the dense unyielding fibrous tissue into which the muscular fibres are inserted; this band effectually prevents dilatation or enlargement of the orifice of the bladder beyond a certain and very limited extent, without laceration, dreadful suffering, and imminent danger." This you see completely confirms what I had so long laboured to establish. Entertaining this opinion very strongly, I confess I never felt any thing more distressing than the consciousness that I had not cut what I ought to have cut, in consequence of my not being able to reach the part with my finger. When this happened, the patient died; and yet the result was quoted as an argument against a free incision, while I well knew the error had been of an opposite kind, and it was on this account, that I introduced the knife which I have shown you; but which, I am happy to say, is now no longer necessary, as the patient, when under chloroform, does not make those involuntary muscular efforts by which the parts were, in former times, drawn beyond the reach of the operator, unless he had a very long forefinger. If, on introducing your finger to dilate the deep part of the incision, you feel you have not cut enough, pass a straight probe-pointed bistoury along your finger, and, by a gentle sawing motion, carry the incision on to the extent that you find necessary, in order to enable you to dilate with facility. There is only one other point to which I have time to allude at present, and that is the form of the forceps. It is a curious circumstance, that Cheselden, who, I do not hesitate to say, of all the surgeons of this country, has shown the greatest surgical genius as an operator, has left no work on surgery, except some remarks and figures of instruments appended to Le Dran's work on the operations in surgery, translated by another man. It is this work that I now hold in my hand; here you see represented, in one of the plates, the operation of amputation by double incision to which I alluded in a late lecture; here is a portrait of his lithotomy forceps, and so accurate was everything he did, that forceps can now be constructed by an instrument-maker from this pattern. Observe the difference between these forceps which I had constructed many years ago, after Cheselden's pattern, and this pair, which I obtained from an instrument shop; the blades of the former are, you observe, considerably longer than those of the other pair, and, therefore, approach nearer to a state of parallelism when the stone is seized, and the extraction is thus rendered much more easy. I may remark, that you will generally see the best operators slow in making their incisions, and rapid in extracting the stone. The incisions require to be made with precision, and dilatation must be thoroughly effected with the finger; and it is no matter how long these parts of the operation occupy; but when they have been properly done, the extraction of the stone is a comparatively easy process.—*Lancet*, *May* 19, 1855, *p.* 505.

158.—ON THE PREVENTION OF HEMORRHAGE AFTER LITHOTOMY.

By John Hilton, Esq., F.R.S.

A few weeks ago Mr. Hilton had occasion to perform lithotomy on a man who

was in a very feeble condition, and to whom the loss of even a small quantity of blood would have been of great consequence. As, however, the man was known to be the subject of an enlarged prostate, and had also suffered from calculus for a very long period, it was deemed likely that the parts about the neck of the bladder would be unusually vascular, and that the bleeding might, consequently, be more than ordinary. With this prospect, it became of importance to devise some means of effectually preventing the hemorrhage which was so much dreaded. To the plan frequently adopted, of plugging the wound with sponge, there are several objections: in the first place, the escape of urine is prevented; in the second, the portions of sponge imbibe the urine, and become irritating to the parts in contact with them; thirdly, they adhere to the sides of the wound, and their removal is attended with difficulty, pain, and risk of exciting fresh bleeding.



To obviate these difficulties, Mr. Hilton contrived the little apparatus which we here figure. It consists of an ordinary tube *a b* (such as those employed after lithotomy by Liston and others), to which is attached, by means of a ring *c*, fixed about an inch and a half from its extremity, a conical bag of oiled silk, *d d d*. In use, the end *a* is passed up through the wound into the neck of the bladder, and the end *b* being left projecting externally, the cavity of the bag is next filled with sponge, so as to exert pressure on the surrounding parts. In this way the wound may be most effectually plugged, while the free escape of urine is provided for. When it is thought safe, or becomes necessary, to remove the pressure, the sponges may be taken out, one by one, without the least pain to the patient, as they are prevented, by the intervening layer of oil-silk, from adhering to the sides of the wound.

In the case for which it was devised, the expedient answered admirably. As had been expected, there occurred after the extraction a rather free oozing of venous blood, which was, however, most completely arrested by the plan described. The apparatus was, of course, introduced immediately after the completion of the operation, and previous to the removal of the man from the table.

The device is such a manifest improvement over the old method of plugging, that we hasten to attract the attention of lithotomists to it. Every one will have seen cases in which the oozing of even a few ounces of blood added very materially to the patient's danger, and in some such, the employment of the plan described might, perhaps, be the means of preserving life. Should the tendency to hemorrhage be very great, Mr. Hilton suggests that portions of ice might be introduced among the sponge, thus to gain the advantages of both pressure and cold. The apparatus is made by Messrs. Bigg, of St. Thomas's-street.—*Med. Times and Gazette, May* 19, 1855, *p.* 490.

159.—ON THE INJECTION OF IODINE IN OVARIAN DROPSY.

By I. B. Brown, Esq. (Read before the Medical Society.)

The author said that the injection of the tincture of iodine into ovarian cysts was almost new in London practice; not so, however, in Paris or Edinburgh. He mentioned that the injection of the tincture of iodine into the tunica vaginalis for the radical treatment of hydrocele was prominently brought before the profession many years since by Mr. Ranald Martin, and that it is now the regular practice among English surgeons; but the French surgeons have carried their experimental inquiries much further in this direction. Velpeau first suggested its application for effusion into large joints. Bennet, of Lyons, however, made the first trial, and with perfect success; this success was soon followed by many others in the hands of Velpeau, Robert, and some others. Then Boinet, of Paris, took up the subject, and applied the injection of iodine into ovarian cysts, and published several successful cases in the 'Gazette Médicale de Paris.' Others followed his example; among the number, Monod, one of whose successful cases Mr. Brown said had fallen under his own

observation. The author further said that French surgeons have also used this injection of iodine for peritoneal dropsy, and it appears from various discussions and trials, with undoubted success. Our intelligent neighbours have gone further, especially Velpeau, Jobert, Maisonneuve, and Ricord, and have injected the peritoneal sac of hernia, and have obtained by it the radical cure of ruptures. Mr. Brown observed that lately his colleague, Mr. Coulson, of St. Mary's Hospital, had tried this plan. The author said that the first real practical application of the injection of iodine into ovarian cysts that he knew of in this country was by Professor Simpson, of Edinburgh, who, with his usual zeal and industry, has pushed his acquirements and experiments very freely. The result of his investigations Mr. Brown said he would allude to fully when he gave his own practical remarks. The author then said he would relate a case which he had lately treated in St. Mary's Hospital, and then offer some practical remarks.

Mary B., kindly sent him by Mr. Coulson, was admitted into the Boynton Ward on the 9th of December, 1854. She stated she had had two children, and one miscarriage. She was 27 years of age at the birth of the first, and 29 at the birth of the second child. From the latter period, she had noticed herself getting bigger around the waist, and troubled much by flatulency; she has been regular in menstruation all through her illness; she has no pain, but suffers considerable inconvenience from leucorrhœa. Mr. Brown examined her on the 12th of December, and found a well-marked ovarian cyst, apparently unilocular, and fluctuation distinct; the measurement around the abdomen was twenty-nine inches below the umbilicus, and twenty-eight above. He placed her under medical treatment, with a view to improve her general health, which was much impaired by the secretion of the fluid into the cystic cavity. On the 20th Mr. Brown proceeded to empty the sac. First placing the patient in the horizontal position, he then introduced a large trocar through the semilunar line, and evacuated twenty pints of a thin, turbid fluid, which was found to be strongly albuminous—almost solidified by the joint application of heat and nitric acid; it also contained abundant crystals of cholesterine.

Mr. Brown then introduced a long, flexible catheter, and through it injected five ounces of the tincture of iodine (of the Edinburgh Pharmacopœia), which is about double the strength of ours. The pain experienced was very trifling, described by the patient as merely smarting. The wound having been closed by strapping, he applied appropriate pads, and one of his many-tailed bandages. The patient was then placed in bed, two grains of opium given, and four ounces of port wine ordered for the next twenty-four hours. In the evening, the patient felt very comfortable, and had no pain or tenderness in her abdomen, only a nasty taste in her mouth, like sea-weed; her breath smelt of iodine. The amount of urine voided for the first two days was more than the fluid taken, but afterwards less. Mr. Brown then ordered a diuretic mixture, and the effect on the secretion was, that the amount of the fluid taken corresponded to the amount voided. There was now an apparent refilling of the cyst, but it proceeded very slowly; the patient's appetite was good; she slept well, and felt no pain.

Jan. 20th, 1855. There appeared about two quarts of fluid in the cyst, but it did not seem to increase, and the patient was decidedly better in health. Mr. Brown then ordered her to wear one of his ovarian bandages, to keep up gentle pressure over the whole abdomen, so as to give support to the whole parietes, and to arrest the refilling of the cyst. In a few days she left the hospital, considering herself much improved, and showing no external signs of the disease. Mr. Brown said he had lately examined her, and could find no increase of fluid, but great improvement in her general health, and she says she is in excellent health and spirits.

The author then said, before offering any practical remarks, he was desirous of drawing the attention of the Society to the important fact of the iodine being taken up into the system, as evinced by the breath and taste in the mouth. He was anxious to ascertain whether it was to be found in the urine, and therefore requested the dispenser, Mr. Copney, to examine some for him on the day after the injection. The urine gave unmistakable evidence of the presence of iodine by the following tests:—

1. The formation of the blue iodide of starch.
2. By its forming, with the salts of lead and mercury, the characteristic iodides of those bodies.

3. By the production of the iodide itself.

Three days afterwards he examined the sweat, but found no trace of iodine; but he thought that if he had tried it sooner he would have found evidence of elimination. Mr. Brown then said, the first question, in a practical point of view, was, Had the treatment in this case been successful? The answer was, that it had been only partially so at present; for although it had not prevented some return of that fluid, still it had evidently controlled the refilling of the cyst, and the general health had much improved. Further time was required to test the extent of benefit. Mr. Brown's opinion was, that in such a case the injection of iodine was not likely to effect a complete cure, yet that it would arrest the refilling. He believed that those cases which were radically cured by this plan were the more simple form of unilocular ovarian dropsy—*i. e.*, when the cyst is thin, and its contents non-albuminous, or slightly so. The dropsy of the broad ligament was especially favourable to this plan; but then so it was for simple tapping and pressure, and the latter plan was less hazardous than the former. Mr. Brown observed that he could not expect to solve the whole question until we knew more of the pathological structures of the different cysts in each form of the disease. He stated that he was investigating this question by the aid of the microscope, and hoped to arrive at some certain facts as to the peculiar condition of the secreting surfaces, and peculiar fluid from those surfaces, so that by drawing off a small quantity of the fluid we might be so certain of our diagnosis as to recommend with greater confidence the adoption of any mode of treatment best fitted for the especial case. Mr. Brown said in the next case he would use ten or twelve ounces, so as to insure the complete covering of all the cystic surface.

Mr. Brown rather attributed the partial failure to the circumstance of the whole of the surface of the cyst not having been covered.—*Med. Times and Gazette, April* 28, 1855, *p.* 421.

160.—ON EXCISION OF THE KNEE-JOINT.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[It is well known that Mr. Syme does not altogether approve of this operation. The profession will see his reasons stated as follows:—]

In the first place, the knee-joint is much larger than the elbow, and its excision is, therefore, more formidable. Secondly, it is a well-established fact, that injuries of the inferior extremity are much more serious in their effects than those of the superior. Thirdly, the leg is merely a support for the body, while the arm carries the hand, by means of which all the undertakings connected with the intelligence of man are carried out. Indeed, some people have gone so far as to attribute his superiority over the lower animals solely to the possession of a hand; although this is an opinion to which it is hardly necessary to say I do not subscribe. The loss of a leg may be supplied by an artificial substitute; but no effort of human skill has ever succeeded in constructing a hand at all comparable to the original member. Last year I amputated the hand in two cases, in both of which Mr. Gray, of London—a very accomplished artist—made artificial limbs, which may be considered as perfect specimens of what the art of man can provide under such circumstances; yet all that can be said of these substitutes is, that they have the shape of a natural hand, and that they possess fingers which may be moved by the other hand; but, for any useful purpose, the instrument to be used—such as a fork, or a hair-brush—must be fixed into a socket in the palm. So, even if the motion of the elbow were not perfect, the mere preservation of the hand would be of infinitely greater consequence than saving the leg. From these considerations I have long been persuaded that excision of the knee-joint is not an expedient operation; and experience, I must say, tends to confirm this conviction. For, in the first place, it appears that there is much more danger connected with excision of the knee-joint than with amputation of the thigh. In the second place, the cure is far more protracted, tedious, and troublesome. Thirdly, the result is very apt to be unsatisfactory. And, lastly, the limb, in its most favourable condition, must require so much careful attention to protect it against the effects of injury and exertion, as, in my opinion, to be less useful than an artificial substitute.

With regard to the first of these propositions, that regarding danger, although statistics have been quoted to show that the operation is not very dangerous, I could lay before you some facts that would lead to a different conclusion; for all the cases operated on have not been published. But I shall pass from that, and allude only to published cases, and you will find from the reports that even in patients apparently most favourable for the operation, it has frequently proved fatal. And if the mortality, as shown by these statistics, were not greater than that of amputation of the thigh, yet you would have to remember that the operation has been performed only in selected cases, while the results of amputation are collected from all sorts, no matter how unfavourable. Yet of the last twenty cases in which I have amputated the thigh in this hospital for chronic disease, not one has died, although some of them were almost hopeless at the time of the operation, on account of the extreme degree to which they were prostrated by long continued discharge, or other causes. With regard to the time and trouble spent in the after treatment, you will find that eight, ten, twelve, or fourteen months of constant care have been devoted to these patients. Some cases which recently occurred in this hospital, and which are considered very favourable, remained here many, many months, with a nurse especially devoted to them, and made the subjects of a degree of attention and care, which, independently of every other consideration, render this treatment, in my opinion, quite unsuitable for hospital practice. You must also consider the risk the patient runs from this protracted stay in an hospital; for the longer he remains with suppurating sores, the more is he, as well as his neighbours, apt to suffer from vitiation of the atmosphere. Then there is the unsatisfactory result. It has been found that there is great difficulty in preventing the bones from being drawn out of the straight line in one direction or another. Sir P. Crampton operated on two cases, of which one died, while the other made a much-boasted recovery, as the patient, to use her own expression, "was able to walk the length of a day." In testimony of her gratitude, she bequeathed her limb to Sir P. Crampton, who divided the bones longitudinally, and presented one half to the College of Surgeons of England, where you will see that they have been anchylosed at a right angle, which, together with the necessary shortening of the limb, must have rendered her mode of progression rather peculiar in its character. Finally, an artificial substitute is, in my opinion, preferable to the resulting limb; for a labouring man, who could not afford to procure anything but a wooden leg, this would be much more serviceable than the shortened limb, in which exposure to changes of temperature and hard work will be apt to set up disturbance, incapacitating him for exertion; while a rich man, who can afford to procure a shapely and flexible artificial limb, would find it preferable to the shortened and stiffened one resulting from the operation; but this I would not insist much upon, as patients are apt to prefer a part of their own body to any substitute, even though the latter be much more useful. But I must say that at one period I entertained a different opinion of this operation, and I entered upon the consideration of the subject prejudiced rather in favour of the procedure than against it, having been the first surgeon in Scotland to adopt it. Of the cases on which I operated, one died, but the other was very successful. It has been alleged by Mr. Fergusson, formerly of Edinburgh, now in London, that in this case "the patient was obliged to support himself by a crutch, whilst the limb hung useless, being shorter than the other by several inches, and so flexible at the cicatrix, that it was totally unfit either for support or progression." I performed the operation in 1829, and in 1831, in my treatise on "Excision of Diseased Joints," stated that the patient could walk and run; and in 1834, at the first meeting of the British Association in this city, I produced the boy, who there stood, and walked, and ran, upon a firm limb, without support of any kind. How the statement to which I have alluded can be reconciled with these facts, I leave Mr. Fergusson to explain. This case, therefore, was a very favourable one; but two considerations afterwards led me to entertain a different opinion of the advantage gained. The first of these was, that as the boy grew, the leg did not lengthen proportionally to the rest of the body, so that he required the sole of his shoe to be four inches thick; but the principal objection was one derived from my experience of excision of the elbow-joint, in which I remarked that it was often long before the oozing of matter ceased from openings in the soft parts. Discharge will often occur in small quantity from an elbow for eight or twelve months after excision of the joint, but in the meantime the hand and arm are as

useful, or nearly so, as if this were not the case; but it appeared to me that, from the large size of the parts concerned in the knee-joint, the discharge of matter might be expected to prove much more troublesome and protracted, while, at the same time, so long as the smallest sinus remained open, the limb would be of no use for support of the body. I recollect once making an experiment in this theatre with a patient, on whom I had performed excision of the elbow-joint about six weeks before. I desired him to walk on all fours; but though the arm was already nearly perfect for the use of the hand, the pressure of the osseous surfaces against each other was so painful as to prevent his leaning his weight upon it. Unless the knee be fit to support the body, it is of course worse than useless. On the whole, therefore, while I do not deny that recovery may occur after excision of the knee-joint, and that even, under peculiar circumstances, a limb of considerable utility may be obtained, I have no hesitation in giving the preference to amputation of the thigh. Professing to practise surgery as a useful art, you must consider what course is the best for your patients in general, and not expose the majority to risk for the chance of benefiting individuals, or performing with success a curious experiment. In any given number of cases of incurable disease of the knee-joint, I believe you would find amputation of the thigh productive of a much more satisfactory result, on the whole, than excision of the knee-joint; and, under this conviction, I consider it my duty to express a decided opinion against the latter operation.—*Lancet, April* 21, 1855, *p.* 397.

161.—ON POISONING BY STRYCHNIA.—ANIMAL CHARCOAL AN ANTIDOTE.

By Walter Chippendale, Esq.

[In a paper read before the Abernethian Society, the author relates the case of a strong muscular man, aged 26, who had taken four grains of strychnia, with an equal quantity of morphia, dissolved in an ounce of spirits. In rather more than half an hour, violent spasms and convulsions came on, attended with extreme pain. When admitted, he had lost all command over the lower extremities, the countenance was flushed, expressive of the greatest distress, the tongue was dry, and he complained of great thirst. The tube of the stomach-pump was introduced, and three or four ounces of animal charcoal injected, mixed with water; the stomach was then completely emptied. He continued very ill for some time afterwards; the paroxysms were frequent and violent, the pulse 150. Chloric ether and brandy were given, and he rapidly improved, the convulsions ceased, he passed a good night, awoke free from convulsions, and was discharged the next day.]

On reviewing this case, we cannot fail to remark the length of time that elapsed between taking the poison and the first manifestation of the symptoms. This will appear the more striking, if we contrast the period that intervened with the results of those experiments recorded by Christison and other observers, where much smaller doses were employed. The former succeeded in destroying a dog in two minutes, by injecting one-sixth of a grain into the chest, and here the symptoms commenced in forty-five seconds; and Pelletier has seen them appear in so short a time as fifteen seconds. In the above case, to the combination of circumstances unfavourable to the speedy action of the poison, may be attributed the retardation of its effects.

In the first place, strychnia is most difficult of solution (and the rapid action of a medicine is always proportionate to its solubility); 2ndly, it was in combination with an equal quantity of morphia, the principle of a drug which arrests all vital processes; and, 3rdly, the patient's system had been fortified by habit against this alkaloid, and had in a great measure learned to tolerate it.

• It may excite surprise that the system did not appear to be at all affected by the morphia, which was given simultaneously, and in the same proportion as the strychnia. This alkaloid, which exerts a directly contrary influence to the latter, may have contributed to the delay of its operation; but we may, I think, more satisfactorily explain the absence of any indication of its action by a reference to the analogous effects of opium in tetanus. We find that in that disease it is most difficult and almost impossible to bring the system under its influence, and fabulous doses have been given without producing any good or ill effect. It might be worth while

to try the converse effect of strychnia in hopeless and irremediable cases of poisoning by opium.

Having thus attempted to account for the long interval that elapsed between the introduction of the poison and the first manifestations of its symptoms, a few words must be permitted respecting the plan of treatment pursued. And in this respect the case is especially interesting, in that it demonstrates the correctness of Dr. Garrod's views as to the capability of animal charcoal to combine with and render inert the poisonous principles of animal and vegetable substances, and overcomes the objections that have been raised against its employment.

The decolorizing and purifying properties of animal charcoal have long been known, and it is largely employed in various pharmaceutical processes. The superiority of animal over vegetable charcoal for this purpose is referred to in the minute separation of the carbonaceous particles by the inorganic constituents of the substances employed in its manufacture; for its decolorizing power is greatly reduced by dissolving out the salts of lime by an acid. It has been found, in the purification of many pharmaceutical preparations, that the effect of animal charcoal is not confined to the removal of colouring matters, but that it also separates a large proportion of their active principles, resins, &c.; so that a considerable loss is always entailed upon the manufacturer in the purifications of the alkaloids. It is capable of overcoming chemical affinities of some intensity, and will remove lime from lime-water, iodine from iodide of potassium, sulphuretted hydrogen from a solution, soluble salts of lead, and metallic oxides dissolved in ammonia. Hence this substance is a very general antidote in case of poisoning. The matters thus separated are not decomposed, or altered in their nature, but simply adhere to the surface of the charcoal.

In all those experiments, which were made by Dr. Garrod, when the strychnia was administered, previously mixed with charcoal, the animals were not in the slightest degree affected, and he stated that in the case of the milder vegetable poisons, when charcoal was given within ten or fifteen minutes afterwards, the results were mostly satisfactory. I believe that this is the first instance on record in which animal charcoal has been tried on a human subject. I have searched through the works of the chief authorities on Toxicology, and have not met with a similar experiment, not even on an animal, when so long an interval has elapsed, or so large a quantity of the poison has been given as in the present instance, and I think that the result is so satisfactory as to establish its recognition as a most suitable remedy in poisoning by the alkaloids.

I may add that Dr. Garrod's experiments were not confined to strychnia, but that he found animal charcoal equally efficacious as an antidote for all the vegetable poisons, as well as for hydrocyanic acid. He was induced by the success obtained in these experiments to try its efficacy in poisoning by mineral substances, and he found that, in many instances, when administered in large quantity, it was more successful than those remedies usually employed. It was certainly observed to lessen the effects of corrosive sublimate and also of arsenic; but it did not neutralize the operation of these substances so completely as to justify an entire reliance upon it alone.

The beneficial effect of charcoal in these cases has been supposed to be purely mechanical, and that it preserves life by enveloping and isolating the poisonous particles, preventing them from coming into contact with the absorbents, the orifices of which it is believed to obstruct. Such an explanation in the case of the inorganic poisons is doubtless correct, but in the case of the alkaloids we may attribute its efficacy, in some degree, to its power of overcoming chemical combination, and, as in the present instance, of reducing the poison to a condition of inactivity, from its greater difficulty of solution in the uncombined state.—*Med. Times and Gazette, April* 28, 1855, *p.* 423.

162.—ON CHARCOAL FOR SANITARY PURPOSES.

By John Stenhouse, LL.D., F.R.S.

The following table exhibits the amount of the more important gases absorbed by a single volume of box-wood charcoal, as determined by the experiments of Saussure:

	Volumes.		Volumes.
Ammonia	90	Bicarburetted hydrogen	35
Hydrochloric acid	85	Carbonic oxide	94
Sulphurous acid	65	Oxygen	92
Sulphuretted hydrogen	55	Nitrogen	75
Nitrous oxide	40	Carburetted hydrogen	5
Carbonic acid	35	Hydrogen	17

This table exhibits the extraordinary power of absorption for gases possessed by vegetable charcoal.

The next table, constructed from experiments made by Dr. Stenhouse, shows the comparative power of the three different varieties of charcoal. Half a gramme (five decigrammes) of the following kinds of charcoal absorb the undermentioned number of centimetres of different gases:—

—	Ammonia.	Hydrochloric Acid.	Sulphuretted Hydrogen.	Carbonic Acid.	Oxygen.	Sulphurous Acid.
Wood.....	98·5	45·0	30·0	14·0	·8	32·5
Peat.....	96·0	60·0	28·5	10·0	·6	27.5
Animal.....	43·5	—	9·0	5·0	·5	17·5

It thus appears that wood charcoal possesses rather the highest absorbent power for ammonia, sulphuretted hydrogen, and sulphurous acid; while animal charcoal is decidedly inferior to both wood and peat charcoal, as an absorber of gases, but as a deodorizer it is of course very greatly superior.

Now, the deodorizing properties of charcoal arise in some measure from its great absorbent power, and which, again, is dependent upon its extreme porosity. The following experiment, made by Mr. Turnbull, of Glasgow, illustrates forcibly the deodorizing power of charcoal. The bodies of two dogs were placed in a wooden box, on a layer of charcoal powder of a few inches in depth, and covered over with a quantity of the same material. Although the box was quite open, no effluvia were ever perceptible, and on examining the bodies of the animals at the end of six months, they were found to be in a very advanced state of decay. The charcoal which surrounded the dogs, on being analysed, was found to contain appreciable quantities of nitric and sulphuric acids and phosphate of lime, and but little ammonia, and scarcely a trace of sulphuretted hydrogen.

It is commonly believed, and it is stated in many chemical works, that charcoal is antiseptic. This is the very reverse of the fact, as shown by the condition of the bodies of animals which have been long buried in charcoal, which are usually in an advanced stage of decay. This opinion has doubtless arisen from the fact that the ordinary evidences of decomposition—viz., disagreeable effluvia, are absent when bodies pass to decay in contact with charcoal, the charcoal absorbing these gases as fast as they are formed.

There is good reason for believing that charcoal is not only an energetic absorber of gases, but that it is also a powerful oxidiser of them, and that its beneficial operation in many cases arises as much from this as from its absorbent properties.

Dr. Stenhouse very properly points out that the oxidizing property of charcoal renders it improper that it should be mixed with manure, since it would destroy some of those products of decomposition upon which the fertilizing power of manure is dependent, and so tend greatly to reduce the value of the manure itself.

"Notwithstanding this obvious objection," writes Dr. Stenhouse, "this is the very process which a company, called the 'Health of Towns Improvement Institute,' established in Dublin, is constantly recommending."

Notwithstanding the absorbent powers of charcoal, some have doubted whether it really is a disinfectant or not. This opinion has arisen probably from imperfect views of its *modus operandi*. Since charcoal not only absorbs and destroys all offensive effluvia, and oxidizes many of the products of decomposition, there is scarcely a

reasonable ground of doubt remaining, that it does really possess the property of a true disinfectant, acting by destroying those lethal organic compounds upon which infection depends.

The sanitary applications of which charcoal is susceptible, are many and important. The following is an enumeration of the principal:—To the construction of respirators, both to warm the air and to disinfect it, as well as to purify the breath; to ventilation, to purify the air; to prevent the escape of various effluvia from graveyards, and from dead and decomposing animal and vegetable matter, wherever or under whatever circumstances they may be met with. Thus it may be used with good effect in coffins, in the wards of hospitals, in dead-houses, in dissecting rooms, in malarious districts, and, lastly, on the field of battle, where the work of disinfection is now left to the dog and the vulture, by which the process is far less efficiently performed.—*From a Review of Dr. Stenhouse's Pamphlet, Lancet, April* 28, 1855, *p.* 436.

163.—CASES OF CHOREA TREATED BY THE CARBONATE OF IRON.

By Dr. Peacock, St. Thomas's Hospital.

[Severe cases of chorea have lately been very common. The following show the superiority of iron to zinc in their treatment:—]

Margaret G., aged 15, was admitted on Feb. 20th, 1855; she had then had chorea for a month, though not very severe, but her extremities and head were in constant motion. After taking an aperient she was directed to have two grains of the sulphate of zinc in an ounce of infusion of valerian three times daily, and this dose was gradually increased by the 17th of March to sixteen grains three times a-day, and she had the tepid shower-bath, first every other day and then every day. The zinc never produced any inconvenience except occasionally a slight feeling of sickliness when first the dose was increased. On the 24th of March no amendment had taken place, the symptoms being on the contrary more severe. The zinc was therefore discontinued, and instead of it a scruple of the saccharine carbonate of iron was taken three times a day, the shower-bath being still continued. On the first visit after this change an obvious improvement was perceptible, and from this time she steadily progressed, and was discharged entirely cured on the 5th of April. When first the iron was taken, an occasional aperient was required, but afterwards the bowels acted regularly without any assistance.

Ann B., aged 10, was admitted into St. Thomas's Hospital, under the care of Dr. Peacock, on the 5th of February. Her mother stated that three months before that time she was observed to have singular motions and twitchings in the lower extremities, and this had continued up to two weeks before her admission into the hospital, when the upper extremities also became affected. On the first of February she had a fall, and, though not much hurt, she was alarmed, and the spasmodic affection became much more marked. When admitted there were incessant movements of the extremities, head, and feet, so that she was altogether incapable of walking, or, indeed, of leaving her bed, or of retaining anything in her hands; she could only protrude the tongue suddenly and with great effort, and after several ineffectual trials, and she as suddenly retracted it. The pupils were very much dilated, and the expression of countenance fatuous. She was directed to take an aperient, and was then prescribed 2 grains of sulphate of zinc in ℥ss. of infusion of valerian, three times daily. At first she improved in some degree under this treatment, but the amendment was not maintained; and on the 20th, when she was taking 6 grains of the sulphate of zinc three times a day, and using the tepid shower-bath every day, she had certainly not advanced, and was, if anything, worse than when the treatment was first commenced. Doses of 10 grains of the saccharine carbonate of iron were, therefore, substituted for the zinc. On the 23rd there was some amendment, and from this time she daily improved, till, at the end of the month, she was nearly well, and when discharged, on the 16th of April, she had been quite well for several days. The pupils had recovered their proper size, and her expression of countenance was intelligent and cheerful. At the commencement of the treatment a small blister was applied to the upper dorsal region, but without any advantage.

Remarks.—These cases are not adduced under the most remote impression that they imply the general superior efficacy of the carbonate of iron over the preparations of zinc. The rule in therapeutics which they chiefly illustrate is the propriety of making a change of remedies when, after a fair trial, the one first selected does not appear to benefit. In no disease is this rule more necessary to be observed than in chorea. Cases must have come under the notice of all physicians in which, after iron had failed, zinc succeeded, and in which, after perhaps both of these had been tried in vain, the ammonio-sulphate of copper, some preparation of arsenic, or perhaps the addition of valerian to the zinc, at once arrested the disease. Our art is fortunate enough to possess at least half a dozen mineral preparations of proven specific power against chorea, but as yet few, if any, trustworthy rules have been established for our guidance in preferring one to the other. A more minute investigation of the disease might be expected to show that the cases, such as these just quoted, in which the sulphate of zinc manifests not the least efficiency, differ in somewhat from those in which it cures like a charm. In our present ignorance, however, we must be content to be guided by the knowledge obtained during the progress of the treatment, and to ring changes on the remedies until the right one is found. Perhaps, however, in virtue of their blood-improving qualities the preparations of iron have a claim to be tried before most of the others, and of those preparations the saccharine carbonate, as used by Dr. Peacock, is probably the best for the purpose.—*Med. Times and Gazette, April* 28, 1855, *p.* 411.

164.—*Formula for the Administration of Elaterium.*—The following prescription, copied from the Pharmacopœia of the London Hospital, is a very convenient one for the administration of elaterium :—

R. Elaterii extract. gr. iss.; pulv. capsici gr. vi.; hydr. chlorid. gr. xij.; ext. gentian. 3 ss.; sacch. fæcis q. s. Ft. pil. xij. Sit dosis pil. i. vel ij.

If needful, the proportion of elaterium may be increased to 2 or 3 grains, according to the wishes of the prescriber. The capsicum is of the greatest use in preventing the distressing nausea which elaterium often causes.—*Med. Times and Gazette, April* 28, 1855, *p.* 412.

165.—ON STRICTURE OF THE RECTUM.

By Jas. Syme, Esq., Prof. of Surgery in the University of Edinburgh.

The rectum is liable to stricture of two kinds; the one a mere simple induration of the coats of the bowel, which is a rare affection; the other a thickening from malignant deposit, which I am sorry to say is very common. Malignant stricture of the rectum has one feature that distinguishes it from malignant diseases generally—viz., that it is often very slow in its progress. Its course is certainly sometimes rapid; but it may go on for twenty or thirty years before causing the death of the patient, as it always does in the end, in spite of remedies, unless he be carried off in some other way. Both of these forms of stricture exist in the same situation—viz., between two or three inches from the anus, where the rectum makes its turn backwards; and it may be said, that whenever stricture exists in the rectum, it is within reach of the finger. There is no rule without exception: and so it is possible that stricture may occur, in rare cases, higher up; but I never met with an instance of it during a practice of thirty years, in which I have had pretty good opportunities for observing this class of diseases. It is quite true that it is often believed that stricture occurs at the distance of four or five inches from the anus; but this depends on a deception. A bougie passed into a healthy rectum goes up pretty easily till it reaches the promontory of the sacrum, when its progress is apt to be arrested; and the medical man, unless aware of this source of fallacy, may be led to suppose that a contraction of the bowel is the cause of obstruction. I might mention many instances of this; but one, which is particularly striking, will suffice. It took place in the case of an old lady, whom I was asked to see by Dr. Begbie; she had been between two and three years under the care of two deservedly-distinguished practitioners of this city, now deceased, on account of supposed stricture of

the rectum. On entering her room, I saw a large tray covered with bougies of every possible size and consistence. On examination, however, I found all the bowel, within reach of my finger, sound; and, therefore, expressed a decided opinion, that no stricture was present. Some months afterwards, the patient died; and Dr. Begbie, and one of the other gentlemen, were present at the examination of the body; in which the rectum, like the rest of the intestinal canal, was found free from contraction. The gentleman expressed great surprise, and said that not less than three hundred hours had been spent in introducing bougies, and how it was that no stricture existed he could not understand; so he proceeded to make an experiment by introducing the bougies in the same way as he had been accustomed to during life, and found, that when it had reached the height of the supposed stricture, it was stopped by the promontory of the sacrum. When you consider the duration of the deception, and the high standing and eminent talent of the gentlemen thus deceived, you will allow that this case proves, in a very striking manner, the risk of being misled by this source of fallacy. But that any man can maintain the existence of a stricture at a still higher point—for example, twelve or thirteen inches up—or, still more, profess to cure it, I say, is what I cannot understand any one capable of, unless he be extremely ignorant, or, still worse, intentionally deceiving the patient. Unhappily, however, it has long been a practice with specialists, in this class of diseases, to employ treatment directed to strictures alleged to exist in such situations. I will now show you a patient, in whom this has been done, although I firmly believe he never had anything wrong with his rectum.

[The patient, W. C., aged forty-two, residing at York, was brought in, and stated that ten years ago he suffered from some bowel complaint, accompanied with a mucous discharge from the rectum, and that he was then sent up to London to consult a certain rectum doctor, who told him that he had a stricture of the bowel, and passed very long bougies for a distance of about thirteen inches. He remained nine weeks under his care, the bougies being introduced about four times a week. He was, however, no better, and returned to York with some medicine and a bougie which he had purchased in London by the doctor's direction, and which he has passed himself two or three times a week during the ten years that have since elapsed. The man having left the theatre, Mr. Syme said:—]

Had I told you such a tale as this myself, you might perhaps have supposed me to be romancing; but you have now heard it from the patient's own lips. But you may say this is an old story, and that the light has perhaps shone in upon this practitioner during the last ten years, and shown him the error of his ways; but not long ago, I was consulted by a gentleman from England, who had been just before under the charge of the same person, and treated in the same way, although his rectum was as sound as any man's in Edinburgh. Such practices should be made fully known to the public as well as to the profession. It may be difficult to do this in London, from a natural unwillingness to incur the hostility of vindictive neighbours, but not being influenced by any such fear, I am resolved to do what may be in my power.—*Lancet, April* 7, 1855, *p.* 356.

166.—ON THE TREATMENT OF VARIX.

By Henry T. Chapman, Esq.

[The modifications of treatment by bandaging are considered under different heads.]

1. *Curative Treatment of Simple Uniform Varix.*—Whether it arise primarily or consecutively, the tendency to dilatation may readily be counteracted at an early period by equable and steady support in conjunction with cold and astringent lotions, although time will still be needed to restore the full tone and elasticity of its coats in order to guard against a relapse. Unfortunately the surgeon is rarely consulted until the dilatation has become confirmed, and the duration of the treatment will, of course, depend very much upon the length of time it has existed.

Plaster strapping, as suggested by Mr. Hodgson, applied according to Mr. Scott's method from the toes upward, is not ill-suited to fulfil the end we must aim at,—that is to say, the closure of the vein to the circulation for a longer or shorter period,—if the requisite amount of pressure by such agency could be borne by the

patient sufficiently long. After a while, however, the skin is sure to become heated, inflamed, and excoriated, and is sometimes deeply cut by the edges of the straps, whatever may be the plaster employed or the material on which it is spread. I have, accordingly, for some years entirely abandoned its use in the treatment both of ulcers and of varicose veins in favour of the following very simply constructed bandage, which possesses all the advantages of plaster strapping without its inconveniences:—

Placing the patient on a low seat, and elevating the foot until the veins empty themselves by the gravitation of the blood towards the trunk, I apply wet straps of linen or calico precisely in the same manner as Mr. Scott applied adhesive plaster. The bands for this purpose should be from two to three inches in width, and from twelve to sixteen in length, stout enough to prevent them tearing easily, but not too thick. Adjusting the middle of one of the shorter and narrower of these bands, previously soaked in water, just above the heel, the two ends are brought forward over the ankles, drawn tightly, and crossed upon the instep. The middle of another is placed beneath the sole of the foot, its extremities brought up firmly over the instep, and laid down smoothly upon the other. A third is applied, like the first, from behind forwards, but a little higher; and thus, ascending the leg, the process is repeated with the rest of the bands, each one in succession overlapping the upper half of that below it, until the limb is firmly and evenly cased to the knee. Over the straps a calico roller is carefully put on, the greatest attention being paid to its equable adjustment, so that the pressure may be uniformly distributed over the entire limb.

Through its texture the course of the varicose veins should be moistened several times daily with cold water, diluted Goulard lotion, or solutions of alum, sulphate of zinc, or chloride of lime. Whenever the bandage is taken off for renewal, the dilated veins should be subjected to brisk friction upwards with the hand for some time, and a douche of cold water poured over them, the foot being kept in an elevated position until it is re-applied. In winter, and when patients are elderly or delicate, the free application of cold water or lotions is not always safe, and this part of the treatment must of course be left to the discretion of the surgeon. It is better under such circumstances to confine the astringent influence of cold strictly to the vessels affected.

No one who has not tried this mode of bandaging can form an estimate of the power of adhesion possessed by the wet strapping, or the amount of steady, even support it is capable of affording. In these respects—as I have elsewhere stated when advocating their employment in the treatment of ulcers and cutaneous eruptions on the leg—their action is in no degree inferior to that of plaster, at the same time that the softness of the material allows of its closer adaptation to the inequalities of the limb, and precludes all risk of cutting or excoriation, accidents so common when plaster strapping is made use of that its contact could never be borne long enough to produce any permanently good effect.

The wet straps are especially serviceable in compressing the bulk of the soft parts before the application of the roller, and they give the latter a much better hold than it can take of the bare skin, or even of plaster, thus materially diminishing the chance of the roller slipping. Their greater permeability, again, admits of the more efficient use of lotions, and their cheapness, when compared with diachylon or any other kind of plaster, is no slight recommendation. Unless displacement of its turns should occur earlier, or uneasiness be felt at any point, if the skin be sound the bandage need not be renewed before the fourth, fifth, or sixth day of its application; indeed I have frequently left it undisturbed for a fortnight or three weeks.

In cold weather a flannel roller is preferable to one of calico in all respects. It is quite as elastic as Churton's cotton-web bandage, supports the veins more firmly and comfortably, and is not liable to rope. In summer, or with patients who find both flannel and calico heating, I employ rollers of coarse mull muslin, which are light and cool, and give as much support as calico.

Although an elastic stocking employed alone may sometimes arrest the progress of varix in its earlier stages, it can never be regarded as a curative measure. In fact the heating properties of the material rather augment the tendency to dilatation; and as the ordinary pressure exercised by it but partially empties the veins of blood, lateral expansion in a saccular or serpentine form will often go on beneath the stock-

ing. When combined, however, with wet strapping its efficiency is very considerably enhanced. But in order to derive the full advantage from this combination, the dimensions of the leg for the stocking must be taken after the wet strapping has been worn for some days, and the limb has been reduced a trifle below its natural size. Patients, accordingly, who are unable, or who will not take the trouble to bandage a varicose leg in the manner set forth above, may adopt the alternative of an elastic stocking drawn on over a casing of wet strapping.

2. *Curative Treatment of Saccular, Vesicular, and Serpentine Varix.*—In cases of long standing and more aggravated character, where great tortuosity, saccular expansions of the main trunk and large branches,—numerous clusters often sprouting from these latter,—and vesicular enlargements of the cutaneous veins exist, the support distributed over the general surface of the limb by the wet straps and bandage alone, not entirely emptying the diseased veins, will do little more than palliate the complaint.

To direct the pressure, therefore, effectually upon those points where it is most called for, I have recourse to compresses of lint or spongio-piline in combination with the bandage, large enough to cover the chief clusters and each of the sacculated portions of the vein separately, and thick enough to close their channels. These compresses, moistened in cold water or astringent lotions, are placed on the most salient dilatations, and the straps and bandage carried over them. Many of the varicosities rapidly disappear; others require the maintenance of the compression for a longer period; and in serpentine varix, especially, the process of reduction is occasionally very tedious. In saccular expansion and mulberry-like vesicular clusters in the skin I seldom rely for the completion of the cure on the mere diversion of the circulation into other channels.

Taking advantage of the known tendency of the lining membrane of the veins to adhesive inflammation, I endeavour to obliterate all lateral pouches and vesicular protrusions through the skin by submitting them to a high degree of pressure, so as to maintain their opposite surfaces forcibly in contact; no very difficult matter when they happen to be situated over a bone; but when they lie upon the soft parts of a calf, as is most frequently the case, they recede beneath the pressure of the ordinary compresses, and require some more unyielding substance to close their cavities entirely. To meet this difficulty, I make use of the leather cones with which billiard cues are tipped, or smooth wooden or ivory buttons, choosing a size corresponding with that of the varicose pouch; and for the blue cutaneous vesicles, glass or porcelain shirt studs. The former are readily retained *in situ* by lint compresses and the bandage; the latter I insert into a small slit or button-hole in one of the wet straps, and carefully adjust it to the pit in the integument through which the vesicle has protruded; and by their aid I generally succeed, ere long, in effacing both large and small cells.

Where the saphena in the thigh, as well as the branches below the knee, are diseased, the pressure from within upon the walls of the latter will be more than doubled by the superadded weight of the column of blood in the former. Until this dilating force from above be neutralized, no rational expectation of reducing the enlarged veins to their natural size can be entertained. At the same time, therefore, that the several remedial measures described above are brought to bear upon the yielding branches, the dilated trunk ought also to be effectually supported. As, however, it is not possible, by any form of bandage, to exercise the same degree of equable compression upon the veins of the thigh as on those of the leg, I never attempt to carry the wet straps and roller above the knee; but, leaving them to counteract all undue accumulation below, I trust to Mr. Startin's elastic riband, wound spirally round the thigh from the knee to the groin, to divide the column of blood in the dilated trunk, and take off its downward pressure upon the branches; and I have more than once had occasion to observe that by the time the veins of the leg are restored to a healthy condition, the dilatation of those of the thigh has likewise materially diminished.—*Med. Times and Gazette, May* 19, 1855, *p.* 486.

167.—*Employment of the Chlorate of Potash as a Topical Application.*—The chlorate was used in solution in the proportion of from one drachm and a half to three

drachms to one pint of water. He (Mr. Moore) had found it very useful in cases of indolent ulcer and phagedæna, in cleansing cancerous sores, and as an application to the mucous membrane of the nose, mouth, and tongue in cases of ozena and secondary ulceration. The author suggested that the beneficial effects of the application were probably due to its setting free oxygen, and proposed its use in some forms of dysentery, with affections of the lower bowel.

Dr. Mayo said he believed Mr. Stanley used the same remedy (scruple doses every four hours) for phagedenic syphilitic ulceration.

Mr. C. Hawkins generally combined it in such cases with tincture of myrrh. The remedy was very useful in cases of cancer, in removing the fœtor, independently of its effect upon the sore itself. Scruple doses he thought scarcely sufficient to administer internally.—*Med. Times and Gazette, May* 19, 1855, *p.* 504.

168.—*Treatment of Piles.*—In the out-patients' room at St. Bartholomew's, the other day, Mr. Wormald ordered for a middle-aged man, of spare habit and yellow complexion, long the subject of piles, the following medicines:—A pill consisting of two grains and a half of blue pill every night, a scruple of extract of taraxacum, dissolved in water, three times daily, and an injection of two grains of sulphate of iron to the ounce of water, to be used every morning. For the relief of the irritation caused by the tumours, and by the tendency to prolapse of the rectum which attended them, the man was directed to place just within the bowel after every motion a small plug of cotton wool, well oiled.—*Med. Times and Gazette, May* 19, 1855, *p.* 491.

169.—*On Colchicum in Neuralgia.*—In the out-patients' room at St. Thomas's Hospital, Dr. Peacock has recently had frequent occasion to remark upon the examples of neuralgia occurring seemingly in connexion with the rheumatic diathesis. In these the treatment by anti-periodics has always failed, and it has been requisite to employ either colchicum or iodide of potassium. In several quinine had been largely tried prior to admission, and without success, while the rapidity with which benefit ensued when the change in remedy was made, appeared to show conclusively the real nature of the disease. It is important to bear in mind, in the management of these so-called nervous diseases, that their true pathology may often be humoral, and that resort to eliminants is almost as frequently necessary as to nervine tonics and antiperiodics. An introductory treatment by purgatives, diuretics, or the specifics against gout and rheumatism, will, in fact, in the generality of cases, secure a much better action for the quinine afterwards. The employment, in the first place, of one or other of these, is, indeed, a matter almost of routine with many practical physicians.—*Med. Times and Gazette, May* 19, 1855, *p.* 490.

170.—*Iodine in Fibrous Tumours of the Uterus.*—Dr. West almost invariably orders for those of his patients at St. Bartholomew's who are the subjects of fibrous tumours of the uterus, a long course of one or other of the preparations of iodine. The following is the prescription which was ordered for a middle-aged woman, who applied with that disease on Saturday last:—

Potassii iodidi gr. i.; syrupi ferri iodidi ℥xx.; aquæ carui ℥ss.

Ter die sumend.

Dr. West remarked at the time, that were the patient one in the higher ranks of life, she would be just the one likely to be benefited by being sent to drink the Kreuznach waters. In common with Dr. Rigby and other physicians, Dr. West entertains a high opinion of the value of the iodides in procuring the diminution of these tumours.—*Med. Times and Gazette, April* 28, 1855, *p.* 412.

171.—ON LARYNGOTOMY.

By John Erichsen, Esq.

[In commenting on a rapid case of laryngitis which Mr. Erichsen attended, and in which laryngotomy was performed, the following practical remarks are made:—]

This case illustrates the rapid progress of acute laryngitis, and also the importance of effecting a timely opening in the air-tube, in order to establish an aperture through which respiration can be carried on until the obstruction about the larynx has subsided. A question of considerable importance, however, arises as to the part of the windpipe that should be opened; whether, in fact, laryngotomy or tracheotomy should be performed. In my opinion, laryngotomy is the operation that should always be preferred, and this opinion is based on the following reasons:—

1. That, as in laryngotomy, the air-tube is always opened below the seat of obstruction, there can be no necessity to make an aperture further from the seat of disease. In laryngitis, whether that affection assume the acute or chronic character, the obstruction in breathing is in great measure mechanical, and dependent upon the infiltration of the submucous cellular tissue of the larynx, and partly of the large plane of this tissue, which lies behind the box of the larynx, and which, by expanding as it were, into the pharynx, obstructs deglutition; and afterwards, by the extension of this swelling and infiltration to the lips of the glottis and the interior of the larynx, causing an impediment to the entrance of air into the bronchi. But, as has been pointed out by Mr. Prescott Hewett, this submucous cellular tissue terminates at the true vocal cords, where the mucous membrane becomes directly applied to the subjacent fibrous structures, the swelling and consequent mechanical impediment is confined to the limits of the thyroid cartilage, and any opening made below this will clear the lowest limit of the disease which is always accurately and almost mathematically bounded below by the vocal cords. Hence an aperture in the crico-thyroid membrane is quite as effectual as one in the trachea.

2. Laryngotomy is a far safer operation than tracheotomy. On this point I need scarcely dwell: a glance at the anatomy of the parts concerned will be sufficient to establish it. The crico-thyroid membrane is nearly subcutaneous, and no parts of importance can be wounded in opening it if we except the small inferior laryngeal artery, which crosses it, and which might be cut across, but from which I have never seen any trouble arise. The trachea, on the contrary, is not only deeply seated, but covered by a large plexus of bloodvessels, which, when rendered turgid by the asphyxiated condition that exists when these operations are required, pours out a large quantity of dark blood, and thus seriously embarrasses and delays the surgeon at a time when the life of the patient depends on the speedy admission of air to his lungs.

3. Laryngotomy can be much more quickly performed than tracheotomy. This I look upon as an inestimable advantage in many of the cases requiring operation, a few seconds, more or less, being sufficient to turn the balance either in favour of life or of death. The rapidity with which laryngeal obstructions—partly mechanical and partly from spasm—set in, is sometimes so great, more particularly when an acute inflammation supervenes on chronic disease of the larynx, that life may be extinguished before the surgeon has time to open the windpipe, if he endeavours to do so by tracheotomy. In extreme cases, such as where the lungs have become slowly engorged, the action of the heart is already enfeebled, and a sudden spasm occurring at the glottis, will at once place the patient beyond recovery. But even though life appears for the moment extinct, it is the imperative duty of the surgeon to open the air-passages as speedily as possible, and to endeavour, by means of artificial respiration, to recall the flickering spark; and it is impossible to experience a greater satisfaction in the exercise of our profession, or to witness a greater triumph of art, than in thus successfully snatching a patient away from the very jaws of death.—*Lancet*, *April* 28, 1855, *p.* 427.

172.—*New Forceps for Extracting Bullets.*—At a meeting of the Medical Society of London, Mr. Lee exhibited a new pair of Forceps, invented by Philp and Whicker, for the extraction of bullets. The improvement consisted simply in the forceps

being formed by separate blades, on the plan of the midwifery forceps. By this contrivance less space was required for grasping the bullet, as the blades could be passed one at a time, and subsequently locked together.—*Lancet, Feb.* 10, 1855, *p.* 456.

173.—*Nævus treated by Ligature.*—Mr. SPENCER WELLS says, if a nævus be small, a ligature is sufficient; but if large, the knot should be tied over a piece of bougie, which can be twisted each day after the operation, until the thread cuts its way through the base of the nævus.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 460.

A SYNOPSIS.

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOREGOING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

DISEASES AFFECTING THE SYSTEM GENERALLY.

DROPSY.—Don't be afraid of giving diuretics in renal dropsy, as most practitioners are. You may give digitalis, squill, and bitartrate of potash, either singly or combined, without materially increasing the irritation of the kidneys. You may give the 75th part of a grain of digitaline three times a day. This dose may seem small, but the tenth of a grain will kill a little dog. (Dr. R. Christison, p. 101.)

GOUT.—In all cases of genuine gout, uric acid exists in the blood in abnormal quantities. In rheumatism this is not the case. Test the thing as follows, by what is called the "uric acid thread experiment:" Immerse a single fibre or two of unwashed huckaback linen in a drachm or so of the serum of the blood, previously acidulated by a few drops of acetic acid, and put into a watch-glass. Put it aside, in order to allow spontaneous evaporation, when, if there be an abnormal quantity of uric acid present, the fibres will be covered with the crystals of this body. (Dr. A. B. Garrod, p. 44.)

Envelope the parts affected in piline saturated with the tincture of piper methysticum, and take this medicine in the following form: ℞. Tinct. pip. meth. ℥ss.; aquæ ℥vj. Take a fourth part of this mixture every four hours. (Dr. E. W. Pritchard, p. 45.)

Gout and Rheumatism.—Attend to *all* the excretions, and not to those of one organ exclusively. Unless fever be present, give bark and hydrochlorate of ammonia, which stimulate all the excretory glands. Try to improve the general health by change of air and exercise, steel, cod-liver oil, and a variety of other means, before giving colchicum, guaiacum, and other medicines of this kind.

Be careful, especially in puerperal cases, to remember that a joint may be affected by a *gonorrhœal taint*, and may closely resemble pure gout or rheumatism. Ask if the child has had mild or severe purulent ophthalmia: if this has been the case, it is very probable that the joint affection of the mother is gonorrhœal. (Dr. Fuller, p. 42.)

RHEUMATISM.—If the skin be hot and pulse strong, begin with a mixture of sulphate of magnesia and tartarized antimony until the skin becomes relaxed and the pulse reduced; then give the Rochelle salt in drachm doses every two or three hours in the day-time, until the urine becomes alkaline, when it may be gradually suspended. Externally apply a lotion of carb. potass. ℥j., with opium ℥ij. to the pint of water. The urine will become alkaline in about five or seven days, and from that time the case will generally improve. (Dr. Fuller, p. 43.)

Give two-scruple doses of bicarbonate of potash, well diluted, every two hours day and night, till the patient has been free from the affection for two or three days. (Dr. A. B. Garrod, p. 46.)

Lemon-juice is still relied upon extensively, but since it became known that it contained some of the salts of potash, these have been largely prescribed in the acetate, tartrate, or nitrate of potash. Give half-a-drachm of acetate of potash, ten grains of the nitrate of potash, and ten minims of vinum opii diluted with barley-water, two or three times a day. (Dr. Barlow, p. 46.)

YELLOW FEVER.—Give fifteen minims of spirit of turpentine every three or four hours, combined with a little nitric ether and camphor mixture. As soon as a remission takes place give quinine, and a more nutritious diet. (Mr. J. Laird, p. 40.)

AFFECTIONS OF THE NERVOUS SYSTEM.

APOPLEXY.—Don't be led away by the popular opinion that in all cases of apoplexy we must bleed. If the patient be full and plethoric, with a strong heart, you may bleed; but if he have been intemperate, or have organic disease of the heart or arteries, or of a gouty or rheumatic disposition, do not bleed. In the majority of cases bleeding is improper; they may be better treated by purging, shaving the head, keeping it cool, and perhaps blistering it. (Dr. R. B. Todd, p. 64.)

CHOREA.—After an aperient, give two grains of the sulphate of zinc in an ounce of infusion of valerian, three times daily; the dose may be increased to sixteen grains, and the tepid shower-bath used daily. If this does not relieve, give ℈ scruple of the saccharine carbonate of iron instead, and if both these fail, try the ammonio-sulphate of copper, or some preparation of arsenic; if one fails, you will generally succeed with another. (Dr. Peacock, p. 306.)

CONVULSIONS *in Children*.—Use warm water injections to a great extent, even in infants, (twenty-four ounces of warm water have been injected into an infant.) The convulsions will often cease on the injection of the first few ounces, but may return again and again, till the bowels have been well washed out. (Dr. R. Molloy, p. 60.)

DELIRIUM TREMENS.—You may give chloroform with safety in those cases where stimulants have been suddenly left off. First, give half an ounce of whiskey in the yolk of an egg, with a little sugar and nutmeg, and when well mixed, and just before being taken, add one drachm of chloroform. Give also strong beef-tea, with a chop, and a little porter, according to the case. If the first dose of chloroform does not produce sleep, give a second, a third, and a fourth, and even a fifth and sixth at intervals of an hour or so, according to the violence of the case. The great point is to keep the patient *well up* with food, such as egg and whiskey, or broth, beef-tea, chop and porter, &c.,—all these to be used judiciously. You may begin to give the chloroform in half-drachm doses every three or four hours, till you feel your way with it, and then increase it, and give it if necessary every hour. (Mr. J. Tufnell, p. 70.)

You may administer chloroform by the stomach with safety, but not by inhalation. You may begin with half a drachm in camphor mixture, with a little liquorice powder, and increase this dose according to its effects. One patient took by mistake four and a half drachms with excellent effect, although it would be imprudent to give such a dose. (Dr. Corrigan, p. 260.)

EPILEPSY.—Give the cotyledon umbilicus as in the following formula: ℞. Liq. cotyl. umbil. ʒj., ter die sumend. At the same time give purgatives occasionally, and preparations of iron. (Dr. E. H. Sieveking, p. 58.)

Epilepsy and Convulsive Diseases generally.—Avoid all the usual excitements; avoid low postures, deep sleep, and tight collars or cravats. Relieve acidity of the stomach with bicarbonate of potass, also scybala in the colon by enemata. Sometimes a good emetic, as half a drachm of ipecacuanha, will check a convulsion. It is possible that minute doses of strychnia may do good in the undue spinal excitability of epilepsy, in the same way as a little alcohol does in the undue cerebral excitement of delirium tremens. (Dr. Marshall Hall, p. 58.)

NEURALGIA.—Apply aconitine ointment over the course of the nerve, consisting of two grains of aconitine to an ounce of lard. Be careful, however, how you trust a patient to use this ointment continuously, as the drug is cumulative, and the effect may become suddenly powerful. (Mr. J. Hilton, p. 239.)

A liniment, composed of equal parts of chloroform and olive oil, will act most beneficially. (Dr. Corrigan, p. 261.)

If in connexion with the rheumatic diathesis, quinine will almost always fail; you must give colchicum or iodide of potassium, rapid benefit generally follows. (Dr. Peacock, p. 311.)

Sciatica.—Apply a blister, the size of a crown piece, over the seat of pain. Remove the cuticle, and sprinkle over the raw surface a powder composed of one grain of acetate of morphia, and a little white sugar. Repeat this dressing for six successive days, keeping the surface raw if necessary by cantharides or savine ointment. This will do for a mild case, but if the pain goes elsewhere, follow it with the same treatment. (Dr. P. Blakiston, p. 69.)

Tic Douloureux.—If a severe case, apply an ointment composed of one grain of pure aconitina to four drachms of lard. (Dr. A. N. Holmes, from Dr. Watson's Lectures.)

Apply chloroform locally, thus:—Take a piece of lint and saturate it with chloroform, then place it under a wine-glass, and let it be moved about over the seat of pain as long as the patient can bear it. (*Lancet*, Feb. 10, 1855, p. 173.)

AFFECTIONS OF THE CIRCULATORY ORGANS.

Aneurism, *Popliteal.—Treatment by Ligature.*—"Take if you please half-an-hour, or a whole forenoon about it, but do not pass the needle till the artery has been sufficiently separated from the contiguous structures to enable you to avoid all risk of injury to the vein, which is the great source of danger to be dreaded. The femoral vein is more intimately connected with the artery than most veins. If, therefore, you do not separate the one from the other *with the greatest nicety*, you will very likely either pierce or bruise the vein, and Mr. Guthrie says, that the artery alone might be obstructed without death of the limb, but that this was the invariable result if both artery and vein were obstructed together. The *femoral* artery varies in its origin from the common femoral, and it is very important that the ligature should be considerably below its lowest origin. *Therefore, always tie low enough*, i.e., where the artery has become overlapped by the sartorius muscle. If, however, you go below this muscle, you will be too near the aneurism. (Prof. Syme, p. 143.)

Treatment by Compression.—It is not necessary to cut off the entire flow of blood to the tumour, neither is it necessary to continue the pressure always in one spot, but it may be shifted from one place to another. In many cases, it is only necessary to *modify* the flow of blood through the main artery. Carté's circular compressor is a good instrument. It has an ingenious addition to the common screw, in form of bands of caoutchouc, whereby a certain resiliency is acquired, which keeps up effectual pressure and modifies the screw. (Prof. Fergusson, p. 145.)

Elastic-pressure is the latest improvement in the treatment by compression. The screw apparatus seems to have been discarded in popliteal aneurism. The elastic spring seems to be quite enough to restrain the current of blood, which is sufficient. (Ed. of 'Lancet,' p. 147.)

Aneurismal Tumour (*Nævus*).—Puncture the tumour, and immediately plunge into it the canula of a glass syringe filled with acetate of lead, and inject eight or ten drops or more. Then withdraw the syringe, and apply the finger to the wound for about a minute. (M. Lassana, p. 148.)

Aneurism and Varix.—Make use of galvano-puncture, acupuncture with zinc, or with needles covered with a layer of this metal instead of steel. Insert a certain number of needles into the aneurism or varix, and connect them with the positive pole of a galvanic battery. The negative pole should be supplied with a plate of platinum, which must be put upon the skin adjacent to the aneurism, after having augmented the conductibility of the epidermis by moistening it with a saline or acidulated solution. (Dr. Steinlein, p. 151.)

Hemorrhage.—A new hæmostatic agent is benzoic acid, dissolved in alum. If you cannot arrest hemorrhage by compression, and where it is not necessary to ligature the artery, soak a little lint in Monsel's preparation of benzoic acid and alum, and place it upon the bleeding surface. Professor Hannon's preparation is "Benzoic acid one part, alum three parts, ergotin three parts, water twenty-five parts—the whole to be boiled thirty minutes with constant stirring, and renewal of water. Then it must be evaporated to the consistence of an extract. A layer spread over the bleeding surface produces instant coagulation. Compression is not necessary." (M. Monsel and Prof. Hannon, p. 151.)

VARIX.—When the limb is strapped with plaster, from the toes upwards, according to the usual plan, after a while the skin is sure to become heated, inflamed, and excoriated; but, if straps of wet calico be applied in the same manner, each one in succession overlapping the upper half of the one below it, until the limb is firmly and evenly encased to the knee, over which you must apply a roller, with the greatest attention to its equable adjustment, you will have all the advantages of the plaster strapping without its inconveniences. If necessary, you will also be able to apply lotions to the veins, without the necessity of removal of the straps; when they are removed, the dilated veins should be subjected to brisk friction. The power of adhesion by the wet strapping is surprising, and in no way inferior to that of plaster; at the same time it is much softer and more comfortable. In cold weather a flannel roller is preferable, and in summer a roller of coarse muslin is much preferable to calico. If the varix is sacculated and expanded, you must apply compresses of lint or spongio-piline, moistened with astringent lotion, or cold water, to the sacculated portions of the vein, and carry the bandage over them. If there are lateral branches, you must endeavour to obliterate them by submitting them to a high degree of pressure, so as to maintain their opposite surfaces in contact, which will induce inflammation; this is easy over a bone, but if upon the soft parts of the calf, you must use leather cones, or smooth wooden buttons, inserted into a small slit or button-hole in one of the wet straps, to keep it in situ. At the same time, the dilated trunk must be supported; this is best done by Mr. Startin's elastic riband, wound spirally round the thigh, from the knee to the groin. (Mr. H. T. Chapman, pp. 149, 309.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

℞ BRONCHOCELE.—Endeavor to induce and maintain, for a time, inflammatory action in the gland, which may be followed by adhesive and constructive inflammation from effusion and infiltration, ultimately giving rise to absorption from obliteration of nutrient vessels. Select the most prominent part of the tumour, and apply the potassa fusa over a space the size of a florin. In about twelve days the slough will separate,—then apply the caustic again and again, in succession, until the thyroid is exposed by the gradual destruction of the intervening tissues. Next, pass a blunt-pointed grooved director freely into the substance of the enlargement, in various directions, which will give rise to considerable constitutional disturbance; wait then patiently for the result. (Mr. W. Turner, p. 224.)

CATARRH.—Heat a piece of metal in a spirit lamp, and sprinkle a few grains of powdered opium upon it. Let the patient hold his head in the fumes, and make a few forced inhalations. [This will relieve the great pain in the frontal sinuses, which sometimes exists; but we doubt its efficacy in relieving the febrile affection.—Ed.] (Dr. Lombard, p. 88.)

EMPYEMA.—You may inject a solution of iodine into the pleural cavity. The following formula was successful:—Iodinii ʒj.; potassi iodid. ʒij.; spirit. rectificat. ʒj.; aquæ ʒv. M. Let this injection remain in the pleural cavity a short time, moving the patient about so as to bring it into contact with all the pleural surface, and then let out as much as will flow out. The greater portion, however, will remain in the cavity. (Mr. J. Windsor, p. 85.)

LARYNGITIS.—Mix three grains of the solid nitrate of silver with one drachm of sugar. Place the powder in a steel pen, which is itself firmly inserted in a quill open at both ends. This little apparatus is then put into the mouth, so that the end of the steel pen shall be on the root of the tongue, then the lips are to be closed round the quill, and the patient is to respire forcibly. By a little practice, the patient will be able to inhale small particles into his windpipe. (Dr. Ebert, p. 84.)

PHTHISIS.—Amongst other things, try a new preparation composed of cod-liver oil and quina, this being, in fact, a solution of quina in cod-liver oil. (Mr. Bastick, p. 91.)

In cases of circumscribed induration, administer iodine in every possible way:—1. Internally in the form of solution, and if necessary, by enemata. 2. In the form of vapour, by inhalation, once, twice, or thrice a day. 3. In the form of ointment, by friction over the diseased parts. 4. By iodine baths. (M. Piorry, p. 84.)

Instead of blistering too much, or as a change, paint the chest with a solution of iodine and iodide of potassium, one part of each, in two parts of rectified spirit. (p. 84.)

Irritable Stomach in.—Give hydrocyanic acid and trisnitrate of bismuth, as in the following formula:—Hydrocyanic acid three minims; trisnitrate of bismuth ten grains. Make this into a draught, with mucilage and green mint tea, and give it three times a day. (Dr. Peacock and Dr. Risdon Bennett, p. 84.)

Night Sweats of.—The oxide of zinc and hyosciamus pill (mentioned in our last volume) is perhaps one of the best medicines to check night-perspirations; also, a mixture of tannin, nitric acid and lupulin.

Purging.—Give a mixture of decoct. hæmatoxyli and aqua calcis, which sometimes succeeds when, perhaps, bismuth has failed. (p. 84.)

Pneumonia, *Chronic.*—Counter-irritation is of the greatest benefit. Apply one or two blisters, and follow these by Croton oil liniment, or iodine, or a combination of both. If there be no tubercular tendency, you may employ mercury carefully combined with a more tonic plan of treatment. (Dr. R. P. Cotton, p. 75.)

Typhoid.—Take care how you confound pneumonia occurring in the course of typhus, with a pure inflammatory attack. The symptoms may easily lead you astray, but to treat a case of typhoid pneumonia on the antiphlogistic plan would probably be destructive. Too great weight is placed on physical signs in typhoid pneumonia. The successive signs of crepitus, dulness, cessation of vesicular breathing, and its replacement by bronchial respiration, are too often mistaken for the effects of inflammation, whereas the same signs are seen in typhus. They are, in fact, often the symptoms of typhus, and of all other forms of fever, whether these be variola, erysipelas, purulent poisoning of the blood, glanders, scarlatina, or measles. One of the greatest modern improvements of medical practice consists in treating these cases on the plan recommended by Dr. Stokes. Look attentively to the antecedents of a case of this kind when called upon to treat it; examine its history. The editor has been guided by one simple index in most of these cases—viz., the brown tongue. If the tongue be ever so slightly brown at the back, take care how you deplete; on the contrary, begin in good time with broth, ammonia, and wine or good brandy. Don't be afraid of the physical signs of pneumonia, if the history of the case and the tongue indicate coming typhus. It is truly surprising how these physical signs vanish by the timely exhibition of stimulants. The blood is decomposing, the colouring matter is acting on the tongue, the stimulating properties are disappearing, and the functions of life will soon cease, unless you compensate for a time for this loss of stimulation, by an artificial one, till the poison has been exhausted or eliminated. These views are also fully supported in some of Dr. Todd's late lectures. (Dr. Stokes, p. 17-29.)

Tracheotomy.—Before opening the windpipe, always introduce a hook into one side, so as to keep the part steady. The opening is then made much more easily, and the orifice kept open and prominent for the introduction of the canula. (p. 226.)

AFFECTIONS OF THE DIGESTIVE ORGANS.

Cholera.—The saline injection in the worst cases of collapse has the certain effect of *prolonging* life, if it does no more. In some cases it has proved the means of recovery. With each pint of water mix 64 grains of chloride of sodium, 64 grains of carbonate of soda, and 32 grains of chlorate of potash—about five pints may be prepared, and to the five pints add half a grain of iodine. The whole five pints may be injected if necessary. (Mr. H. Ancell, p. 271.)

Dr. Richardson suggests that, instead of injecting a large quantity of fluid at once into the vein, a long narrow syphon tube should be fixed in the vein with the other end immersed in saline solution, in order that "the suction power of the circulating organ may gradually take in from the reservoir the required element." His objection to Mr. Ancell's plan is, that the heart may become exhausted by the sudden over-distension. This objection, however, seems to us to be unnecessary, seeing that the solution may be injected as slowly and gradually as the operator sees fit. (Dr. B. W. Richardson, p. 265.)

Rouse the sunken innervation of the ganglionic nervous system, and prevent its deeper prostration. Valerianic acid exercises a directly stimulating action on the abdominal ganglia, as ammonia does on the nervous centres and the whole organism. Thus valerianate of ammonia is one of the best stimulants, as it rapidly enters the circulating fluid. It must be given as early as possible. From 3 to 6 scruples will generally subdue the disease. The following formula will answer:—℞. Valerianat. ammon. ℈j.; aquæ destill. ℥iij.; syrup. simpl. ℥ss. Give a tablespoonful of this mixture every quarter of an hour, and when reaction sets in, give it seldomer; at the same time, rub the skin with hot flannels. (Dr. Ottinger, p. 276.)

Colic, *Painters'*.—Give half a drachm of chloroform every two or three hours, and increase this to a drachm, if necessary. It may be given in any liquid, such as water. (Dr. Corrigan, p. 93.)

Dyspepsia, *Atonic*.—When accompanied by acidity and foul eructations give chloride of calcium, liquor calcii chloridii, one drachm three times a day, in a little water. (Dr. A. Leared, p. 260.)

Fistula.—Mr. Partridge uses a probe-pointed bistoury with movable sheath made of silver, which slides off to any extent when the instrument is introduced, thus offering a cutting edge of greater or less extent, according to circumstances. (p. 167.)

Fistula-in-Ano.—Wherever a fistula has existed six weeks or two months, you must suppose that an internal opening exists. The tract that leads to it may be tortuous, but you must search for it again and again. You may introduce a bent probe up the fistula, and bring it out at the anus, and then cut out the probe, if you like, with the bistoury. The intervening parts are generally very superficial. (Prof. Syme, p. 156.)

Hernia.—The opinion of Sir Astley Cooper, that the inguinal canal is endowed with muscular contraction, and is consequently influenced by tobacco, chloroform, tartar emetic, the warm bath, &c., is probably erroneous. The canal is altogether passive, and the obstacle to the return of a hernia is generally its size. *Ice*, therefore, is a most valuable remedy. (Mr. Clark, p. 155.)

Make a small incision over the inguinal canal, and introduce a fine trocar into the sac. Inject a small quantity of the tincture of iodine, and close the wound by means of the twisted suture. When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous, it may be punctured at once with a trocar without previously making an incision. (M. Jobert, p. 153.)

Strangulated.—Never handle an incarcerated or strangulated hernia too much. In seven out of ten cases it will probably be found that omentum and not intestine is the chief constituent of the tumour, which you cannot get back by rough usage. When you have fairly tried the taxis, the warm bath, or ice, and fail, you must at once use chloroform and have recourse to the knife. (Mr. Stanley, p. 154.)

The cough-impulse may be a valuable diagnostic sign. If there be no effect on the hernia by coughing, you may suspect that the stricture is tight. If impulse be felt in the upper part of the neck of the tumour, and not in other parts, the seat of stricture may be accurately determined, being just below the spot where the impulse is felt. (Mr. J. Luke, p. 155.)

Prolapsus Ani.—Smear the whole surface of the protruded bowel with solid nitrate of silver, and then return it. Repeat this once a week or fortnight. This need not be confined to simple prolapsion, but may be used in cases of hemorrhoidal congestion and thickening of the mucous membrane about the verge of the anus. (Mr. E. A. Lloyd, p. 158.)

In Children.—If the child be about two years old, apply a blister to the cleft between the nates, and dress the blistered surface with 1-20th of a grain of strychnia—on the fourth day 1-16th of a grain may be applied on a second blistered surface, the cuticle of course being removed. Five or six days afterwards this may be repeated if necessary. In a child of four years old, 1-8th of a grain may be used each time. If this fail, apply the actual cautery in three or four places at the junction of the skin with the mucous membrane. This may be repeated in a few weeks, if necessary, but less extensively. Strychnia may do for very mild cases, but the actual cautery is the most certain remedy. (Mr. A. Johnson, p. 156.)

Piles.—Give 2½ grains of blue pill every night, a scruple of extract of taraxacum

dissolved in water, three times a day, and an injection, with two grains of sulphate of iron to an ounce of water, to be used every morning. (Mr. Wormald, p. 311.)

The following ointment may be used when the piles are not of very large size. It is from the Pharmacopœia of Guy's Hospital. ℞. Gallarum contrit. ʒij.; opii (emolliti aquæ cum ʒj.) ʒss.; liq. plumbi diacet. ʒij.; adipis ℥j. M. ft. ung. If the piles be large, ulcerated, and painful, apply nitric acid. (p. 159.)

Bleeding from.—Inject daily a solution of sulphate of copper—fifty centigrammes in a glassful of water. (M. Trousseau, p. 159.)

Pyrosis.—Give gallic acid, unless the affection be accompanied by ulceration or organic malignant disease of the stomach or liver. (Dr. Bayes, p. 94.)

Rectum, *Stricture of the.*—May be from simple induration of the coats of the bowels, but it is far more frequently malignant; and although generally very slow in its progress, it always causes the death of the patient in the end. It exists in that part of the rectum where it makes its turn backwards, within two or three inches of the anus, and never beyond the reach of the finger, though it is often supposed to be situated higher, but this is owing to the practitioner mistaking the promontory of the sacrum for a stricture when the bougie is arrested in its progress by coming in contact with it. (Prof. Syme, p. 307.)

Sarcinæ Ventriculi.—Chloride of calcium is perhaps a better remedy to restrain the vomiting than hyposulphite of soda. Give the following draught: ℞. Liquoris calcii chloridi ʒj.; aquæ ʒvij.; sumat haustum talem ter quotidie. (Dr. A. Leared, p. 92.)

Tape-worm.—Give ʒij. of the etherial extract of the male shield fern in the evening, and follow this in the morning by ℥j. of castor oil. If necessary give another scruple of the extract the next evening, and another dose of castor oil the following morning. These doses may be repeated according to circumstances. (Dr. J. Hughes Bennett, p. 94.)

AFFECTIONS OF THE URINARY ORGANS.

Bladder, *Apparatus for Injecting.* Adapt a short bit of india-rubber tubing to the end of the catheter before its introduction. After introducing it, the conical nozzle of a common ear-syringe is easily admitted into the other end of the elastic tubing, and the thing is ready for use. This keeps the catheter more steady. It prevents *joggling*. It saves the use of a stopcock, as the tube is compressible, and lastly, it connects any sized catheter with the syringe. (Mr. Wormald, p. 168.)

Diabetes. You cannot restrict patients exclusively to animal food. Let animal food however predominate, and mix with it such vegetable matter as will not yield much sugar. Give succulent vegetables and well fermented beer. Insist on regular exercise and warm flannel clothing, with sponging and friction of the skin, always remembering that the skin and kidneys are vicarious of each other to a considerable degree. Dont give opium if you can avoid it, but mild mercurials occasionally; ox gall also may be useful, given so as to mix with the food as soon as it has been got out of the stomach. Creasote, half a minim, given at the time of taking food has been very useful. Give also strychnine, quinine, or cascarilla. (Dr. T. Hodgkin, p. 96.)

Address your remedies to the brain, instead of the stomach or kidneys. Apply blisters, cold lotions, and setons to the head and neck. Apply a continuous galvanic current from the nape of the neck to the epigastrium. Use Strongfellow's or Pulvermacher's portable electro-voltaic battery for this purpose. (Dr. Goolden, p. 100.)

Test for Sugar. Sugar in very small quantities may be detected by treating the fluid with trisacetate of lead, filtering, removing the lead with bicarbonate of soda or potash, and again filtering, by which means a colourless urine is produced, giving, on the addition of a few drops of concentrated solution of potash and heat, a *bright orange colour*. (Dr. A. B. Garrod, p. 277.)

Incontinence of Urine *in the Aged.* When not owing to diseased prostate, but probably to want of tone in the bladder, give the following: Tinct. ferri sesquichlor. ʒij.; balsami copaibæ ʒj.; strychniæ gr. j.; infusi quassiæ ℥xij. M. fiat mistura cujus sumatur ℥j. ter die. (Mr. J. Z. Lawrence, p. 168.)

In Children. Give cubebs; to an infant a few grains, and to a child half a teaspoonful, twice or thrice a day. Sometimes it has to be continued for some weeks. (Dr. Deiters, p. 102.)

Make a mixture of equal parts of Tinct. nucis vomicæ and tinct. ferri acet. Give from ten to sixteen drops twice every evening; or give one grain of tannin night and morning. (Dr. Blaschko, p. 102.)

LITHOTOMY. If a patient be relieved by making water, the case is probably one of urinary irritation. If the pain occurs during micturition only, there is probably stricture, but if the pain be slight *before* micturition, increases *during* the process, and becomes severe for a short time *afterwards*, we have probably a case of stone. The success of lithotomy depends in a great measure on the manner in which the prostate is divided. There is one part which cannot bear injury, this is at the base of the gland where it joins the base of the bladder; it is a dense texture, very tough and unyielding, forming a sort of ring around the urethra, endowed with an extraordinary degree of sensibility. Now, if this texture be torn, the patient will die, or if injured, the danger will be in proportion. The different success of different surgeons depends upon this ring being divided sufficiently by some and not by others: therefore, if on introducing your finger to dilate the deep part of the incision, you feel you have not cut enough, pass a straight probe-pointed bistoury along your finger, and, by a gentle sawing motion, carry the incision on to the extent you find necessary, in order to enable you to dilate with facility. (Prof. Syme, p. 295.)

Lithotomy Simplified.—Introduce a grooved staff in the usual manner, and confide it in the usual way to an assistant. Introduce the index finger of the left hand into the rectum, placing its extremity in contact with the staff as it occupies the prostate, and press it firmly against the staff so as to steady it; then, with a sharp-pointed straight knife pierce the perineum in the middle line, about half an inch above the anus, or at such a distance as may appear necessary to avoid dividing the fibres of the external sphincter. Carry the knife steady and firmly on, till it strikes the groove of the staff, the deep sphincter lying between the knife and the directing finger. Then move the point of the knife along the groove of the staff towards the bladder a few lines, and then withdraw it, cutting upwards, so as to leave an external incision of from three-quarters of an inch to one inch and a half, according to the presumed size of the stone. Next introduce a long ball-pointed probe or wire into the bladder, and withdraw the staff. Then grease the fore-finger of the left hand, and pass it with a semi-rotary motion along the probe, through the prostate into the bladder, when the stone will generally come into contact with it, and if not too large, passes at once into the wound on withdrawing the finger, the patient still having power to strain upon the stone and thus facilitate its expulsion. If the aperture through the prostate be too small, dilate it by means of Arnott's hydraulic dilator, or by adding to the finger india-rubber finger gloves, one over the other, so as to thicken the finger to the size required. (Mr. G. Allarton, p. 288.)

To Prevent Hemorrhage after.—There are many objections to plugging the wound. It prevents the flow of urine, which creates much irritation, and on its removal fresh bleeding is often excited. To obviate this, attach to the common tube a conical bag of oiled-silk, about an inch and a half from its inner extremity, pass the tube into the neck of the bladder, and then fill the oil-silk bag with sponge. The wound may in this way be most effectually plugged, and the escape of urine is secured by means of the tube. (Mr. J. Hilton, p. 298.)

RETENTION OF URINE.—In retention of urine from obstinate stricture, when the rectum is obliged to be punctured, the instrument sometimes slips out. A modification of Mr. Wakley's instrument is now used at Guy's Hospital. It expands by a spring when once in the bladder. (P. 167.)

SEMINAL EMISSIONS.—When occurring involuntarily at night, give three granules of digitaline, equal to about one grain of powdered digitalis every night. (M. Laroche, p. 167.)

STRICTURE OF THE URETHRA.—Stricture never occurs in the membranous portion of the urethra. If you divide the urethra into three portions, the prostatic, membranous, and the remaining part which is covered by the corpus spongiosum, it is in the last portion that all strictures exist. It is never necessary therefore to cut beyond the bulbous part of the urethra, as no strictures are beyond the bulb. If you bear these facts in mind, you need not dilate sinuses, which are sure to close

when the stricture is removed. A catheter should be introduced into the bladder after the operation, and retained for 48 hours—not less, on account of the risk of extravasation of urine, and not longer, because it is unnecessary and apt to do harm. (Prof. Syme, p. 160.)

Mr. Syme's set of bougies are exceedingly small in size. No. 1 is perhaps smaller than those made anywhere else. The tightest and worst strictures are *anterior to the scrotum*. A stricture in this place is felt like a small pea, somewhat elongated. This is the kind most unmanageable by dilatation, and most successfully and easily treated by division. Pass a very small director (which is here comparatively easily done, but would be very difficult at the *bulb*). Introduce the point of the knife into the groove of the director *anterior to the stricture*, and push it backwards through it, while the end of the penis is held firmly. Cut *backwards* in stricture anterior to the scrotum, and *forwards* when the stricture is at the *bulb*. You will never meet with a tight stricture *behind the scrotum* along with an extreme contraction in *front* of it. (Prof. Syme, p. 162.)

A practical distinction of the utmost importance arises from the question whether the stricture is *anterior* or *posterior* to the membranous portion of the urethra. The deep perineal fascia lies in the immediate neighbourhood of the membranous portion, and a wound of this fascia, in opening the urethra, would very likely end in urinary infiltration. When, therefore, a stricture is situated in the bulb of the urethra, it may be divided from without, and any urine which escapes is sure to pass out at the external wound. But if the knife passes along the grooved director or sound, and perforates the deep perineal fascia, and wounds the urethra as it passes through this part, the urine which escapes may lodge in the wound made in this fascia, and a drop or two infiltrated behind this dense structure may cause violent inflammation and constitutional disturbance. Hence there are two classes of stricture—those in which it is situated in that part of the urethra corresponding to the corpus spongiosum—and those in which it is situated at the anterior part of the membranous portion of the urethra. In the former, you may divide it with impunity; in the latter, there is great danger of causing infiltration. (Mr. Henry Lee, p. 165.)

Testicle, *Enlarged*.—When compression is wanted, use Hutchinson's air compressor. A kind of double nightcap, made of impermeable material, such as Macintosh, or oil-sick, is folded over the testicle, and then, by means of a stop-cock and force-pump, air is forced into the bag, by which almost any degree of compression can be produced. Some forms of hydrocele may be treated in this way. (Mr. Hutchinson, p. 169.)

AFFECTIONS OF THE SKIN, &c.

Acne Rosacea.—Smear the following over the face with the finger every night; camphor, a drachm; milk of sulphur, twice as much, or the sulphur sublimatum. Afterwards as much water as will render it sufficiently liquid. The camphor must be powdered by the usual addition of a little spirits of wine. (Mr. E. Wilson, p. 195.)

Apply a minute drop of the solution of the nitrate of mercury in strong nitric acid, by means of a finely-pointed glass brush, on the top of the tubercle, if indolent. (Mr. J. Startin, p. 240.)

Carbuncle and Boils.—*General Treatment*.—Give purgatives and alteratives, with stimulants or salines according to the constitutional disturbance. Incisions give great relief, especially when the carbuncle is spreading and painful; but if the pain has subsided, incisions are not required, but are even injurious. (P. 187.)

Apply a solution of the nitrate of mercury in strong nitric acid (hydrarg. ℥j.; acidi nitric., sp. g. 1·50, ℥ij.; solve) to one central spot about the size of a shilling or sixpence. It produces an eschar from beneath which the slough separates. For *boils*, the caustic answers even better than the knife; apply a full-sized drop to the apex of the boil. (Mr. J. Startin, pp. 188, 240.)

Erysipelas.—Apply the protosulphate of iron ℥viiss. to ℥xxiv. of water, or 8 parts to 30 of lard. Apply either of these three times a-day to the part, and to a little distance beyond the disease. (M. Velpeau, p. 195.)

M. Debout recommends the following formula. *Ointment*:—Sulphate of iron 5

to 10 parts, water 12½ to 25 parts, oil, 12½ to 25 parts, lard 70 to 40 parts. *Solution*:—Sulphate of iron 10 to 20 or 40 parts, water, 120 to 110 or 90 parts, glycerine 70 parts. (P. 195.)

NÆVUS.—If a nævus be small, a simple ligature will be sufficient; if large, *tie the knot over a piece of bougie or other substance*, which can be twisted each day, until the thread cuts its way through the base of the tumour. (Mr. S. Wells, p. 313.)

In moles and nævi of the face, if superficial, apply a solution of the nitrate of mercury in strong nitric acid. (Mr. J. Startin, p. 241.)

PLASTIC SURGERY, *Hints on*.—A transplanted flap of skin will not unite readily to the denuded edges of a defective part, such as an old cicatrix. Even if it did unite, it would become raised and thickened. The whole of the epidermoid surface of the old cicatrix should, therefore, be pared away, so that two raw surfaces may be brought into contact. Before applying the transplanted flap, the pared edges of the defective part should be loosened from their attachments to the extent of two or three lines, by *flat strokes* of the knife. If this be not done, it is probable that the two edges will not be on the same level, the transplanted part will be raised. (Mr. Spencer Wells, p. 181.)

PSORIASIS DIFFUSA.—When in large patches over the flexures of joints, forehead, neck, chest, &c., give a quarter of a pint daily of the decoction of the urtica dioica (common stinging nettle), made by boiling an ounce of the leaves and stems in a quart of water, down to a pint. An extract may be made from this—dose, five grains three times a day. The same medicine is efficacious in inveterate chronic papular eruptions (lichen), and in various forms of vesicular and scaly diseases (eczema, lepra, and psoriasis), especially in cachectic states of the system. (Dr. J. Bullar, p. 172.)

SCABIES, *and other Parasitical Diseases of the Human Skin*.—The vapour of benzoin, or benzole, which is prepared by the decomposition of benzoic acid, destroys parasites more surely than almost any other application. It may therefore be used in pityriasis and scabies most effectually. (M. Reynal, p. 196.)

SORES AND WOUNDS, *Sloughing*.—Use chlorine. It is a powerful antiseptic and prevents sloughing. Make a solution of chloride of lime, two drachms of the powder to a pint of cold water. Cover the wound with pads of tow dipped in this solution, and renew them about twice a day. (Mr. T. S. Fletcher, p. 191.)

ULCER OF THE LEG *in Elderly People*.—Opium has a wonderful effect as a stimulant in these cases. Give good diet, and a common opium and soap pill at bed-time. Assist by strapping and bandaging if necessary. The soap and opium pill may be given once, twice, or three times a day if necessary, and if other symptoms do not forbid its exhibition. Sometimes the soap and opium purges: in this case leave off the soap, and give half a grain of extract of opium night and morning, or once a day; increase to one grain if necessary. Opium gives energy to the capillary system of arteries, promotes warmth, and thus secures an equable balance of the circulation. (Mr. F. C. Skey, p. 191.)

WARTS.—When these are about the anus and other parts, sprinkle a little calomel over them every two or three days. In children, use the grey powder, and for distant patients whom you seldom see, use the following: Hydrarg. chlorid. gr. viij.; zinci oxyd. gr. viij; mucilagin. 3 ij.; aquæ 3 vj.; Ft. lotio. (Mr. Lloyd, Mr. Wormald, p. 196.)

WHITLOW, *Deep-Seated*.—This being an inflammation of the inside of the sheath of the flexor tendons of the fingers, rapidly running into suppuration, make an early, free, and deep incision into the sheath. If you are obliged to do this in the palm of the hand, you may avoid the palmar arch of blood-vessels by attending to the directions given at page 184. (Mr. J. Hamilton, p. 184.)

WOUNDS, *Incised*.—These often will not unite by first intention, on account of the oozing of blood on their surfaces. This has been remedied by deferring the dressing for a few hours—but it may be accomplished by *pressure* on the *sides* of the wound by means of dossils of lint, taking care to leave the edges free. Don't disturb the dressing for three or four days. When you cannot apply dossils of lint on account of the irregularity of surface, apply a large sponge.' (Prof. Syme, p. 282.)

AFFECTIONS OF THE BONES AND JOINTS.

AMPUTATION *of the Thigh.*—In the upper part of the thigh the flap operation answers well, but at the lower third the circular, or a modification of it, is preferable. Mr. Syme often modifies the circular operation as follows: form two short flaps of skin by semilunar incisions on the anterior and posterior surfaces—then retract the skin so as to expose two inches of the muscles *above the angle of union of the flaps*, and divide the muscles in front as high as they are exposed, and those behind as low as they are exposed, to compensate for their greater contraction. Take care to retract the muscles an inch and a half or two inches before sawing the bone, as it is not the length of the flaps that prevents the future protrusion of the bone, but *the height at which the bone is divided above the angle of union of the flaps.* (Prof. Syme, p. 119.)

Of the Leg.—Do this about an inch and a half below the tuberosity of the tibia. Take the head of the fibula or the tuberosity of the tibia as your guide, and make a short anterior and posterior flap of integument only, entering your knife about a hand's breadth below the bony prominence at the point which is to form the angle of junction of the flaps at one side. Having dissected up the integuments to an inch and a half below the tuberosity of the tibia, divide the muscles behind about an inch lower down, to allow for their retraction, and then saw through the bones as high as the skin has been dissected up. The nicest kind of artificial leg is made of *tin* with a *wooden* pin—something like an inverted quart-bottle without a bottom. (Prof. Syme, p. 122.)

At the Ankle.—Make a transverse incision across the sole of the foot, from the tip of the external malleolus, or a little posterior to it (rather nearer the posterior than the anterior margin of the bone) to the opposite point on the inner side, which will be rather below the tip of the internal malleolus. If the incision be carried further forward, considerable inconvenience is experienced from the greater depth of the flap, the operator, getting into the hollow of the os calcis, cuts and haggles in striving to clear the prominence of the bone. Another incision is then to be carried across the instep joining the ends of the former. In separating the flap of skin from the os calcis, you must cut *parallel to the bone.* This is of the greatest consequence, as the flap is supplied with blood only by those vessels which run through it *parallel to the surface*—consequently if you are not careful you divide these vessels, and deprive the flap of nourishment. In dressing the wound afterwards *be careful to avoid all pressure*, as this will kill the flap. In this operation don't leave the astragalus, but take it away, as it is very likely to be involved in the disease, and even if not carious then it is very disposed to be so, and you had better remove *all* the bones which are liable to caries, not excepting the os calcis. (Prof. Syme, p. 119.)

DEFORMED BONES.—In cases of deformity of the bones, whether from accident or disease, perform what is called subcutaneous osteotomy, which is a much less dangerous operation than resection of bone exposed by a large external wound. Make an incision rectangular with the longitudinal axis of the bone, dividing the integuments and periosteum—apply a fluted chisel-drill (two lines in breadth, attached to a centre-bit) in the centre of the incision, and perforate the bone (the tibia, for example,) transversely from within, outwards. The cessation of resistance safely denotes the accomplishment of the perforation—a narrow saw is now introduced into the aperture drilled through the bone, and the greater part of the bone divided. The bone is directly straightened by fracture of the remainder. The normal position need not be restored at once, but can be accomplished during the consolidation of the bone. (M. Langenbeck, p. 126.)

EXCISION *of the Elbow-Joint.*—Always remember that in caries of the joint, little more than the articular surface is engaged in the disease. It is the ignorance or neglect of this point which has made many cases unsuccessful. The incisions originally practised by Moreau are the best, viz.: a transverse one across the back of the articulation, immediately above the olecranon, from the ulnar nerve to the external condyle, and two longitudinal through the extremities of the transverse, so that the incision altogether has the form of the letter H. When the flaps have been raised, free access is gained to the articulation: and when the operation is

completed the edges of the transverse incision, if brought accurately together, generally adhere by first intention. Having dissected up the flaps, do not attempt to remove all the articulation at once, as otherwise there will be a risk of cutting the ulnar nerve; but having exposed the olecranon, cut it off in the first instance, so, as to get free access to the joint; then divide the external lateral ligament, and having pushed the ulnar nerve over the inner condyle, free the end of the humerus and saw it off on a line with the tuberosities; and lastly, remove in succession the ends of the radius and ulna on a line with the base of the coronoid process. More than the extent thus defined would be unnecessary and injurious, while less would hardly remove the disease, and even if it did, would incur the risk of anchylosis. (Prof. Syme, p. 106.)

When this joint has been anchylosed in a wrong position, so as to be useless, a certain degree of motion may be secured by the following operation: cut out a large portion of the end of the ulna, radius, and humerus, making, in fact, a resection of the anchylosed joint. (Prof. Fergusson, p. 108.)

Of the Knee-Joint.—Take care to clear away from the face of the flap the whole of the synovial membrane. Be careful of the direction of the saw as you saw through the femur and tibia—take care that you cut through these bones *parallel to each other*, else their surfaces will not meet properly. The line of section in the femur should be close to the edge of the articulating cartilage of the inner condyle, which will remove all the articular surface likely to be implicated in disease, and still leave a breadth of bony surface about equal to the cut surface of the tibia. The patella must be left. It is indispensable. (Dr. Wm. Keith, p. 108.)

Fractures.—All kinds of fractures of the leg, excepting compound, may be treated with the starched apparatus. Apply a dry bandage next the skin, then two pasteboard (side) splints made perfectly soft by soaking in hot water, and moulded carefully to the shape of the limb, and lastly, a bandage which has been tightly rolled in a thick solution of starch. This becomes quite hard and dry in about twelve hours, and the following day the patient may walk about on crutches, with his leg suspended in a sling from his neck. The same application may be used in fracture of the humerus. In fracture of the femur, the long splint must be kept on for a month before applying the starched apparatus, and the long splint (without the pad) for a day or two afterwards, until the apparatus has become thoroughly dry and firm: but don't include the knee-joint, as this may become stiff. In oblique fractures of the leg, use Mc Intyre's splint for ten days or a fortnight before using the starch. (Mr. C. Royston, p. 125.)

Knee-Joint.—In operations for diseases of the knee-joint, *save the patella, if possible*, with its ligament. (Mr. G. Jones, p. 105.)

Tumour of the Jaw.—Diagnosis is often obscure in these cases. Puncturing these tumours may enable us to diagnose them. In fibrous tumours there is resistance to the *lateral* movement of the instrument—you cannot move it about—the tumour is too hard. Examine the teeth, gums, and bones of the part: in medullary sarcoma the bones are soft and degenerated, the palate feels soft and elastic to pressure, the teeth are irregular, loose, and projecting. (Mr. H. Hancock, p. 139.)

VENEREAL AFFECTIONS.

Bubo.—To prevent a bubo from suppurating, employ counter-irritation by means of a strong solution of nitrate of silver, three drachms to the ounce of distilled water, with twenty minims of strong nitric acid. It may be applied with a glass rod or stick, or a glass brush. The black eschar will peel off in a few days, and then it should be reapplied. (Mr. H. Thompson. p. 229.)

Gonorrhœa, *Painful Erections in.*—Give lupulin; begin with one scruple and increase the dose if necessary. Zambaco gives this dose and increases it to sixteen scruples, but we should hesitate how we commenced with stronger doses than five or ten grains. (p. 241.)

Syphilis.—It is stated that a *neutralizing liquid* has been discovered for this poison, composed as follows:—Distilled water 3 ixss.; perchloride of iron, citric acid, hydrochloric acid, of each 3j. Let a drop of this fall upon the part inoculated, and allow

it to remain fifteen minutes, or apply a bit of lint soaked in the fluid. If the lint be applied for an hour the antidote is complete. (M. Rodet, p. 237.)

AFFECTIONS OF THE EYE.

Fistula Lachrymalis.—Instead of passing the knife into and along the lachrymal canal, make a simple puncture into the abscess or lachrymal sac, and then, introducing the style, guide it to the orifice of the canal, and with gentle pressure pass it along the tube. (Mr. E. R. Bickersteth, p. 198)

Squinting.—In this operation take care to know, and to tell the patient beforehand, whether one or two operations will be necessary. "If you find the vision of both eyes good, but the degree of convergence considerable, in all probability two operations will be necessary." If with this convergence you have unequal visual power, one operation will generally be sufficient. (Mr. S. Wells, p. 196.)

Tarsal or Meibomian Tumour.—This is an encysted growth, and therefore, the cyst must be taken out, or injured by laceration, so that adhesive inflammation may follow. This has generally been done from the inside of the lid. Mr. Walton does it from the outside. He divides the cyst outside the lid, squeezes out the contents, and then, if possible, *pulls away* the cyst with a pair of forceps, or if he fail in this, he dissects it out. The external incision being made horizontally, there need be no fear of a scar. (Mr. Walton, p. 198.)

MIDWIFERY AND THE DISEASES OF WOMEN.

Fibrous Tumours *of the Uterus*.—Order a long course of iodine, the following is a good form: ℞. Potassii iodidi gr. i; syrupi ferri iodidi ℥ xx.; aquæ carui ʒ ss. M ter die sumend. (Dr West, p. 311.)

Give the liq. calcis chlor. from 30 to 50 minims, twice a day for months, or give the Kreuznach-water (artificially prepared by Mr. Hooper) ʒj three times a day in warm water, at the same time using an injection for the vagina of ʒij to the ʒj of water. Dr. Preiger, however, chiefly depends on baths and fomentations, composed of the Kreuznach-water, and some extraordinary effects have no doubt been produced by him, as we have seen. (Dr. E. Rigby, p. 212.)

Flooding *after Delivery*.—Inject cold water into the womb. Use a Weiss's syringe, and introduce the nozzle of the tube an inch or two fairly into the os uteri. (Dr. S. Wray, p. 208.)

Ovarian Dropsy.—First empty the cyst, then introduce a long flexible catheter, and inject a few ounces of the tincture of iodine of the Edinburgh Pharmacopœia, which is double the strength of the English. Close the wound by strapping, and apply pads and bandages. The cases adapted to this operation are the simple, single ovarian cysts. (Dr. J. Y. Simpson, Mr. I. B. Brown, p. 216.)

Polypus Uteri.—Remove large uterine polypi by incision, rather than by ligature. You can always arrest hemorrhage by well plugging the vagina. For excision use a polypus-knife (called a *polyptome*). First reach the peduncle by the apex of the fore-finger of the right hand introduced along the pubic surface of the vagina. Then push the instrument along this finger, and by hooking down the peduncle it is divided by the knife, which is placed in the concavity of the instrument, something like the lately improved hernia knife. (Dr. J. Y. Simpson, p. 210.)

Tedious Labour.—The tepid douche, used in the way adopted to induce premature labour, may sometimes stimulate the uterus when inactive. You may use the douche for ten minutes or a quarter of an hour at a time. The same means may assist in dilating a rigid os uteri. (Dr. Bourgeois, p. 207.)

MISCELLANEA.

Charcoal.—This body should be perfectly dry before being used. If moist or wet,

place it before the fire, and gradually dry it. By doing so it may be used over and over again. Charcoal has a most surprising power of *rapidly absorbing sloughs*. It hastens decomposition. In infectious diseases, bolsters and mattresses may be made of it. It completely destroys smells of all kinds even when placed near their source. (Mr. L. Ormerod, p. 255.)

As a Disinfectant.—As the most powerful disinfectant, it may be used in various ways. It should be perfectly dry. Heat it thoroughly in a covered crucible, with a small hole in its lid to allow any oxidised material which it may contain to escape, taking care not to have the hole sufficiently large to allow the charcoal to undergo combustion. When thoroughly heated allow it to cool. Next place it in shallow vessels where wanted to deodorise or disinfect. It will lose its powers in a few days, and ought then to be heated again in the same way. (Mr. G. J. Barford, p. 258.)

CHLOROFORM.—This may be given in various diseases and in larger doses than has generally been done. For example: *In painter's colic*, give half a drachm every two or three hours; increase this to a drachm if necessary. *In delirium tremens*, give at first one drachm increased to two drachms. Combined with opium it may prove still more valuable, but don't give it by *inhalation*. You may give large doses by the stomach, but not by inhalation, in these cases. *In neuralgia*, a liniment composed of equal parts of chloroform and olive oil will act most beneficially. (Dr. Corrigan, p. 260.)

Don't be guided by the circulation, but by the *respiration*, in your exhibition of this remedy. Take a folded towel or handkerchief, presenting a pretty large surface, instead of a small bit of lint, or any other apparatus held to the nose, and don't stint the quantity, but get the patient under its influence as rapidly as you can. The moment the breathing is stertorous, cease the administration. Attend also carefully to the *tongue*, open the mouth if the respiration is difficult, seize the tip of the tongue with the artery forceps, and *pull it well forward*. Always give chloroform in the horizontal posture. (Mr. Syme, p. 262.)

COD-LIVER OIL, *Test for.*—Sulphuric acid appears to be a perfectly reliable test of the excellency of cod-liver oil. The *lilac-hue* produced by this acid is different from all other colours. Place a small quantity of the oil to be examined on a white tile, or clean writing paper; drop two or three drops of sulphuric acid into the centre, and instantly a number of radiating currents of a delicate and beautiful lilac hue are seen. The goodness of the oil is in proportion to the richness and persistency of this colour, and the rapidity with which it is produced. The colour produced in all other oils, as in seal oil, train oil, sperm oil, and vegetable oils, is entirely free from a purple or violet tinge. (Dr. E. H. Greenhow, p. 88.)

ELATERIUM.—In giving elaterium, we all know the great sickness which it sometimes causes. *Capsicum* given with it will often enable us to continue its use much longer than we otherwise should be able to do. Give the capsicum in half or one grain doses. (p. 307.)

NEW FORCEPS *for Extraction of Bullets, &c.*—These consist in the blades being separate, so that one blade at a time can be introduced into a sinus or cavity, and when the object is secured the handles can be locked like the midwifery forceps. (p. 312.)

NITRATE OF SILVER. Instead of dissolving this salt in water, dissolve it in common nitric ether. The ether acts as a solvent of greasy matter on the skin, and from its volatility, quickly dries, enabling us to apply several coatings of the solution in a short time. The strength may vary, but about eight grains to the ounce will generally be sufficient. In erysipelas, this will be found an improvement. (Mr. Ward, p. 236.)

POISONING *by Lead, Mercury, Gold, Silver, or any other metal.*—Place the patient on a wooden bench, in a metallic bathing-tub, isolated from the ground, and filled with water up to his neck. The water is made slightly acid with nitric or hydrochloric acid, if mercury, silver, or gold is in the system; and with sulphuric acid, if lead is suspected. The negative pole of a pile is then brought into contact with the sides of the bathing-tub, and the positive pole placed in the hands of the patient. The work of purification is now in full activity. The electrical current precipitates itself through the body of the sufferer, penetrates into the depth of his bones, pursues in all the tissues every particle of metal, seizes it, restores its primitive form,

and chasing it out of the organism, deposits it on the sides of the tub, where it becomes apparent to the naked eye. (MM. Poey and Vergnés, p. 227.)

By Strychnia.—Animal charcoal is a very general antidote in cases of poisoning; it may be conveyed into the stomach in considerable doses mixed with water, by means of the stomach-pump; it combines with the poisonous principles and renders them inert, or, in some cases it may preserve life by enveloping and isolating the poisonous particles, preventing them coming into contact with the absorbents. In experiments on animals, when strychnia was previously mixed with animal charcoal, they were not in the slightest degree affected. It is equally efficacious as an antidote for all the vegetable and animal poisons, as well as for hydrocyanic acid, and in many instances is more successful than the usual remedies employed in poisoning by mineral substances, especially corrosive sublimate and arsenic. (Mr. W. Chippendale, p. 303.)

Polypus of the Nose.—When you are going to remove a polypus, remember that non-malignant diseases of this description are generally very limited in the seat of their origin from within the nose. They never proceed from the *floor*, *septum*, or *external wall*, but *always from the roof*, or that part which is formed by the turbinated plates of the ethmoid bone, so that the growth always *descends* and fills up the lower parts of the nose, even extending into the throat. Never, therefore, attempt to extract it *low down*, but *high up*. Select forceps of a very small size; introduce the instrument gently along the upper passage of the nose, with the blades separated as much as possible, till you come to the neck of the polypus—then compress the handles, and by a combination of traction and torsion in one uniform direction, break through the connecting textures. If the growth does not follow the instrument, try again and again. (Mr. Syme, p. 223.)

Salivation.—In excessive salivation, or when you wish to put a stop to it, give from 3ss to 3j of chlorate of potass per diem. This may also be given in the same dose in cases of ulcero-membranous stomatitis, or in mercurial stomatitis. (MM. Herpin and Blache, p. 235.)

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